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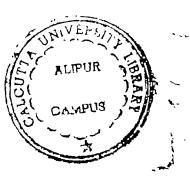
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Erratum:

In his Presidential Address, "The Competence of Political Science: 'Progress in Political Resarch' Revisited," which appeared in the March 2000 issue of the Review, Matthew Holden discussed the article by Frank R. Baumgartner and Beth L. Leech, "The Multiple Ambiguities of 'Counteractive Lobbying,' "American Journal of Political Science 40 (May 1996): 521-42, which is a critique of David Austen-Smith and John R. Wright, "Counteractive Lobbying," American Journal of Political Science 38 (February 1994): 25-44. In his footnote 20, page 10, Holden stated that "using J-STOR, I am surprised not to have found a response by the authors they critiqued." Austen-Smith and Wright did reply to the Baumgartner and Leech critique in their article "Theory and Evidence for Counteractive Lobbying," American Journal of Political Science 40 (May 1996): 543-64. Baumgartner and Leech then responded to the Austen-Smith and Wright reply in their article, "Good Theories Deserve Good Data," American Journal of Political Science 40 (May 1996): 565-9. On the first page of this last article, the editor of the AJPS noted, "To provide closure on the debate, Professors Austen-Smith and Wright were not asked to respond to this manuscript.

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The Competence of Political Science: "Progress in Political Research" Revisited Presidential Address, American Political Science Association, 1999

MATTHEW HOLDEN, JR. University of Virginia

Political science is two realms, the intellectual and the organizational, and the task is to consider how the organizational realm might be adapted to the highest improvement of the intellectual realm. Political science has a certain competence (domain) in the study of politics as the organization of power. It also seeks to expand competence as capability. Charles Merriam provides a point of departure. Merriam's most successful idea has been that of enhancing competence through improvements in "the field of method." Competence, however, now demands methodological flexibility, so as to probe more into the exercise of power. Four fields are strategic: public administration, political interests, urbanization, and the interpenetration of politics and economics. Competence also leads into unorthodox subjects, such as force and foolish, irrational, and pathological decision making (or "the Oxenstierna-Mullins Effect"). Finally, competence demands (and is enhanced by) the reach of political science into serious practical problems of human affairs.

n framing this address I thought about my longtime friend, Aaron Wildavsky, who was, as Paul Sniderman said recently,1 consciously or otherwise an advocate for political science. Once, he disagreed with something I said, and his query came back: "But what shall we teach the young?" He understood the folly of recruiting and helping to educate human beings and at the same time pummeling them into conscious disdain for what they do. In that sense, I take on an advocate's role self-consciously. The young scholar learns to think about where the discipline is going, and this is important. But it is so much more important to think where the discipline ought to go and to rise above pettiness in so doing. This is about the calling, or about the purpose to be served by political science and by one's work in political science.

The calling—obligation or even duty, to use a very old-fashioned word—is to enhance the competence of political science in the interest of human understanding. This depends, among other things, on ideas to guide research and on strategic conceptions about what research is worth the efforts of grown men and women. In political inquiry, human beings still see, as St. Paul said, through a glass darkly. The task that justifies the allocation of social resources to support a political science is the task of seeking knowledge that would make it possible to see through the glass a little more clearly. Thomas Hobbes ([1651] 1991) starts with two crucial issues: how to create political order and civilized life and how to discover the ways of doing so. Hobbes lived in a world of extraordinary turbulence and saw the vast ignorance with which people sought to meet that turbulence. It led him to theoretical extremes which give him the bad reputation that makes him unwelcome today. "The skill of making and maintaining commonwealths ... consisteth," he wrote, "in certain rules... which neither poor men have had the leisure, nor men that have had the leisure, have hitherto had the curiosity or the method to find out" (p. 145).

The search for the rules of politics, which will explain what is involved in the making and maintaining of commonwealths, is the calling. The world of political science is two realms, intellectual and organizational. The intellectual realm is contained in the theories, hypotheses, doctrines, facts, and so forth, that are embodied in our study and our teaching about the parameters of politics (culture, economics, climate, demography, etc.), outer limits of which are set in the extreme case by human heredity and nonhuman environment; the institutions and actual processes of action in politics (adjudication, legislation, administration, elections, coalition formation, factions, committees, wars, agitation, resistance, persuasion, etc.); and the product of politics (policies, programs as decisions subordinate to policies, operational decisions as subordinate to programs, etc.).

Political science is also an organizational realm of departments, schools, journals, editors, applications, review committees, and so on. This organizational realm is a human network with something akin to its own largely unwritten "constitution." It has a set of more or less definitive understandings as to who is a member (full, partial, or potential), who is an acceptable outsider, with some legitimate presence, and who is entirely excluded. These understandings extend to posts of honor, eligibility for them, and accession to them. They extend to decision process rules, rules as to substantive outputs (which are mandatory, which permissible, and which impermissible), and some understandings as to rules for changing the rules.

These two realms shape each other, although I think the intellectual realm is largely the expression of what people in the organizational realm do. In the organizational realm, however, the purpose of what we teach is to enable young scholars to enter, and entering, to make a contribution in the intellectual realm. Career is

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¹ Private conversation, August 1999.

mere necessity, but calling is far more important than career.

THREE LEVELS OF KNOWLEDGE

The young scholar needs to learn to understand at three levels. First, he or she must acquire what is taught expressly. That is the first level. Second, the young scholar should come to understand what lies beneath what he or she is being taught. What were the assumptions, facts, methods, and subtleties that shaped what the teacher understood clearly, but lacked time, incentive, or sometimes means expressly to communicate? Third, he or she must understand what lies beneath the beneath. What was the intellectual history and atmosphere that affected the teachers' teachers? Deep grounding in the literature is a necessity.

On such a premise, I revisit the presidential address of Charles E. Merriam (1926), entitled "Progress in Political Research." I will not go very far into the overall career of Merriam, although the temptation is great. His relationships to his students, Chicago politics and politicians, the national government under Hoover and Roosevelt, foundations, the political science and social science communities, and the University of Chicago's president, Robert Maynard Hutchins, all are worth the kind of treatment that Robert Skidelsky (1994) gave to John Maynard Keynes (see also Karl 1974).

My choice is to describe Merriam's presidential address, what political scientists have taken from it, what elements remain to be developed, and what are its implications at the end of one century and the beginning of another. When I was a graduate student, behavioralism was on its upswing. In that time of intense exchange between "behavioralists" and "traditionalists," Merriam was the scholar who seemed to have anticipated it all, but with a firm grasp of hard political realities. His expressed approach, more than that of any other scholar, provided the scope that I found helpful in going beneath the beneath. "We do not," he said, "teach all that we know, and are driven sometimes to teach what we are not so sure of." The moral is: "Take a chance intellectually, because you do not have to know that what you are proposing is true before you begin to investigate it." He also said that the existing conventional categories are by no means the only ones for studying important matters relating to power. We might, he said, "have studies of the use of force in political situations, and its opposites, passive resistance and noncooperation; ... the nature of political interests; ... use of magic, superstition, and ceremonialism in politics; ... propaganda; ... the actual process used in conference, so significant a function in modern affairs; ... the maintenance of political morale; ... leadership, obedience, cooperation; ... the causes of war as well as its diplomatic history and law" (Merriam 1926, 6). Merriam in 1925 thus issued what was a veritable charter for the more adventurous program that has characterized behavioral political science at its most expansive.

I adapt the Merriam themes by including in my title

"the competence of political science." Competence means the capacity to do something. It means what one possesses, as in a Victorian novel a young person with some trust income is described as having "a competence." It also means the domain in which one has the right to act, as in the term a "competent" court.² To claim competence is not to say there is no need for self-improvement. Of course there is a need for self-improvement.

In the culture of political science there is sometimes a certain anxiety. Young scholars learn to justify arguments with the phrase "political science has neglected." The phrase implies intellectual or moral negligence, as if people knew what they should have done but did not do it. But this bespeaks an emotional not an intellectual state. It is an inheritance from years ago, certainly after World War II. Political scientists learned a certain inverted Pharisaism: "God knows, I am not as good as other scientists are." It would surprise me not the least if similar views were entertained by many deans, provosts, and presidents across the country. The term "self-hatred" has come, from psychoanalysis, into discussions of group behavior. It is used to describe a member of a group who treats the group with disesteem and seeks acceptance into some other group of higher standing. Under such a condition, it would also not be surprising if some members of the discipline would wish to assimilate to disciplines of higher status (to be taken by economists for economists) and look down at other colleagues who do not have the same wish or capacity. (Sometimes we compensate by seeking others we can snub, although in the end that is counterproductive.3) It is hardly surprising that there is a need for a good infusion of self-respect and selfconfidence, and along with self-confidence a degree of intellectual respect that allows differences to be examined with some precision and clarity.

IN BEHALF OF OURSELVES: WE KNOW MORE ABOUT POLITICS THAN DO OTHER SCHOLARS

Of course, our discipline is greatly imperfect. What discipline, profession, or human activity is not? Law? Medicine? Theology? Psychiatry? Economics? True, some other disciplines are logically more coherent. They have a dominant set of assumptions, theories, methods, and data, although none of the social sciences approach the coherence of physics, chemistry, or biology. Some are equipped with better mathematicians. Someday we may get closer to the standards in those fields. But, as far as we are, we know more politics than

² See Garner 1999: "competence, n...2. The capacity of an official body to do something, 'the court's competence to enter a valid judgment.'"

³ One of my graduate school contemporaries, a famous political scientist I will not name, said that at a certain stage of his life, he and his friends used to stand near the education faculty as they marched into commencement and conspicuously call them "mister." If this is still our attitude, it can only obstruct our need to infuse what we know from political science into primary and secondary education.

any other discipline. It is our business to know politics, however imprecisely we state it.

There is, I contend, one common thread in politics: the organization of power. The concepts, techniques, or data of international politics, comparative politics, American politics, and so forth, are translatable each into the language of the other. Power is the key to political practice and, accordingly, to the study of that practice. The competence (domain) of political science is the study of the parameters, processes, and products of power. Its competence (intellectual capability) also extends to wherever human beings organize power, whether it is called officially "public" or "private." This is the point of view that I attribute to Charles Merriam, and I now turn to an excursus on method and then to some big issues within conventional fields where there are significant opportunities.

MERRIAM'S FORMULATION OF PROGRESS AND THE TRIUMPH OF METHOD

When Merriam (1926) chose as his subject "Progress in Political Research," the American Political Science Association was twenty-one years old. Merriam concluded that "these twenty-one years have been a period of substantial progress and solid achievement, more than justifying the expectations of those who aided in launching the Association" (p. 4). In 1999, we should recall that the original objectives were by no means theoretical only. At first, there was a proposal to create the American Society of Comparative Legislation (Dodd 1904, 6). The originators soon concluded that it would be better to have a broader society "to embrace the whole field of Political Science, and thus embrace Comparative Legislation as one of its topics" (p. 6). Comparative legislation was a practical subject, "including the operation of statutes, uniform legislation, and the promotion of publications, especially indexes and digests of record material" (p. 11).

Merriam offered a summary of where he thought there had been progress and stated an agenda. The research agenda includes both what we now regard as conventional and what he expressly described as "unorthodox." He also discussed opportunities for extending political science beyond the existing academic universe. I shall discuss these in turn to show how they are pertinent to the competence of political science at the end of this century.

More specifically, Merriam saw progress in certain fields of political science and in "the field of method." He mentions the following areas: parties (Merriam 1926, 2–3), political theory (p. 10), international relations and international law, constitutional law, public administration (including administrative law and organization), legislation, and problems of the modern city (p. 1). Indeed, he said, "it would be interesting to examine the fact-content of these various inquiries and to develop the various principles and conclusions that have been established, but the limits of this discussion will not permit such an appraisal" (p. 3).

These are now conventional (standard) fields of research and teaching. The major additions since Mer-

riam's time are comparative politics and methods. The physical and intellectual growth of the discipline has occurred primarily through specialization within all these general fields. There is still considerable scope for intellectual progress. So much has happened in 74 years, and so much remains. I return to this below.

It is in "the field of method" that the most change has occurred. The political scientist of 1925 would simply be lost in 1999 material. By the same token, new scholars in 1999 are being educated in a way that leaves them with no connection to why they are doing what they are doing, or what the changes since 1925 were intended to achieve.

"In the field of method some stirrings may be observed," Merriam (1926, 2) stated, and he was often the stirrer, as his audience would have known. "On the whole, the most striking tendency of method during this period has been that toward actual observations of political processes and toward closer analysis of their meaning—this in contrast to a more strictly historical, structural, and legalistic method of approach to the problems of politics" (p. 3).

Merriam evinced in his address an unqualified commitment to "the field of method." He advocated turning concepts and data into terms subject to mathematical measurement, and the acquisition of the data was an important element. Merriam participated in these innovations to a limited degree, particularly with the help of Harold F. Gosnell and Harold D. Lasswell (Simon 1996). He was less the doer than the inspirer, sponsor, and academic entrepreneur. Merriam put a view into the following sentence. "The quantitative study of economic and other social phenomena holds large possibilities of fruitful inquiry, providing of course that the numbers and measurements are related to significant hypotheses or patterns" (p. 7).

As the inspirer, Merriam played a significant role. Quantitative analysis of politics had not been established as a strong central concept, although it was not an unknown idea. Francis Lieber ([1853] 1877), the German-born political scientist, said in 1853 that he had long been interested in election statistics from many countries (see Freidel 1947, 292-5; Haddow 1939, 138-44; Lieber 1993, 21-32; Merriam [1903] 1969; Nevins and Thomas 1952). Some historians working on politics in the 1890s had begun this kind of work. A. Lawrence Lowell, closer in time to Merriam, was on the quantitative trail. In his 1910 presidential address, Lowell anticipated Merriam: "The more complex the phenomena to be studied, the greater the care that must be used in accepting the conclusions given by statistics, and yet in political science they are invalu-

⁴ I have consulted (1) some widely known scholars who are good judges of quantitative methods; (2) Political Analysis, the specialized journal of political methodologists; and (3) cursorlly, the last two years of the APSR, the American Journal of Politics, the Journal of Politics, and the Political Science Quarterly. In this part of the preparation, in particular, I had the excellent help of Amy Kristen Stewart. I have also had the benefit of discussion over the past several years with Paul Freedman and Marc Hetherington, whose explanations have been helpful. In this subject matter, above all, the errors are mine, and no one should blame Preedman, Hetherington, or Stewart for any of it.

able" (pp. 10-1). According to Rogers (1968, xxxi), "if there was not mathematics [in Lowell's writing], at least he used numerals."

Fifteen years after Lowell's presidency came Charles Merriam. At that time, the requirements were still more or less literary: command of the English language, sufficient ability to read German or French, and passage of the field course in political philosophy or history of political theory. As to the dissertation, it was not unusual for a student to choose a topic, or be given one, and write the project with little or no further consultation with the faculty. Merriam proposed a major change in both procedure and substance. Those who studied political science at Chicago testify that Merriam was different, including Herbert Simon (1996, 57, 63), C. Herman Pritchett, Gabriel Almond, and Robert E. Martin. 6

On the whole, political science had accepted this emphasis by the early 1930s. The most obvious form was modern polling techniques, which made it possible to go behind the mere paper record and ask the voters directly what they did and why. This is the hallmark of the work of Angus Campbell and others at the Survey Research Center in Ann Arbor. This procedure became the norm, the expected standard.

Quantification and/or mathematical analysis now appears in virtually every field—international relations, comparative politics, state and local politics, judicial behavior—with the possible exception of political theory. Some major journals are virtually closed to anything else, and most carry a great deal. Very few are predominantly narrative. The same pressure is present, although not to the same degree, in the most prestigious university presses.

Dimensional analysis, model specification, refined distinctions among different types of designs, quasi-

⁵ Thus is shown in the way Emmette S. Redford wrote his dissertation at Harvard. He had great enthusiasm and respect for Charles H. McIlwain at Harvard but decided not to work on political philosophy because he was not good at foreign languages. "McIlwain did not think one should major in political philosophy unless he knew French, German, Latin, and probably Greek." Ultimately, Redford wrote under Arthur Holcombe, who gave him a topic (constitutional problems of American economic planning). He wrote the dissertation without discussing it with anyone, as far as he remembered in later years (Redford interview by William S. Livingstone [Baer, Jewell, and Sigelman 1991, 57]). The way Redford worked at Harvard probably was about the same over the rest of the United States into the 1960s. (It was the way I worked with Norton Long at Northwestern.)

experiments and challenges to the very idea of them, operationalization or measurement of variables and concepts, time-series analysis, panel and cohort analysis, maximum likelihood estimation, Monte Carlo simulation, and bootstrapping are standard matters for the education of new scholars.

Almost twenty years ago Nathaniel Beck (1983, 557) reported the maturation of the statistical study of time series: "While this is not the place to discuss the theoretical merits of these studies, one must be impressed by the growing methodological sophistication of the discipline's graduate students who are now expected to add ARIMA (autoregressive integrated moving average) and GLS (generalized least squares) to their list of acronyms." The list of acronyms grows, as does the need to know how to perform the operations. Young scholars who do not develop this capacity to some minimal degree will be at a disadvantage. Senior scholars who never developed this capacity or have allowed it to wither have to refresh it. Otherwise, they will be like the residents of a country who cannot speak the language of a flood of new migrants.

Indeed, many of us are learning to speak the migrants' language, however ungrammatically. But what of the migrants' ideas? Has the rapid saturation of quantification/mathematical analysis been unrelievedly beneficial for progress in scientific content in politics? When one probes deeply into content, the answer is "no" or "not yet," although this answer requires some nuance. Advanced quantitative/mathematical political science has been striking in regard to electoral studies. and in a lesser degree to legislation—a point to which I return momentarily. Yet, overall, substantive improvement in the whole body of political science is not commensurate with the rate of change in quantification and mathematics. The substantive part of the Merrian program has been fulfilled to a far more limited extent, and in some respects it is virtually untested. The word "method" meant more to him than quantification merely, and in this respect Merriam has been stymied, if not entirely defeated.

If we say Merriam's substantive agenda is not worth studying, that is one answer. If we say that it is worth studying, and that others are doing it well enough already, that is another answer. Either could be advocated or opposed. But to do so explicitly is to proceed in accord with the idea that young scholars should be encouraged to think much more about calling. There is a need for far greater intensity about deciding what is worthy studying and why, actually studying it, reporting it, and distilling its essence into the aggregate body of knowledge.

The competence of political science is, in some respects, vastly enhanced by the triumph of the field of method, but not without costs. With respect, I suggest that the decision rules within our organizational realm—as practiced in the aggregate by deans, department chairs, promotion and tenure committees, conference program committees (to a lesser extent), journal editors, and grant review panels—have a tendency to suppress discovery. But discovery is the name of the game. Herbert Simon (1996), who made thinking one

⁶ Pritchett said: "We were propelled into an acceptance of the fact that political science was a social science and was obligated to use the methods of the other social sciences" (Pritchett interview by Gordon Baker [Baer, Jewell, and Sigelman 1991, 109]). Gabriel Almond stated: "While I suffered a bit at the beginning of my career because a Ph.D. in political science from Chicago was a little unconventional, I got an enormous advantage out of it in the period after World War II, when I discovered that I was about a decade ahead in terms of my training as compared with colleagues in my age group" (Almond interview by Richard A. Brody [Baer, Jewell, and Sigelman 1991, 123]). Robert E. Martin commented in the same mode, referring to himself: (I was) "the first black political scientist to do a doctoral dissertation in political science with heavy reliance on quantitative methodology my study of the AAA referenda, which produced hundreds of statistical tables and numerous graphs" (Martin interview by Russell Adams [Baer, Jewell, and Sigelman 1991, 162]).

of his many specialties, is uncertain exactly how he thinks. He is satisfied if he can form problems through loose unrigorous symbols in the mind, subject to proof later. If that is so for Simon, surely it is so for the rest of us. At least it is for me. Some costs are too high, and some adjustments are intellectually feasible.

THE OLD LITERATURE AND HISTORY STILL COUNT

The corpus of political science is, and will long continue to be, a source from which important understanding can be distilled. In this I include political theory of the grand tradition (Holden 1996, 16-20), which is not a subject I studied well. In pondering some problems of public administration, I wondered which of the earlier writers would have been concerned with such matters. By no analytical process that I can identify, Hobbes came to mind, which in a millisecond I justified on the basis that Hobbes would have to be concerned with executive power. He was. Thus, when I went to consult Hobbes ([1651] 1991, chap. 23) I found a concept of "staff" and "line" quite as serviceable as that in contemporary public administration. It is useful to know that the concept had a good formulation three hundred years before it came into American writing.

This to me was evidence not of the backwardness of American scholarship but of the underlying permanence (or at least recurrence) of the phenomenon. For instance, I find great intellectual power in the dictum that "the making and maintaining of commonwealths" depends on skills that have to be studied, not merely on practice. For look what a mess practice has produced (Hobbes [1651] 1991, chap. 20). Deutsch's (1953; Deutsch et al. 1957) movement, in an early quantitative format, from nationalism and social communication to the historical problem of integrating the European political communities is very close to this idea, and both are helpful. That does not mean my preference is limited to political theory of the grand tradition. There is also the vast store of empirical literature—based on experiential observation or reporting and not primarily normative—that has accrued over the course of political science's existence.7

The potentialities of a political science of history also warrant our attention (Holden 1999). That merely means the application of analytical questions or principles of political science to data, extracted from such records as we obtain or re-create, about the actions and behavior of people long since dead. It is highly probable that many of the things we believe about the United States (or any other country, perhaps) will be altered significantly by close study of data from the past.

Beyond this, I propose a deliberate decision to encourage "soft" research. Merriam (1926, 2) referred

approvingly to an improvement in methods: "The most striking tendency of method during this period has been that toward actual observations of political processes and toward closer analysis of their meaning." What are the ways of actual observation? One of the ways of "actual observation," is participant observation (Holden 1996). It remains the best single method of hearing and seeing directly what politicians do as they are doing it. The work produced by its most consistent and explicit advocate, Richard Fenno, receives nothing but well-merited praise (Fiorina 1996, 90). Yet, the methodologists hardly take note of his writing on the method that he himself employs (Fenno 1990).

The triumph of method to which I refer has carried us away from the aspiration toward actual observations. Reinvigorating such qualitative research is desirable because, at minimum, it is a precursor to quantitative work that may yet be years away. Participant-observation has been the subject of one article in the leading annual on methods. I am aware of no other, although in some disciplines it has been reconceived as ethnographic research (Schwartzman 1993). Merriam found that the most striking tendency in methods was the use of "actual observation of political processes and closer analysis for their meaning," but the tendency has been sharply reduced. This works against the competence (knowledge and capability) of political science.

Leaving aside the observation method for a moment, I turn to the importance of thinking. Simon (1996, 106-7) says that he used mathematics as a means of thought, and this mathematics "is relatively unrigorous, loose, heuristic," and "solutions had to be checked for correctness." He found unexpectedly that Tjalling Koopmans did not agree at all. Koopmans deemed mathematics a language of proof "to guarantee that conclusions were correct, that they could be derived rigorously" (pp. 106-7). Simon could never persuade Koopmans that "the logic of discovery is quite different from the logic of verification." Their friendship continued until Koopmans's death. Perhaps had Koopmans lived longer, Simon said they might have reconciled their different views about mathematics. "It is his view, of course, that prevails in economics today, and to my mind it is a great pity for economics and the world that it does" (Simon 1996, 107).

Anything should do if it helps one formulate the problem. It is impressive that Simon had such a view. But there is no reason the same principle is not legitimate even if the problem is formed with words or even with literal pictures in the mind. There are times when the significant intellectual problems flow directly out of the solution of an instant problem. But that is not the only way. Scientific progress demands methodological flexibility.

On this point, let me recite a fish story—from ichthyology, not the liars' bench. The salmon is born,

⁷ Due to changes in style and language, it is necessary to note that "empirical" refers to experience and can be based as well upon the naturalists' method of observation as upon structured experiment. Simon (1996, 57) makes the point that his Administrative Behavior was empirical in that sense (p. 59 n.). His "Observation of a Business Decision" (Cyert, Simon, and Trow 1956), which in my opinion contains the seeds of "bounded rationality," is similarly based.

⁸ On this I have had some encouraging comment by a young colleague (and e-mail correspondent), Patrick Wolf.

⁹ The first issue of *Political Methodology* contained an article on participant-observation by Jennie-Keith Ross and Marc Howard Ross (1974). I believe this first article may have been the last on the subject.

ranges very far from the natal stream, returns, spawns progeny, and dies (Hasler and Scholz 1983, 12). How? The movement of the salmon from birth to death has been a subject of speculation and study for a long time. Between the 1940s and the 1970s, Arthur Hasler and his students worked out the solution. Did he get the answer from literature or experiments? No. On vacation, climbing in the mountains, he encountered "a cool breeze bearing the fragrance of mosses and columbine." The odor so impressed him "that it evoked a flood of memories of boyhood chums and deeds long since vanished from conscious memory" (p. 12). The very next sentence states: "The association was so strong that I immediately applied it to the problem of salmon homing" (p. 12).

He had the answer, but he had to find the solution, the reason for believing the answer definitively. The successive steps took several years: an hypothesis that each stream contains a particular bouquet of fragrances to which salmon become imprinted before emigrating to the ocean; salmon subsequently use this odor as a cue to identify their natal tributary upon their return from the sea; the soil and vegetation of each drainage basin impart a distinct odor to the water, providing the unique cue for homing; and formalizing the hypothesis in collaboration with one of his students, Warren Wisby, in 1951 (Hasler and Scholz 1983, 12).

Imagine a political scientist saying to a tough critic: "I got this great idea!" "Yeah, how did you get that?" "I smelled a mountain breeze and it reminded me of my childhood." This excursus on "method" is to make one observation stand in bold relief. The triumph of "the field of method," as Merriam called it, has the effect of suppressing much inquiry that cannot be conducted in the forms of the most advanced quantification/mathematics. From the point of a science of politics this is counterproductive. The competence of political science is diminished if emergent political scientists increasingly are obstructed from working in simple ways—if they wish to attain the highest levels of respect—when the more advanced ways will never reach the purpose. The competence of political science is enhanced if young scholars are given greater latitude in the ways of "soft" research, from traditional theory and history to sheer thinking, all in the purpose of a stronger substantive purpose.

The strongest concentration of combining advanced methodology and substance probably is around political parties, elections, and public opinion. My guess, and it is a guess, is that the next area of concentration is "legislation," which in contemporary political science can refer to committee politics, floor politics, and congressional voting (if not otherwise characterized as floor politics). In a sense, this area analyzes politics as the production and use of information (Ferejohn and Kuklinski 1990), with some attention to money (although not much explicitly). It is about democracy as a going concern, in which most people participate at a fairly low level of intensity. There are very well-regarded studies of this type, such as the widely cited work by Lupia and McCubbins (1998).

Elections are very important to the arrangement of

power, at least in some societies and countries. But the election is not itself mainly an exercise of power. There are exercises of power in strategic and tactical decisions that will structure who gets to run for office. But in free countries these are chiefly nonelectoral and preelectoral decisions. Elections themselves are not exercises of power, except that they give the command to someone to "get out of office." Nor are they usually conceived as such by the voters. They are, as exercises of power, spasmodic at best. When the command to "get out of office" is ignored or resisted, it can be enforced only by various nonelectoral pressures (such as when powerful groups indicate that they will not cooperate with the resisters) or, ultimately, when "the popular will" is manifest by boycotts, protests, or more strenuous action.10 If I reason correctly, then the part of political science where quantitative/mathematical advance is most to be celebrated often leaves our understanding of the exercise of power relatively unaffected. It also leaves relatively unaffected the most disruptive factors in the parameters of politics, and quite often the products of politics (policy).

BIG ISSUES IN "CONVENTIONAL" FIELDS

What should follow if most of the substantive part of the Merriam program were taken into serious account in planning a research agenda on a large scale? The choices would concentrate on inquiries into the organization and exercise of power. In the near to middle term (five to ten years) there should be an intense concentration on both establishing the "fact-content" as well as developing the "principles and conclusions," which would mean a full review of all the areas mentioned and their logical and strategic interrelations. The ultimate objective would be, as the economist Alfred Marshall ([1890] 1997, xi) said of his own book, "an attempt to present a modern version of old doctrines with the aid of the new work, and with reference to the new problems of our own age." Ultimately, that means a coherent summation of the findings of empirical research and their integration across the fields of the discipline. This is notably important, as the fields themselves (American, Comparative, etc.) are more conventional social organizations-organizational subrealms, so to speak-than they are intellectually coherent units.11

My point of departure is a concern for power. Four

¹⁰ This kind of action was reported in the media regarding Indonesia in October 1999. In due course, some scholar presumably will discuss whether this is a correct interpretation of that situation.

¹¹ It also means a clear conception, clearly argued, as to what part of our present empirical and conceptual core should now be abandoned because it is not grounded in the product of the most advanced quantitative/mathematical inquiry. Conversely, it entails the clearest attention to the theoretical significance of the adaptations that have occurred, including a clear summation as to whether they are producing empirically correct results that are more than a numeralized form of contemporary institutional history. I am contemplating a matrix with the major methodological innovations and techniques on one axis and substantive fields of work on the other. This may be impractical, although I do not see why it should be more than tedious. Collegial comment would be welcome.

sets of problems are strategic within this context: public administration, the nature of political interests, the politics of urbanization, and the interpenetration of politics and economics. They would permit, and sometimes require, linkages across the existing fields.

The Centrality of Administration

If students of politics, in this country and around the world, wish to observe power in action, then the crucial focus is discretion regarding the actual use of information, money or its surrogates, and force. These are the resources necessary to induce joint action, and they are found in every society. Their use is what I mean by the centrality of administration to power. Inverting Long's (1962) insight, administration is the lifeblood of power. This is not the "public administration" of a merely technical logic. The issue is still relevant to many governments in the world, including some at the subnational level in the United States. From the point of view of the organization of control, one can develop a much deeper agenda of research.12 Within "public administration," four matters appear to me of fundamental importance: the mechanisms of power, public opinion and public administration, public administration and the plural society, and the politics of prosecu-

Three Mechanisms of Power. The expectation is that if one takes all the units in the world called "nation-states," one will discover two mechanisms of power, and in some societies a third.

Mechanism 1 is the structure for giving and receiving executive direction at very high levels. We may think of it as aggregating data that will motivate authorities at the highest level, allow them to engage in decision making, and announce, declare, or proclaim their decisions. This structure is triangular. There is a central or chief executive: president, king, prince, pope, prime minister, and so on. There is an "entourage" (which more conventional political science language used to call "staff"), a network of "friends and cronies," "young servants," and "migratory technocrats" floating around the chief executive. ¹³ The third part of this structure consists of operating entities in the giving and receiving

12 This can be said if there is effective public government. I note that there is great discussion today of "privatization" and a "New Public Management." The resonance of this language was made clearer to me in a conversation with R. A. W. Rhodes at the meeting of the Political Studies Association of the United Kingdom in March 1999. My thesis, however, is that privatization does nothing to alter the basic pattern except one of two things. (1) It reduces the scope of public government, and in that event transfers power to the corporation or some nonprofit form of private government. (2) It keeps the scope of public government in form, maintaining the form of policymaking at the highest level, and delegated, assigned, or contracted program decisions and operational detail decision to some private entity. I benefited from separate discussions of the same general subject with Lawrence Howard, who, given his values and epistemological assumptions, expressed discouragement, and with Michael B. Preston, who merely treated the issue clinically.

¹³ This formulation turns out to be convergent with studies of high level executive decision making being conducted by a group of psychiatrists (Volkan et al. 1998, 160-1).

of executive direction. In the United States, the presidency (in which the president is the center but not the totality) is mechanism 1, but every nation-state has a mechanism 1, as does every state (and most local governments of any size).

Within this mechanism is a simple process that I designate as "bargaining and command." Presidents (or the chief executives in other systems) have a common feature. They are human beings with biological and psychological demands upon them, just as other human beings are. They operate with conceptions of free choice (authority and power to do whatever they think right), but they are also subject to organizational demand from the aggregate of subordinates about what is wise and right. They are also subject to demands from the world outside official government.

Delegation, formal and informal, acknowledged and unacknowledged, enters into the decision-making process. Between chief executives and official subordinates there is a continuous and simultaneous process of bargaining and command. This is not a research finding but a synthetic statement based upon what we know (or at least what I know) from the written record. Research can be planned and executed to elaborate and test the formulation against any political system in the world for which there is any chance of obtaining data. There is likely to be some variation based upon culture, legal authority, and so forth. The president of Finland has more power within Finnish government than the president of the United States has within American government, but neither can be wholly exempt from the generalization. The formulation is also testable against the transactions between the president of the United States, the presidential entourage, and the operating agency heads (secretaries and others) from 1789 to

I make two disclaimers and a bold claim. Disclaimer 1: I know little, if anything, more of President Clinton than does any other member of our discipline, and so far as I know, he has no reason to know my name. Disclaimer 2: I know even less of George Stephanopoulos. Claim: I know George Stephanopoulos and knew him before President Clinton knew him. This is not extrasensory perception or second sight. Stephanopoulos fits my model as the ambitious young servant. This conclusion stands solely on the evidence of his own published work and the known public record (Stephanopoulos 1999).

Mechanism 1 embodies, among other things, the fact of intercollective (interagency/"bureaucratic") politics, which is remarkably widespread. What makes mechanism 1 attractive, moreover, is that I can hope for intertemporal, international comparisons. For example, it can be applied to the Roman empire, for the period described in Fergus Millar's (1992) wonderful account of the emperor as a working official, not a movie character. Similarly, from the work of Conyers Read (1960, 1961), Sir John Neale (1953), and others, it can be applied to government under Elizabeth I.

Mechanism 1 sometimes involves democracy, but not necessarily. The extreme case is Nazi Germany,

whose propaganda systems surely were the antithesis of democracy. Yet, the competing propaganda organizations reportedly fought among themselves during the war, and Hitler—der Fuhrer, remember!—was reluctant to intervene and avoided seeing the combatant agency chiefs when he could.

Mechanism 2, considered generically, meets the problem of field administration. Again, the potentiality for systematic comparison is enormous. One cannot conceive a world without field administration any more than one can conceive a world without some system for food distribution. If central decision makers could dispose of these resources perfectly, then central control should always result. Those who are granted authority in the field are presumed to act upon decision premises adopted by central decision makers. Max Weber's model of bureaucracy fits what people came to believe, at any rate.

All over the world, however, we encounter observations that tell us there is a fruitful potentiality in the study of imperfect hierarchy. Terminology is not well established for administrative practices that do not accord with the pure model of central control. The terminology that I adopt here to express some vertical stratification, but imperfect degrees of hierarchy, is "devolution" and "negotiated power-sharing." Imperfect hierarchy sometimes is precisely displayed where the theory of hierarchy is most explicit. Imperfect hierarchy is obvious but has not always been accepted into theories of administration, although in the past decade and a half American scholars concerned with implementation have rediscovered, and to some extent reemphasized, "nonhierarchical decision making" (Levin and Ferman 1985). But as political scientists we may encourage students to look more broadly, for the imperfection of hierarchy is more pervasive, in many other polities.

The actions of those who are supposed to be subordinates may deviate from what central decision makers would have preferred for at least five reasons: (1) subordinates lack the physical or mental ability to perform what the central decision maker would require; (2) field-level decision makers have been given a bigger job than they can possibly handle; (3) the field administrator is protecting his or her own self-interest, in the narrowest sense; (4) field administrators do not agree with what they have been told to do and implement the orders in ways that are more satisfactory by their own lights; and (5) field administrators feel driven to modify, or reinterpret, their orders or compliance procedures, in accord with the local environment, so as to attract support from people who will not, in their opinion, comply with the orders as initially issued.

Devolution is essentially a bargain between the most central decision maker and the principal field subordinates, who by law, fiction, or custom are allowed dependent positions of rulership. It also involves the willingness and ability of the superior to intervene, punish, and even replace the subordinate official who does not toe the line, or who becomes ineffective. The bargain that is entered provides the benefits of office and the protection of the superior authority in ex-

change for the field ruler's willingness and ability to execute the controls, practices, and policies that the central authority desires.

Negotiated power-sharing (or what is called "intergovernmental relations" [IGR] in the United States) arises when the "superior" level of government has some incentive to offer for a means of action other than compulsion but is not willing to let the subordinate unit have completely free rein. It must mitigate demands it might otherwise then make. More than three decades ago, Michael Lipsky (1982) offered the concept of street-level bureaucracy to describe the transactions between the Nth official in the authority structure and the outside persons with whom an official entity deals.

Practice at this level reveals social realities, even to a degree that those who articulate social doctrine may wish to deny. What is to be said of rights when a police officer can, within station precincts, not merely beat a prisoner but jab a stick into the prisoner's anus?¹⁴ What of Waco?¹⁵

Austin Ranney once observed that most people "almost never see anyone or have dealings with anyone other than 'administrators.' "¹⁶ The "social constitution" is manifested in the daily transactions between those who exercise administrative discretion and those with whom they deal.

Mechanism 3 exists in some polities but not others. It is the optional institutions and processes through which constraints are imposed upon administration. Constraint is limitation, but it is also imperative direction. The distinction between "constitutional" or "democratic" governmental systems, as we now use those terms, and others is whether there are mechanisms for effective constraints. Collective bodies (legislatures or assemblies) are the principal constraining institutions, but adjudicative bodies (courts) also provide means for this end result.

In the American case, at the national level, constraint is notably present in the congressional process. Conceptually, there are four types of transactions between Congress and the executive branch: (1) for the purpose of achieving some legislation; (2) for the purpose of helping constituents ("intermediation"); (3) for the purpose of "oversight" in the more technical sense of getting better administrative performance or service; and (4) for the infliction of pain upon some executive, some set of executive subordinates, or others (regardless of whether the body has any constitutional authority to inflict pain).

The peculiar power of the U.S. Congress, incomparably the most powerful collective body in the world, is directly related to its deep engagement, through vari-

¹⁴ This refers to a New York City police matter in 1999 which is now in the public reports and on which I have not carried out a detailed analysis.

¹⁵ Various allegations have been made about the conduct of federal field agents in the Branch Davidian matter in 1993. The case had yielded, by the time of writing, much senatorial criticism of Attorney General Janet Reno, much reported (but not verified) conflict between the attorney general and the Federal Bureau of Investigation, and the appointment as special counsel of former U.S. senator John Danforth.

¹⁶ Letter to the author, March 25, 1969.

ous constraint procedures, in the administrative process. Congressional investigation is a means by which Congress exercises particular influence upon the administrative process. Upon close review, it appears that at the moment there is no collective judgment among students of the legislative process about this form of congressional power.

There is a small residue of lost knowledge on the congressional investigative power, since Marshall Dimock (1929) wrote a pioneering study near the time of Teapot Dome. Dimock and two other political scientists from prior to World War II wrote books that are "lost" in the sense that no one reads them much now (Dimock 1929; Eberling 1928; McGeary 1940). In the twentieth century, the number of investigations has, presumably, been vast. I have seen no specific accounting. Nelson McGeary's (1940, 9) study, about a dozen vears after Dimock's book came out, reported 146 investigations in a ten-year period. Some material is available to us in three encyclopedia articles (Johnson 1995; Relyea 1995; Tiefer 1995); a very informative account by a participant-observer of the work of the Church Committee, a Senate committee that investigated the Central Intelligence Agency during and immediately after the Nixon administration (Johnson 1988); and one synoptic review of high publicity investigations since World War II (Mayhew 1991).

There is considerable opportunity to regain lost knowledge and to extend discovery by a deeper search into how the congressional investigation has developed and changed in the United States¹⁷ as well as a great opportunity and need for further inquiry. The question of what functions investigation now serves or accomplishes, and of what the functional equivalents are in other political systems are extremely important. Here is a momentous opportunity for some energetic scholar.

Public Opinion and Public Administration. The second area I find particularly compelling is public administration and public opinion. If public opinion is fundamental, then we should expect its manifestations in all the processes of politics (adjudication, legislation, administration). Let me offer a set of reasoned conjectures.

¹⁷ I reach this conclusion after extensive review of electronic search sources, notably J-STOR, Social Science Abstracts, and Public Affairs Information Service. In particular, in July 1999, I reviewed J-STOR for all articles on "congressional investigation" published from 1940 through 1999 in journals in political science, history, economics, and sociology. That relatively broad search yielded a total of 89 articles for the sixty-year period, in the four disciplines. Dissertation Abstracts (1983–99) shows 116 entries. Most of these entries have nothing to do with congressional investigation, but come up as artifacts of the indexing system. If they mentioned either congressional or investigation, the system identified them. My supposition has been, however, that there must be other research of which I am unaware. Accordingly, I consulted some half dozen leading scholars in congressional studies and generally found agreement that my search had about exhausted the actual material.

¹⁸ Beryl Radm (personal communication) points out that this is now very important to American public administrators, shell-shocked by other political attacks. Radm also notes ambiguities in the definition of public opinion. Patrick J. Wolf (personal communication) offers other useful comments, including the normative challenge as to whether public opinion should play a role inside administration. I

First, there is a coercive quality to public opinion. It imposes demands and prohibitions on and expresses tolerances for various actions and results. Public opinion tends, when issues are taken seriously, to a form in which debate produces great anger; side A speaks as if it wished to destroy side B. If both sides are serious, then we should expect an agency to suffer considerable pain if it chooses a decision preferred by one side, but it may be under great pressure to do exactly that. Charles McCarthy (1912, 180), writing about Wisconsin state government, stated that public sentiment might force regulatory commissions to deal perhaps harshly with regulated companies, but ultimately the companies would be protected by the courts.

Second, the power of culture over opinion is that culture underlies opinion, at least the sort of opinion that is normally measured (and perhaps the sort that is measurable). The significance of such cultural values is frequently ignored by students of administration as well as by students of public opinion. Perhaps in the rapidly emerging techniques of research there are much better instruments than poll data. But I have asserted—and not yet tested—that poll data are similar to the surface froth at the ocean known as whitecaps; what truly is important for the continuing public administrator are the beliefs and feelings more analogous to deep ocean currents.

Third, the variety of means for introducing public opinion into the administrative process is very wide: the actions of collective bodies (legislatures or otherwise), common gossip and other means of cultural communication, extralegal and illegal actions, interest groups, legal processes such as hearings and complaint procedures, opinion surveys (Key 1961), and so on.

Fourth, administrative agencies purposely act to influence opinion, which one recent private commentator noted on the draft of a cognate paper as "political entrepreneurialism." The various activities intended to produce opinion amount to public relations or propaganda (in both the narrowest and the broadest senses); the reinterpretation of any given proposal to presumptive larger policy; the reinterpretation of data to fit new circumstances; the cooperation with external organizations on favorable terms; and, on the controlling side, censorship and information control (leaking as a form of control) as well as pejorative rhetoric.

Public Administration and the Plural Society. Most states, for most of human history, have been multiethnic, not homogeneous. This is true even for Western Europe. Therefore, administrators (who have the capacity to make decisions about the definition, collection, dissemination, and use of information) will act in situations such that their purposes, interactions, and consequences will be varied as to ethnic effect. Accordingly, their actions will be varied as to consequences for political stability, change, and viability.

The Politics of Prosecution. In the terms of Merriam's (1926) agenda, prosecution is a variant on constitu-

similarly acknowledge helpful written comment by Paul Freedman and face-to-face comment by Christopher Wlezien.

tional law. The potential for convergence in the politics of prosecution of American politics, public administration, comparative politics, and international politics lies in the parameters, forms of action, and products and consequences of action by government lawyers (in the American case) against other persons.

The prosecutorial function represents great intellectual opportunity. The power of someone or some institution (1) to make some person or institution (2) present itself before some entity (3) upon some pain for failing to do so is crucial. The prosecutor applies controlled and subdued force, exercised through the forms of law, to threaten the loss of freedom, secure that loss, take away money by requiring one to spend for defense, deprive one of peace of mind and quiet sleep, and ruin good reputation. While all the powers of the prosecutor are important, the prosecutor, more than any other, is the official with the power and authority to inflict a scandal by the mere circulation of information that action is being considered.

Prosecution depends upon the collective reaction to the intersection of an act deemed an offense (since there can be no crime without a prior law), the party by whom the act is committed (the offender), the party against whom the act is committed (the victim), and the rest of the collectivity. The question is whether the rest of the collectivity thinks this intersection demands reward, penalty, or neutrality.

The more factionalism increases, the more prosecution will be a part of the struggle. For example, the discussion of abolishing black slavery became, in the 1830s, truly subversive. White people who opposed immediate emancipation held up the fear that a vast black population, out of control, would wage war against the former masters. Whites identified as abolitionists became the objects of control and suppression by those who favored slavery or even those who said they favored "colonization." The issue was not black freedom but the suppression of white debate. Prosecution could play a part. In one account I have seen, a colonizationist district attorney was intent on making a case against an abolitionist and the dangers of immediate emancipation. 19

I invite consideration of this issue from the students of comparative politics, who must have relevant evidence from a number of countries, particularly Italy and Korea, where heads of government or heads of state have been prosecuted in the context of severe domestic political controversy. Prosecution becomes, of course, irrelevant when the forms of political conflict depart from the symbolism of words and the forms of law. At that point, politics returns to its elemental form as force.

Public administration as restated here—in terms of mechanisms, public opinion, ethnic plurality, and prosecution—is emphasized as a logical, essential, central component of reframing a science of politics. I now

turn to another element of the political underpinning encompassed by Merriam's "Progress in Political Research," namely, his truncated suggestion about the nature of political interests.

The Nature of Political Interests

What Merriam meant by "the nature of political interests" is not fully explained. But the analysis of interest groups virtually always assumes the "interest" and focuses on the "group." Under Truman's (1951) adaptation of Arthur Bentley (1908), the interest group is inherent in all politics. At one level of thinking, I believe it would be useful to distill the major concepts, propositions, or implicit theory in Truman's book, separated from his data, which necessarily are time bound. The essentials then could be reviewed point by point in light of studies and specific findings that relate to them.

Baumgartner and Leech (1996, 523) offer a useful heuristic contribution. They critique an article that claims "to refute a prevailing wisdom that groups lobby legislators who already support them [and] conclude that groups tend to lobby those who oppose them, and lobby their supporters primarily to counteract lobbying by other groups." Their methods critique is impressive on its face.²⁰

There is a need at this stage in the collective research life of political scientists for a systematic comparison of the literature over several decades. Truman's (1951, chaps. 1–4) examination of groups starts with the role of groups in society and the circumstances of their activity in governing, and he drew heavily from social psychology and applied social anthropology. Broadly, he examined group organization and problems of leadership (the internal dynamics and cohesion of groups), their relations to government, and their tactics of influence (in different institutions), and he concludes with a view of group politics and representative democracy.

What of "interest" itself? In the deepest inquiry, we seek to identify the variables that will motivate political action before it occurs. In this respect, we should be looking for the driving forces of political action. Imagine that one is a consultant in Lagos, Nigeria. What should one look for in order to anticipate what the local authorities will do? One does not have sufficient knowledge if one has to depend on a catalog of existing groups.

Whether the presence of interest groups has the consequences that Lowi (1969) attributes and whether there is a possible adjustment are very important problems. But is interest truly so fundamental? Madison proposed that interest is universal, grounded in "human nature." If so, then there is a problem in discovering interest expression in a polity that does not even purport to be democratic. Most research is about

¹⁹ See Filler 1963, 75, n. 61; Goodell 1852, 437–8, which also cites Crandall 1836, 46. Filler also refers to Delaplaine 1937, 308. The trial, acquittal, self-exile, and death of Reuben Crandall are briefly described in Strane 1990.

²⁰ Indeed, though I discovered this article very late in revisions on my address, using J-STOR, I am surprised not to have found a response by the authors they critiqued. Perhaps it exists in some form other than an article (e.g., a letter) that would not have been identified by the search mode I used, i.e., asking for "articles."

the United States, then about the United Kingdom, and then about various forms of action in the advanced industrial countries since World War II.

Interest groups are largely conceived as "economic," although neither Bentley nor Truman accepts this limitation. Hirschman (1977), however, discusses the arguments for "interest" as economic factors, more suitable to regimes than less rational passions or more emotional or extrarational behavior.

The Politics of Urbanization

I regard the politics of urbanization as one of the strategic areas that may now span the fields of American Politics and Comparative Politics. But this point should be placed against the specific background of Merriam's address. Merriam argued that there has been great progress in the study of the problem of the modern city, progress related principally to the establishment of research entities that provided data to improve urban government. At the end of the twentieth century, students should recall what American cities were like at its beginning. Essentially, the city was a receptacle into which vast numbers of human beings poured rapidly from all over the world. The governance of the city was affected by the need to reconcile these multiple populations to each other. The governance was largely directed by the urban rich who controlled the economy (Almond 1998) and by the urban bosses who controlled the cities (Gosnell 1937; Gottfried 1962; McKean 1940; Zink 1930) and treated them as their own business enterprises. Students should know the practices of local politics. It may be "colorful" to tell stories of people who got jobs on the public payroll because they were diligent party workers. But those jobs were not free, waiting to be taken. They meant that others on the public payroll had to be fired, in a world where "equilibrium" included a good deal of unemployment.21 Merriam, the theorist turned politician, was with the reformers (Karl 1974). He thus could cite the work of Leonard D. White in developing the study of public administration—chiefly the study of personnel, which White raised to an art form—as an alternative to absolute patronage politics. Merriam thus celebrated the rise of research institutions that provided data and arguments to support reform of city government, in the attempt to overcome the use of the city as a place for the boss to get rich.

From the 1930s into the 1960s, for a variety of reasons, urban politics was not a growth field in political science (Herson 1957). Its revival in the 1950s with a vast amount of research on urban politics (Banfield 1960; Sayre and Kaufman 1960), metropolitan areas (Wood 1964), and notably on the "community power" themes (Dahl 1962; Hunter 1953; Long 1962) can easily be retraced by those who wish. These themes are revisited by Clarence Stone (1989) and those who are persuaded of his approach to "urban regimes." There are, in my estimation, three main threads in the political science that deals with the problems of the modern city: the city as the miniature polity, the city as local government, and the city as a manifestation of the process of urbanization. The last offers the important intellectual opportunity at this stage of the discipline.

Urbanization is important now for three reasons. First, it simply expresses the increase in scale, with the issues about participation as scale goes up (Dahl 1967, 953-70). That would be important itself. But, second, urbanization also yields a version of the Hobbesian problem of making and maintaining commonwealths because it means the drawing into one territorial area, over which control has to be exercised, multiple populations from diverse geographical, cultural, and ethnic settings. In other words, human population is not only more dense, but the multiple social systems are more closely packed together. It is likely that this becomes a more and more compelling human problem as the part of the world that is not rich becomes urbanized. Third, the politics of urbanization is qualitatively different because the very fact of urbanization produces a major shift in our relationship to the physical environment, and thus produces qualitatively different problems of conflict and decision making. Urbanization and the environmental problem are not identical, but they are intimately related.

The Interpenetration of Politics and Economics

In discussing a variety of big public problems, Merriam (1926, 8) maintained that "neither the facts and technique of economics alone, nor of politics alone, nor of history alone, are adequate to their analysis and interpretation." The issue that I raise is the interpretation of politics through contemporary neoclassical theory.²² Coase (1988) refers explicitly to "the economic theory of politics," and Williamson (1996, 251–2) refers to "the use of economic reasoning to examine politics and

²¹ Colleagues will have to endure one anecdote that I deem relevant. Somewhere around the time of the Truman administration, when the old system still existed but was collapsing, I assisted the precinct captain in the 115th Precinct of the 2d Ward Regular Democratic Organization. One resident on our block, Mrs. Walker, lived with her grandchildren and son-in-law, and she was a vigorous, outspoken, Republican. My precinct captain (Albert Grimes) was unhappy with her, though there was no chance the precinct would turn Republican. "We ought to be able to do something about her," he said. "Her son-in-law works for the city, doesn't he?" The son-in-law was a driver on the Chicago Transit Authority. Under the conditions that then existed, however, nothing seems ever to have happened, but Mr. Grimes's presumption was that something could happen and that it would have been legitimate for him to have gotten the Ward Committeeman to take the necessary action.

²² I merely note but do not analyze here the controversy between missionaries of "rational choice" (Green and Shapiro 1995) and dedicated opponents of "rational choice" (Priedman 1996). It is important, but I cannot treat it here, and I hope to get to it at some other time and place. It is rather similar to the controversy between "behavioralists" and "institutionalists" in the later 1950s and 1960s, in that the dispute is confused and exaggerated. People say very similar things to praise their fellow believers and to criticize those who do not believe. It is expected to have considerable consequence for individuals and their futures. Finally, much of what is said has little to do with the exercise of power. Perhaps I may change my mind upon further study.

political institutions." Political relationships can be stated and reasoned about using economics terminology. That which is absolutely fresh is, of course, a net advantage to all, if it is actually knowledge.

Perhaps we might call this approach the Neoclassical Economic Politics (NEP). Some colleagues of extraordinary talent and achievement may not believe there is knowledge that remains independent of adopting economics per se as our guidepost (Alt and Shepsle 1990). I am respectfully skeptical. On the one hand, it is right that we learn more about economic concepts and the practical workings of the market system. On the other hand, I think it is absolutely wrong to set political science students adrift upon the neoclassical sea with no compass. The first objection is that neoclassical economics, which is now being imported as a source of guidance on politics, is grounded in the most extraordinary political innocence.

James Buchanan (1992) provides the evidence for this innocence. At the technical level, he was a student of public finance. His work was influenced by certain Italian economists, he says, because they "routinely modeled political decision structures as part of their work" (pp. 82–4). The sentence comes from his autobiography,²³ but the Italian influence can be found in some of his writing on public finance (Buchanan 1987, chap. 10, esp. 128–9). Buchanan expressly separates himself from the U.S. tradition that treated government as a "monolithic and benevolent" actor. If that is what English-speaking economists taught their students, then they could have abandoned it a long time ago.

Economists, at least in America, have ignored political knowledge that could have been theirs. That is, they have simply rejected the institutionalism of such economists as John Maurice Clark (1957). Williamson (1996, n. 2), a leading figure in the new institutional restatement, has the following mild comment in a footnote about a view expressed by Coase: "Some concluded, too harshly I think, that the work of American institutionalists led to nothing . . . [since] without a theory, they had nothing to pass on." In other words, Williamson thinks there is some intellectual value to the institutionalist residue. In short, economists had another intellectual structure to protect, and they could not protect it if they allowed politics to enter their considerations.²⁴

All they needed to escape that blind spot was interest group politics. The only reasonable inference is that economists as a group made a collective professional decision not to learn interest group politics. People have known about interest group politics in a general way for a long time. Merriam targeted it as a potential research topic in the 1925 address. Childs (1930) and Herring (1929) published relevant work in the 1920s. Schattschneider (1935) wrote on the issue at the beginning of the 1930s, using an economic subject, the tariff,

as his source of material. Truman produced his masterful synthesis in 1951,²⁵ of which there have been many criticisms, but for which there is yet no intellectual replacement incorporating research over the past two decades.

The second difficulty is that there are aspects of economics that economists seem no longer to need to know, but these are important to students of politics. There is no reason to start with economists' concepts per se, especially as it has so often proved necessary to modify them in order to adapt them to observed politics. The adoption of economists' concepts may well be analogous to merging with a firm that is constantly restating its assets. Economists may have no need to reexamine the role of poverty in the thinking of Alfred Marshall, the history of the fight between abstract theorists and institutionalists in the United States, the long struggle between the followers of John Maynard Keynes and the followers of the Austrians and Lionel Robbins. Since the history of economic thought is disappearing from curricula, economists themselves will soon be of no value on those issues. But political scientists who absorb economics as their guidepost have such a need.

Political scientists have the capacity to contribute to broader human understanding. Their proper premise is the interpenetration of politics and economics and, in some respects, the primacy of politics over economics. The competence of political science should be enhanced and carried to a political science of industrial organization. I do not mean that nothing else is relevant, for that is shallow in the extreme. I do mean that the political scientist who starts from political science concepts will make a contribution and will discover other things of relevance. Three areas of inquiry seem reasonable: the corporation, industry sectors, and the larger arena.

Corporate Governance. Opportunities for inquiry about the interpenetration of economics and politics abound in the subject of corporate governance. This is not a new idea. It is expressed in Drucker's (1978, 262–3) writing during World War II, although Drucker believes it hurt him academically. It is expressed in a different manner in the behavioral theory of the firm, as set forth by Cyert and March (1963).

Here are some practical examples. Within the world of the American corporation, the chief executive officer (CEO) is well known. As an initial point, there would be great merit in study of the tenure and turnover of CEOs. Then there would be merit in the study of how CEOs operate, as we also have a professional interest in how public executives operate. The mythology of the CEO as absolute boss, within the limits of the law, is

²³ Buchanan cites, to this point, Viti de Marco 1936.

²⁴ My recollection is that one reason Aaron Wildavsky studied the budgetary process was his dissatisfaction with the budgetary writing of Arthur H. Smithies.

²⁵ When a Nobel Prize is created, at some remote point, for political science, Truman's *The Governmental Process* (1951) will deserve that prize. Admittedly, others would make other choices. Florina (1996, 91) speculated on the idea of Mancur Olson as such a designee. I assume Fiorina was thinking of Olson as the economist that he was. It is ironic that Olson should receive so much attention for a book that is chiefly a contribution in political science and one that is both brilliant and dubious at the same time.

everywhere. Moreover, there is a tendency for CEOs to become increasingly self-assured as they continue in office. This sometimes yields conflicts with boards or directors and, at other times, may lead boards to be acquiescent. But where is the norm in practice? (See Newman 1996.) If one adopts the "bargaining and command" model from the public sector, what would the significant differences be?²⁶

One would expect (predict) the CEO function to be affected by changes in the technological, demographic, and cultural environment of the firm. Technology arguably is a factor altering the role. The greater reliance on information technology creates another seat at some corporate executive committees for a chief information officer (CIO), but I do not know exactly how that works. Nor does anyone else.

Legal requirements, accounting requirements, and investor expectations affect the relationship of the CEO to the chief financial officer (CFO). The CEO function should be somewhat restricted, one would predict, by the growing demand by others in the investment world for better information on finances and internal management. This demand, in turn, leads to an additional demand that the CEO accept an independent audit committee in the board of directors. The audit committee chair is the only person with authority to ask questions directly of any corporate officer, without the clearance of the CEO. Indeed, in the United States, the chair might be legally liable if s/he sought such clearance. Different legal environments in some other countries, such as the United Kingdom, would also be relevant.

Industry-Level Relationships. An industry is not only a set of "economic" transactions but also a functioning political system in which people have ongoing relationships (Hamilton 1957, 6). When a large manufacturer is short of the product that the market demands, it can ration by price, or it can tell steady customers that it will deliver to each some proportion ("allocation") of what was ordered until production increases. This procedure could easily be understood by anyone who deals with the normal realms of politics.

In economic organization, one political dimension is the constant tendency toward oligopoly at the industry level, the manner in which some firms acquire chokeholds by dominating some segment of the industry (such as smelting in the copper industry). The financial community is a notable place for such examination, and we could have some evaluation of which firms provide leadership in the financial community to a greater degree than others and the structuring of syndicates (other than the objectively measurable considerations and alliance patterns within the financial community). Those who raise money, those who certify value, and those who attest to legal validity surely are involved in some dynamic political exchange.

The Larger Arena. There are, of course, questions of public politics and public policy as well as their effects on the internal governance of the corporation. Such rules are interpreted and changed, in the functioning market economy, to protect or advance "high-risk" projects that the pure free market would never sustain. Three examples are the civilian nuclear power program, the proposed Alaska Natural Gas Transportation System, and the synthetic fuels program, all adopted by Congress during the second half of the Carter administration.

Geoffrey Miller (1998), a lawyer, recently published an article in a corporate governance volume sponsored by the Sloan Foundation. The abstract states that it is harder to carry out a takeover in the United States than in the United Kingdom for two reasons. First, there is more room for antitakeover forces in state governments, whereas the centralized U.K. system squeezes those people out. Second, legal doctrine in the United States allows lawyers to make money out of takeovers, and thus "creates a strong interest group within the organized bar" (p. 629).

There is opportunity, and need, for inquiry into the politics of creating and sustaining different kinds of markets. Markets cannot exist without a definition of "mine and thine" and of the conditions under which "thine" becomes "mine." Such rules, if not derived from sheer tradition, come by application of reason, but some are ultimately products of power. Markets, except in the most primitive sense, exist not in nature but in the rules (which is to say political choices) that human beings establish (Douglas 1962, 211–33).

What may be even more fundamental is the political conditions under which rules are changed to facilitate free markets. There still remains the important issue of how, in the political sense, the market system is created or sustained. How do we explain changes that create markets more favorable to "economically correct" results, since such changes necessarily impose at least short-term penalties upon many people?²⁷

The transformation of England from a rigidly regulated market at the time of Adam Smith to a more or less free market seventy years later, at the time of Corn Laws repeal, warrants study. So does the problem of land ownership in a country such as Nigeria, where individual title is not the norm. Suffice it to say that a host of questions—including creation of new categories of gainers and of losers—can be asked about in the former socialist countries. Perhaps something of this kind is occurring in American debates over the restructuring of the electric power industry (Holden 1995).

These four sets of problems—public administration, the nature of interests, the politics of urbanization, and the interpenetration of politics and economics—refer to substantive matters where the exercise of power is displayed. The methodologies that are appropriate may be "hard" or "soft," but these are matters with which young scholars should be concerned as they

²⁶ Cyrus H. Holley, with whom I served on a corporate board for several years, read an article on this subject and said: "Oh, yes, I could apply that in my own company."

²⁷ This general concept now takes operational reality in the experience of the former Soviet Union.

pursue their calling. We seek to enhance competence (capability) in order to achieve and maintain competence (domain).

UNORTHODOX AREAS

In the creation and maintenance of competence, we may, and I believe should, teach the need for coherence and direction, for strategic inquiry into the "unorthodox" portion of Merriam's formulation. These are all matters that, as students of power, we might fit into a coherent whole, and they are all matters that the young might be facilitated to study. But here I offer only a brief thought on force, passive resistance, and noncooperation.

As to where we should place high priority, I restate the view that politics is not merely a debate or a game, it is often a fight. From a moral point of view, the game is preferable to the debate, and the debate preferable to the fight, assuming the participants are on some equality with one another. From what we can observe and what has been recorded, however, that is far from the normal situation. This should be set against the concept of information as a political resource. Insofar as politics can be mainly a game, or possibly even a debate, there is great value in information. Existing and emerging empirical democratic theory is focused on information as a political resource. But politics is not always a process of persuasion or bargaining but of coercion (Pennock and Chapman 1972). I say nothing here of passive resistance, having given it too little thought. There may be colleagues who would prefer, on scientific or moral grounds, to begin with passive resistance and noncooperation rather than force. Force was where Merriam began, although he was aware of Gandhi.

THE OXENSTIERNA-MULLINS EFFECT

When the term "decision making" became an important political science concept, we also began to talk about rationality. Perhaps we borrowed too much (Holden 1996), one evidence of "too much" being the persistent belief that synoptic rationality can and should govern decision making. Vertzberger (1998), in a study of military interventions, argues that this principle is still believed by decision analysts. He thinks it should be rejected, which is why he undertook the study. Vertzberger (p. 407) thus urges that people making decisions know "the nature of the information" they are provided and know whether disagreements in risk assessment are "embedded in different informational bases" or are "judgmental" (different interpretations of the same information). The argument is persuasive, but the solution that decision makers should be trained to do this seems far from realistic. Can the world be stabilized so that heads of government can undergo such training? Can anyone fulfill the requirements of the challenge specified?

The problem is serious, and we can see many disastrous decisions in all walks of life. In general, those who do not think synoptic rationality is real have chosen to

accept "bounded rationality," which basically says that our limited thinking ability is what hinders us. The bounded rationality idea is useful, but it breaks down at some point. It does not show what I have named the Oxenstierna-Mullins effect.

Axel Oxenstierna was chancellor of Sweden under Gustavus Adolphus, during the Thirty Years' War (1618–48) (Wedgewood 1961). On one occasion he wrote his son: "My son, my son, if you knew with what little wisdom the world is ruled" (Myrdal 1965, 20). Moon Mullins was not so elegiac but equally forceful: "Results is what you expects and consequences is what you gits."

People often expect results and get consequences by virtue of their own bad decisions and the dynamics those decisions produce. The enemies of Julius Caesar destroyed him, the republic, and many of themselves. The ruling elites of Germany, Austria, and Russia blindly moved their countries into World War I, not without calculation but without much detailed advance calculation. They were not merely displaced as rulers but destroyed as elites and sometimes as persons. The Japanese elite before Pearl Harbor trembled, as Masao Maruyama (1969, 89) wrote, "at the possibility of failure, [but] still thrust their way forward with their hands over their eyes." The result was imperfect rationality to the extent of pathology.

Force is a crucial element in both domestic and international politics. Crucial does not mean "good," necessarily. Force often leads to one aspect that nearly all social observers undervalue. Merriam (1926, 5) said "I have sometimes thought it would be worthwhile to write a history of political unreason, folly, and prejudice, in order to balance sundry discourses on political theory, and offset the possible conclusion from them that all political action is likely to follow the lines of thought indicated by the great masters of systematic political speculation." The use of force is one situation in which people frustrate the future they wish to create by failures or serious imperfections in rationality. Sometimes, they create situations of foolishness, selfdefeat, or pathology. The examples refer to very highlevel decision makers, but the Oxenstierna-Mullins effect applies to low situations. What rationality requires a police officer severely to beat a prisoner, already under arrest and inside the confines of the police station?

THE OUTSIDE WORLD

The competence of political science is enhanced to the degree that we can reach into the real world beyond the graduate seminar, the journal, and the university press book. When there is an important change, we should be altered.

Opportunity to engage the larger world always exists, and sometimes that opportunity may be unwanted. People often expect a political scientist to have some knowledge or competence about a variety of governmental relationships and problems, which Elinor Ostrom (1998) has called collective action problems. When we are unable to take into account those needs,

the social options go to others. Engagement in the world beyond the academy is often intellectually valuable and professionally compatible. Two fields of political science do make practice claims, namely, public administration and international relations. (Although some colleagues must work in a language that most practical decision makers cannot use, there is no reason to discourage other colleagues who aspire to speak in the language of practice.) David Newsom, the recent Cumming Professor of Diplomacy at the University of Virginia, is a constant advocate of the view that contemporary publications are generally too remote to be used by people who are engaged in foreign policy.28

Even when the language is standard English rather than mathematics, there is an additional impediment to understanding of those who write in scholarly terms. We cannot assume that the meaning of political science concepts will be obvious to practitioners. In part, this is just a question of semantics. The same word will often have different significance to scholars and to practitioners. But it is not just words. It will often happen that the problem perceived by one is invisible to the other and that the need for rapidity of decision will not seem the same to each. Political scientists may need at times to find some entry into the practice, for it is only in the world of practice that they will understand, and be understood by, practitioners. This is perhaps the same as the case that political scientists need to develop some equivalent of engineering, though that is an imprecise idea (Ordeshook 1996, 175-88, esp. 180-1 and 187-8; see also Dennison 1932).29

Participant and/or advisory roles offer entry for political science into some worlds of practice. We know of political theorists (such as Machiavelli and Locke) who have held political posts, whether minor or major. But we need not go so far back. The earliest American example I can cite is Francis Lieber, who had a background in international law and finally persuaded the Army to let him write about the international law of land warfare in the Civil War period. The result was General Orders 100, establishing conduct of armies in the field, which students of international law say has had wide influence elsewhere (Freidel 1947, 336-40; Nys 1947, 355–93).

The advisory role has been adopted by a large number of our colleagues in one form or another. In the local government setting, for instance, the Ameri-

can political scientist who has the inclination is sure to be invited to participate in something. From there, nature will take its course. I am not certain this is so in other countries.30 In most states, the flagship public university (and sometimes other public universities) has a research agency that conducts practical research on such questions as the incidence of taxation in the state, the organization of local government, the state legislative process, and other relatively practical matters. Political scientists, and other scholars, who work in these entities sometimes develop intense knowledge of the state government and themselves become major figures in state affairs. In general, a fairly severe separation operates, so that the work such colleagues do seldom flows back into the main body of intellectual discussion. The young political scientist can be at some professional peril due to outside engagement. One result is that young people who have strong interests outside the academy simply leave because they are unlikely to meet all the internal requirements for promotion and tenure.

The bad result, intellectually, is that their experience is not often fed back into the political science analytical process. It is intellectually counterproductive for external success to preclude feedback into the political science community. The rigidity in our collective decision rules means that other realms-management science, decision theory, schools of public affairs, schools of public administration, schools of international relations-preempt a good deal of the work that we might undertake. This rigidity is not ordained by nature. It is a function of how our organizational realm works. It is within our capacity to alter the rules and often a major intellectual (thinking) issue is contained in the practical issue. What is the explanatory theory in political science that will offer some useful guidance to realistic action? This is an idea that it is imperative to accept, if students and ambitious young scholars are to be free of the terror that "practicality" will render them "inferior." Tenure, salary, and prestige, as Ordeshook (1996, 188) rightly points out, cannot be withheld from people who allocate their time and energy to such work.

The last years of the twentieth century are not the first time we have faced this issue. Merriam's reference to "legislation" was not principally to studies of the combinations and adjustments in lobbying, committee fights, and floor fights. When he says that "significant advances were made by . . . McCarthy, whose untimely death caused irreparable injury" (Merriam 1926, 2), he is referring to Charles McCarthy, who founded the legislative research organization in Wisconsin.

McCarthy soon came into the initial group that called the meeting to create the American Political Science Association. Their interest in "comparative legislation" was in material on the form and content of legislation from one country to another, so that drafters and legislators could make use of them. This practical side of the creation of the American Political Science Association itself is lost knowledge now, but it

²⁸ Newsom was a career Foreign Service officer who retired at the level of Undersecretary of State. A similar complaint is expressed in a letter to the president and president-elect of the association by Dr. Lloyd Etheredge.

²⁹ Harold Guetzkow expressed such an idea in the 1950s when I was a graduate student, although I do not know whether he ever gave it formal published form.

³⁰ Anthony King has (in personal conversation) told me that this kind of engagement is less likely in the United Kingdom than in the United States. I am, however, aware that such a colleague as L.J.

Sharpe (Nuffield College) was a member of the Oxford City Council, although whether this is a function of the special importance of Oxford University in Oxford itself is a question. Moshe Shani, when my temporary colleague some years ago, said that such engagement was much more common in Israel, perhaps as a small country with intense demands for highly educated competence and a strong claim on the moral engagement of its people.

cannot have been lost in 1925. Most of the original participants were still active, although Merriam himself was a subsequent member. McCarthy's short professional career (twenty years at the outside) illustrates the remarkable confluence of chance, personality, and structural forces in society.³¹

Prior to World War I, the APSA was persuaded to create a Committee on Practical Training for Public Service (first called the Committee on Laboratory Methods in Political Science), chaired by McCarthy.³² McCarthy felt that any University of Wisconsin faculty member ought to be available to help the legislature at request. This was the German system, according to McCarthy. He preferred it. Moreover, Fitzpatrick (1944) said, there was one annual report of the American Political Science Association saying that a time of public service should be a prerequisite to a Ph.D. It is not surprising that idea was defeated.

Avoidance of public engagement may be warranted on the grounds that one's freedom will be compromised. One certainly cannot speak as freely if one knows something, for there will be occasions when discretion is necessary. There is almost nothing to be said for avoidance on the basis of developing or maintaining intellectual competence.³³

We have an opportunity to enter more seriously into the popular culture, but we have to want to do so. Political science is where thoughtful people go, once they can no longer depend on their own common sense and on political journalists. It is where political journalists themselves go for interpretation deeper than they can get from "inside" sources. Political science shares its domain (area of competence) with history, law, and philosophy, with sociology, psychology, and anthropology. It may be surrendering vast portions to economics, as it once surrendered a great deal of the field of administration to engineering, business, management science, and others.

Political science has to compete with common sense political theory communicated principally in the general culture. It is structured by education from kindergarten through the twelfth grade. There is also the political theory conveyed in mass media. Executives are trained in business school political theory, although it is generally not conceived as "political" and often is not described as "theory." It is both.

"Judicio-political theory [is] enunciated in law class rooms, law reviews, reformulated in lawyers' briefs, and authoritatively expressed in judicial decisions" (Holden 1996), but is in need of some challenge. That means the immediate frontier may be legal education. Political

immediate frontier may be legal education. Political

31 Charles McCarthy, a young man from Brockton, MA, had gotten
a Ph.D. in history from Wisconsin in 1907 (Fitzpatrick 1944).

32 Lowell (1910, 11) also speaks respectfully of "the bureau of legislation at Madison," but he seems to have thought of it as a

science already has, in its present content, a great deal to contribute to the education of lawyers, whose briefs will frame issues for judges, and of judges, who will decide the issues. The education of lawyers will be altered if law students and law professors debate political science.

Progress in political research also will call for some new thinking about interdisciplinarity. Merriam (1926, 8) was somewhat aware of that issue, as suggested by his list of basic problems, which would require combined efforts to resolve: "punishment and prevention of crime, alcoholism, the vexed question of human migration, the relations of the Negro, and a wide variety of agricultural and industrial problems." This would now be framed differently. Social scientists would likely speak of "the status of African Americans" or "the role of the blacks," or retreat to some other language about "color-blind" policies or "affirmative action," depending upon their ideological commitment. Little appears to be written on the assumption that the pattern of white-black transactions has any important connection to the viability of the United States as a political system. African Americans and European Americans, as groups, coexist as if they were two "nations" within one governmental authority. This calls for serious political analysis, not moral analysis (Holden 1999). The white-black relationship is not as intractable as that between the opposing sides in Northern Ireland, but it is far more so than most people appear to suppose. There is no more important social science problem, with ramifications for human societies around the world, than developing a more fundamental concept of how to create peace between large groups between whom there is a diffused hostility.

The National Science Foundation's (NSF) leader-ship has recently emphasized interdisciplinary research on major public problems. Political scientists might note, for instance, the term "biocomplexity," used by Dr. Rita Colwell, who believes "it will take biologists, computer scientists, engineers, and surely those in the behavioral sciences to understand the signals for survivability." She added that "no problems exist in isolation, whether they are scientific, social, or technical. More often, they are all three at once." NSF's Assistant Director for Social and Behavioral Sciences Bennett Bertenthal even spoke in public of a "Manhattan Project" approach to major problems and urged the social sciences to prepare to take a role so that they would not be overrun by the other sciences.

Human heredity and nonhuman environment have been the outer limits of politics. Contemporary biology raises issues about the heredity parameter that make it appear far less rigid than once thought. A similar point should be made about the biogenetic engineering of crops, which has already raised regulatory process issues in Europe (Barboza 1999).

means to produce more accurate statistics for political scientists. ³³ Indeed, John Maynard Keynes was never the pure academician. Of the first thirteen years of his professional life, six were spent as a titular civil servant, and at least four on very active duty. When he returned to Cambridge, he spent part of his time in London and engaged in public affairs for the rest of his life. The details are given in his obituary (Harris 1947, xviii).

²⁴ Address to the American Institute of the Biological Sciences, August 1998.

CONCLUSION

I have suggested what young scholars should be encouraged to pursue in the intellectual realm and how the organizational realm might be adapted to the highest improvement of the intellectual realm.

First, political science has a certain competence in that its domain is the study of politics (the organization of power), and about that domain it has competence. It is important always to seek a means to contribute to science, but certain disciplines are more or less capable in some terms or certain areas than are others. At any moment, however, a body of scholars in a given line of work can do what they can and can make very serious efforts to do new things better.

Second, Merriam set forth a number of ideas, some of which have been substantially fulfilled, and some of which have not. The most significant change has occurred in "the field of method." Quantitative and/or mathematical analysis is so pervasive that I think it worthwhile to speak of its triumph. The triumph has yielded good results, but it also has had very important side effects that diminish competence.

Third, the enhancement of competence, which is always our valid purpose, is likely to be served by coherent and strategic focus on some areas identified in the Merriam program that now are considered conventional but lend themselves to further development. The strategic areas have to do with the exercise of power and lie in the fields of public administration, political interests, the politics of urbanization, and the interpenetration of politics and economics.

Fourth, the Merriam formulation also lends itself to serious inquiry in unorthodox subjects, which are not necessarily part of the conventional agenda: (1) force, passive resistance, and noncooperation, (2) the nature of political interests, (3) magic, superstition, and ceremonialism, (4) propaganda, (5) conference, (6) maintenance of morale, (7) leadership, obedience, and cooperation, and (8) the causes of war.

I also place some emphasis on political science inquiry into force and foolish, irrational, and pathological decision making (the Oxenstierna-Mullins effect). Force (and its opposites) has a certain importance, particularly if one does not operate with a theory in which information is the central political variable. In this respect, I depart somewhat from the norm.

Finally, a component of competence that I extract from the Merriam formulation is that of extending the reach of political science into the world beyond the graduate seminar, the journal, and the university press. This means that our decision rules—ultimately expressed in pay and prestige and made or implemented by deans, chairs, promotion and tenure committees, and journal editors in the aggregate—should be altered to allow a higher degree of external engagement. This external search is not simply to further career opportunities; practical problems involve issues that evoke the most serious theoretical effort. That, in a certain way, is the ultimate indicator of the competence of political science.

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Improving Quantitative Studies of International Conflict: A Conjecture

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The address a well-known but infrequently discussed problem in the quantitative study of international conflict: Despite immense data collections, prestigious journals, and sophisticated analyses, empirical findings in the literature on international conflict are often unsatisfying. Many statistical results change from article to article and specification to specification. Accurate forecasts are nonexistent. In this article we offer a conjecture about one source of this problem: The causes of conflict, theorized to be important but often found to be small or ephemeral, are indeed tiny for the vast majority of dyads, but they are large, stable, and replicable wherever the ex ante probability of conflict is large. This simple idea has an unexpectedly rich array of observable implications, all consistent with the literature. We directly test our conjecture by formulating a statistical model that includes its critical features. Our approach, a version of a "neural network" model, uncovers some interesting structural features of international conflict and, as one evaluative measure, forecasts substantially better than any previous effort. Moreover, this improvement comes at little cost, and it is easy to evaluate whether the model is a statistical improvement over the simpler models commonly used.

espite immense data collections, prestigious journals, and sophisticated analyses, empirical findings in the quantitative literature on international conflict are frequently unsatisfying. Statistical results appear to change from article to article and specification to specification. Any relationships usually are statistically weak, with wide confidence intervals, and they vary considerably with small changes in specification, index construction, and choice of data frame.¹

Instead of uncovering new, durable, systematic patterns, as is the case in most other quantitative subfields of political science (and public health, of which this field is also a part [King and Murray 2000]), students of international conflict are left wrestling with their data to eke out something they can label a finding. As a consequence, those with deep qualitative knowledge of

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¹ It is not our intention to provide a literature review. Examples of the fragility of findings and sensitivity of exact specification may be found in Thompson and Tucker's (1997) exchange with Farber and Gowa (1997) and Mansfield and Snyder (1997) on the role of democracy in preventing conflict, or in Oneal and Russett (1997), Barbieri (1996), and Beck, Katz, and Tucker (1998) on the role of trade in preventing conflict. We stress that our examples involve current best practice, so the problems we mention are not the result of data errors or simple methodological mistakes.

the subject are rarely persuaded by conclusions from quantitative works (see Bueno de Mesquita 1981; Geller and Singer 1998; Levy 1989; Rosenau 1976; Vasquez 1993). The field has a number of important successes to its credit, such as the democratic peace, and many important and very promising research programs, but the discontinuity between the beliefs of most experts and many quantitative results remains.

A symptom of the fragility of the various models is their poor forecasting performance. To our knowledge, no legitimate statistical model (using annual data) has ever forecast an international conflict with greater than 0.50 probability, and certainly none has done so while also being correct. Political scientists have long eschewed forecasting in favor of an emphasis on causal explanation, and it is clear that any proposed new method must allow for causal interpretation as well as improved forecasting. But causal theories are considerably harder to verify than forecasts, and forecasts have the advantage of being observable implications of the same theories as the causal hypotheses. This means that accurate forecasts can be used at least in part to verify claims about causal structure. In particular, a claim to have found a causal explanation that is a structural feature of the world but changes unpredictably over time (and so is of no help in forecasting) is of dubious validity and marginal value.

Although political scientists are less likely to evaluate models by their forecasting properties than are, say, economists, forecasting properties underlie all evaluations of the performance of statistical models. For example, all likelihood or goodness-of-fit assessment procedures merely ask whether one specification has superior in-sample "forecasting" properties. Yet, all statistical analysts must be concerned about whether they are taking advantage of some idiosyncratic features of the data to improve fit at the expense of detecting structure and hence out-of-sample performance. To guard against this problem, which is more of a worry with powerful statistical models, out-of-sample

forecast accuracy is generally considered the gold standard for model assessment. Even if we have no interest in prediction per se, the poor forecasting performance of standard models indicates that we can improve on them to provide better knowledge of real-world causal relationships. Forecasting is thus of critical, albeit indirect, interest as a key test of whether we have really found causal structure.²

The scholarly attacks on this problem have come from every angle. The most venerable tradition has been to improve the data and measures of international conflict and its correlates (Jones, Bremer, and Singer 1996). Others have modified existing statistical models to accommodate some of the special features of conflict data (Beck and Tucker 1998; King 1989; King and Zeng 1999). Yet others have attempted to derive statistical models from formally stated rational choice theories based on the motivations of states, political leaders, or other domestic actors (Signorino 1999; Smith 1998). We believe progress will ultimately require advances on all three fronts as well as a convergence in theoretical and statistical models and data.

Our approach is based on the belief that a portion of the problem lies in a somewhat overlooked but key substantive issue that is reflected in the mismatch between available data and the set of statistical methods commonly applied. International conflict is a rare event, and the processes that drive it where it is more common are likely to be very different from those elsewhere. As a result, many qualitative researchers expect the relationships to be highly nonlinear, massively interactive, and heavily context dependent or contingent. Because these characteristics would be missed with standard statistical approaches, particularly the typical linear-normal models imported from studies of American politics, we adopt a form of the highly flexible "neural network model." This type of model is well suited to data with complex, nonlinear, and contingent relationships. It is not a panacea or always appropriate, but it provides an immensely useful tool that has not been sufficiently exploited in this field to date.

As an analogy, consider the use of survey data to assess the consequences of lifestyle on the onset of a relatively rare disease. A supposedly healthful lifestyle may be helpful in warding off the disease for everyone, but a simple logit of disease on lifestyle (and other explanatory variables) would, at best, show a weak relationship, since most survey respondents are unlikely to get the disease in any event. Thus, the logit analysis of disease, like a logit of conflict, will average many small effects with a few large ones. If we could

predict that behavioral changes would have a major effect on avoiding disease for those who are at greater risk for it, this would be a great accomplishment. Similarly, if we could show that autocracy dramatically increases the likelihood of conflict for dyads at risk of conflict, this also would be important. In short, we conjecture that many quantitative international conflict studies lack robustness because they look only for the effects of variables averaged over all dyads, whereas in reality the effects vary enormously over dyads and are only substantively large for those already at relatively high risk of conflict.

We find that our models are able to predict international conflict to a degree. Whereas all previous models are unable to predict the occurrence of any conflict, our out-of-sample forecasts pick up about 17% of these disputes. There is still a long way to go to produce highly accurate forecasts of all these rare and unusual events, if that is ever even possible, but our analyses confirm that there is structure in these data. As for causal interpretation of the underlying structure, our more appropriate techniques find hints of robust and replicable patterns.

In the next section, we propose a simple conjecture that seeks to remedy some of the problems found in the quantitative analysis of conflict. We believe the idea may explain some of the anomalies and nonfindings in the literature and why our model is able to forecast reasonably well. It also highlights the features that an appropriate method would need to uncover stable patterns in this field. We then discuss appropriate methods, apply them to real data, and offer conclusions. The Appendix contains some technical details on Bayesian neural network models.

THE PROBLEM WITH CONFLICT STUDIES

A Conjecture

Our conjecture about conflict studies is quite simple, and aspects of it are implied in much of the literature. The idea is that the effects of most explanatory variables are undetectably small for the vast majority of dyads, but they are large, stable, and replicable when the ex ante probability of conflict is large. For example, Swaziland and St. Lucia have essentially no chance of going to war today. Should either become slightly less democratic, conceivably the probability of war would increase a bit, but the increase would be so small that it would be undetectable and unimportant. In contrast, if Iran and Iraq were to become slightly more democratic, then the probability that they would go to war might drop dramatically. If our conjecture is right, then the effects of the causes of conflict differ by dyad, with trivially small effects for the vast majority and larger effects for a few. That is, it is the effects of the explanatory variables, not merely the levels of the variables, that vary.

To be clear, this conjecture addresses the large literature on country-year dyads. Different processes and theories might apply to data based on daily events series or other more finely grained data collections. We

² Of course, accurate forecasts of international conflict would be of tremendous practical value, as a large portion of the foreign policy bureaucracy in many countries is devoted to this task. A quantitative "expert system" to help guide policymakers could be of considerable use. Forecasts of political conflict also would be of interest to political-business risk analysts, public health researchers, and many others. In informal discussions, several former U.S. policymakers indicated that, aside from occasional commissioned studies, no such quantitative expert system is presently in use. To our surprise, they also indicated that even annual forecasts would be of considerable use in policymaking.

do believe that our simple conjecture may help explain diverse features of the quantitative literature on the causes of international conflict. At the least, it appears to be consistent with several observable implications.

First, most scholars use statistical procedures that assume the effects of their explanatory variables are nearly the same for all dyads. (Some use interaction effects that allow more variation over the dyads, but the degree of variation represented is still quite limited.) The estimates these analyses produce are roughly the average of essentially zero effects for the vast majority of observations and larger effects for a tiny fraction of the cases. Unless the effect is enormous in the small set of dyads with a high ex ante probability of war, estimates from most analyses will appear very small or resemble random noise. Indeed, small to nonexistent and highly variable effects are dominant in the literature.

Second, when effects are huge in the dyads with high ex ante probability of war, the average over all the dyads is large enough to be detected reliably with most methods. (Nonetheless, the estimated effect would be too large for most dyads and far too small for a few.) Some uncontroversial variables in this at-risk subset of dyads include contiguity and time since the last war, indeed, these are among the few variables that often give reasonably robust results across specifications.

Third, if only a few observations have large effects, then small changes in the set of dyads included in a statistical analysis will sometimes have disproportionate effects on the results. This also appears true and may account for some of the apparent instability of results in the literature from article to article.

Fourth, a similar observable implication results from the strong priors most scholars derive from their considerable qualitative knowledge about the field. What can we expect to find when strong priors are combined with statistical methods that assume causal effects are the same for all dyads and when data have a low signal-to-noise ratio? We expect researchers to push their data analyses extremely hard in search of effects they believe are there but are difficult to find. Unfortunately, this would make the results differ from investigator to investigator, just as they seem to, since answers will depend very sensitively on otherwise minor coding decisions.

Fifth, some scholars make coding decisions that seem consistent with our conjecture when they discard all dyads but those deemed "politically relevant" or "at risk," in other words, those with a high ex ante probability of war.³ If our hypothesis is correct, then these coding decisions are problematic methodologically. Such problems are often recognized by the authors, who have little choice but to put some restrictions on an otherwise endless data collection. The difficulty is that coding rules amount to dropping many cases without war and a few with war, which in some instances may generate two types of selection bias. (1) Intentional selection may bias the effect upward if the

relevant population to which one is inferring is all dyads and otherwise may correctly increase the effect. (2) Except when the definition of "politically relevant" is clearly based on one of the explanatory variables (e.g., Maoz 1996), these rules also select on the dependent variable, which biases estimates of the probability of conflict. Whatever the goal of the inference, studies that address the problem by selecting cases in this way may give answers that are too small or too noisy, which appears to be the case throughout the literature. The results from these selection rules will be somewhat stronger than when using the entire data set but not as large as qualitative experts expect. Indeed, this seems to fit the literature.

Finally, if our conjecture is right, then the application of an appropriate statistical technique will confirm the existence of sizable and robust effects in the high exante probability of war dyads and tiny effects elsewhere. For many variables at least, the direction, magnitude, and nature of the large effects should not be wildly inconsistent with our qualitative knowledge of international relations, unless there is a clear reason. If this method indeed finds real features of the international system, rather than some idiosyncratic sample characteristics that result from our specification choices or coding rules, then out-of-sample forecasts ought to predict similar patterns in the next data set.

The first five of these observable implications of our conjecture are consistent with observations from the literature. Testing the sixth will occupy most of the rest of this article.

According to our idea, international conflict data differ from other rare events data sets in two ways. The effect of any single explanatory variable changes markedly as a function of changes in the other explanatory variables (that is, there are strong and complicated interactions), and the dependent variables are, in principle, powerful enough to predict whether conflict occurs if the appropriate model is used. Some other rare events data, such as in epidemiological studies of disease, may fit this description, but only if all these characteristics apply.⁴

STATISTICAL MODELS OF INTERNATIONAL CONFLICT

Relative to other types of data and processes studied in political science, international conflict data have some unusual characteristics. They are based on thousands of dyads (combinations of countries taken pairwise) or directed dyads (the actions of A toward B and of B toward A are separate observations in the same data set). Whether the universe of dyads should include only originators of conflict, all nations, or some group in between is by no means clear. Most outcome variables

³ See Bremer 1992 for this argument in its modern statistical guise and Richardson 1960 for some early intuition.

⁴ The leading alternative explanation for the problems in this field as that the data are so bad that it is impossible to discover patterns. Data problems clearly exist (e.g., Vasquez 1993), and may be more severe than in other fields, but they are not unique to international relations.

are dichotomous. The data often concern rare events, with hundreds of times more 0's (peace) than 1's (conflict). Often the explanatory variables are neither dichotomous nor quite continuous, with distributions that are asymmetric or with multiple mass points (such as at the end or midpoint). The indices often are necessarily complicated combinations of diverse measures. In addition, assuming our conjecture is correct, very small parts of very large data sets contain most of the interesting information.

The statistical method we introduce here is a version of a neural network model, first introduced to political scientists by Schrodt (1995) and Zeng (1999, 2000).5 There is an immense literature supporting this technique in engineering, computer science, statistics, psychology, linguistics, neuroscience, medicine, finance, and other fields. Neural network analysts have adopted an extensive and essentially unique terminology. The language is useful because it helps emphasize the rough analogies between these statistical models and some theoretical models of the way human brains may work. Using this language to describe concepts known to political scientists by other names can be counterproductive. Therefore, we introduce these models as straightforward generalizations of logistic models, which are the most commonly used statistical models of international conflict.

In the following subsections, we make the transition from logit to our neural network models. We examine various issues related to estimation, interpretation, and inference with our model, and we introduce the main ideas of Bayesian methods for neural networks adopted here. We also discuss several potential objections to neural networks.

From Logit to Neural Networks

Our dependent variable, Y_i , takes on a value of 1 if dyad i (i = 1, ..., N) is engaged in an international conflict, 0 if it is at peace. If conflict and peace are coded as mutually exclusive and exhaustive (which we make true by definition), then a Bernoulli distribution fully describes this variable. The only parameter of a Bernoulli distribution is π_i , the probability of an international conflict. Let a vector of a constant term and k explanatory variables be denoted $X_i = \{1, X_{1i}, X_{2i}, ..., X_{kl}\}$.

The next step is to specify the relationship between π_i and X_i . The simplest possibility is a linear function, which results in what is known as the linear probability model:

$$Y_{i} \sim \text{Bernoulli}(\pi_{i}),$$

$$\pi_i = X_i \beta = \beta_0 + \beta_1 X_{1i} + \cdots + \beta_k X_{ki}, \qquad (1)$$

where $X_i\beta$ is merely a matrix expression for the linear relationship between π_i and X_i ,

$$\pi_i = \operatorname{linear}(X_i),$$

and the $(k+1) \times 1$ vector β includes a constant term and k weights (or coefficients) on each of the k explanatory variables. The problem with the linear probability model is that it can generate impossible values of π_i (greater than 1 or less than 0), so even values within the correct range near the boundaries are questionable. Moreover, the full posterior distribution for π_i generated by the linear probability model never makes sense in any application because at least some density always falls outside the unit interval. These problems were known long ago, and the linear model was almost entirely supplanted by logit models when these became computationally feasible.

The logit model is similar to the linear probability model except for the functional form:

$$Y_i \sim \text{Bernoulli}(\pi_i),$$

$$\pi_i = \operatorname{logit}(X_i \beta) = \frac{1}{1 + e^{-X_i \beta}}.$$
 (2)

The logit model maps the linear functional form $X_i\beta$, which can take on any value, into the [0,1] interval required for π_i by applying the logit function. The vast majority of analyses in conflict studies use some form of this method. For our purposes, it is important to recognize that the second line of equation 2 specifies the underlying probability of conflict, π_i , as a logit function of a linear function of X_i ,

$$\pi_i = \operatorname{logit}(\operatorname{linear}(X_i)).$$

The logit is thus a generalization of the linear probability model, created by adding an extra level of hierarchy. Our neural network model will generalize the logit by adding an additional level of hierarchy.

The logistic model in equation 2 improves on simple linear probability models by avoiding impossible probability values and assuming a more plausible relationship between the explanatory variables and the probability of an outcome. The effect of each explanatory variable varies across observations, however, and depends on the values of other explanatory variables only slightly more than does the linear model. These effects also depend only trivially on the ex ante probability of conflict (see Nagler 1991). One way to look at changes in the effects of explanatory variables is to examine the derivative of the probability π , with respect to one of the explanatory variables, say, X_{1i} . For linear models this derivative is β_1 , which is obviously constant. For logit models, the derivative is $\pi_i(1-\pi_i)\beta_1$, which is better. But since π , is within a small range above zero for all but a few observations (and, given the logit model's inflexibility, virtually all observations in practice), this is a highly restrictive and nearly constant

⁵ General references on neural networks include Rumelhart et al. (1986), Muller and Reinhardt (1990), and Hertz, Krogh, and Palmer (1991). Detailed discussion of neural networks as statistical models can be found in, for example, Bishop (1995), Cheng and Titterington (1994), Kuan and White (1994), Ripley (1996), and White (1992). A recent application in political science is Eisinga, Frances, and Van Dijk (1998), who estimate a constrained neural network model similar to a generalized additive model. While this application is completely different from our conjecture, it does show the great flexibility of neural nets and their applicability to many complicated data problems.

specification. To avoid this weakness, two different types of generalizations might be considered, either of which would be an improvement, but neither of which is sufficient.

First, we might specify a random effects model. Instead of leaving β fixed at one set of values, as in equation 2, we could let it vary randomly over the observations in some form, such as $\beta_i = \beta^* + \epsilon_i$. Although some assumption about randomness may be better than logit's more restrictive assumption of constant effects, our conjecture is not that the coefficients vary randomly across dyads; rather, they vary systematically with the ex ante probability of conflict as a function of complicated conjunctions of all the explanatory variables.

Second, standard interaction effects can be applied. For example, we might let the effect β_d of democracy X_{di} be a function of whether the states in the dyad are contiguous X_{ci} , by specifying, say, $\beta_{di} = \gamma_0 + \gamma_c X_{ci}$. Substituting this expression back into the second line of equation 2 produces an interaction effect that is easy to estimate: Merely include $X_{dr}X_{cr}$ as an additional variable in X_i and use any standard logit package. This strategy will work in some cases, but it requires a good deal of prior knowledge of the types of interactions to specify. If one does not have sufficiently detailed knowledge, then the number of interactions one would include generally would not be precisely estimable with available data. In fact, too many interaction terms can result in severe problems of numerical instability due to colinearity. In conflict data, massive interaction effects are suspected, most of which are concentrated in tiny areas of the parameter space. Standard interaction-based logit models are too restrictive, require too many interactive terms, and do not address the expected degree of nonlinearity.

Our preference for an approach to this problem is the massively interactive and highly nonlinear neural network model, in particular, the single hidden layer feed-forward perceptron. This biological language sounds complicated, but it can be expressed as a statistical model that is a straightforward generalization of the logit model.

Logit models use an "S-shaped" curve to approximate the relationship between the probability, π_l , and the explanatory variables, X_l . Imagine how much better the approximation could be if more than one such curve were used simultaneously, each with a different curvature and orientation; roughly speaking, that is what neural network models allow. In order to approximate the relationship between π_l and X_l with a set of M logit curves, we use a neural network model with the same distribution as but a different functional form from the logit and linear probability models:

$$Y_{t} \sim \text{Bernoulli}(\pi_{t}),$$

$$\pi_{t} = \text{logit}[\gamma_{0} + \gamma_{1} \text{logit}(X_{t}\beta_{(1)}) + \gamma_{2} \text{logit}(X_{t}\beta_{(2)}) + \cdots + \gamma_{M} \text{logit}(X_{t}\beta_{(M)})].$$
(3)

This neural network model is a type of discrete choice model that differs from the logit only in the shape of the curve. It is easiest to compare this relationship with the standard logit in the special case when M=1, since the second and third lines of equation 3 are then just a logit function of a linear function of a logit function of a linear function:

$$\pi_i = \operatorname{logit}(\operatorname{linear}(\operatorname{logit}(\operatorname{linear}(X_i)))).$$

The larger the value chosen for M (known in the literature as the number of "hidden neurons," but of course nothing is hidden), the more logit curves are used at the third level of generalization and, as a result, the larger variety of shapes the entire expression can approximate.

To be more specific, β in the logit model in equation 2 is a $(k + 1) \times 1$ vector of effect parameters (corresponding to a constant term and the weights on the k explanatory variables). After the β 's are multiplied by their respective X_i 's and summed up, logit $(X_i\beta)$ is then one number, which we also label π_i . In contrast, the $\beta_{(1)}, \ldots, \beta_{(M)}$ parameters in the second and third lines of equation 3 are each $(k+1) \times 1$ vectors, so that each expresses a different weighting of the k explanatory variables. Then the logit function is applied to each of the different weighting schemes $X_i\beta_{(1)}, \ldots, X_i\beta_{(M)}$ to yield a set of M numbers: $logit(X_i\beta_{(1)}), \ldots, logit(X_i\beta_{(M)})$. A weighted sum of these M numbers is taken (with the γ 's as adjustable weights in this linear expression), and the logit is taken one final time to make sure that the entire expression yields a number for π_i that is between 0 and 1. The result is, as we shall see below, a remarkably flexible functional form.

Neural networks meet the needs of conflict research because they allow the effect of each explanatory variable to differ markedly over the dyads, as required by our conjecture about international conflict. As in the case of linear probability and logit models, this can be made clear by examining the partial derivatives of π , with respect to the independent variables. After some straightforward calculus, this yields

$$\frac{\partial \pi_{i}}{\partial X_{ik}} = \pi_{i}(1 - \pi_{i}) \sum_{j=1}^{M} \gamma_{j} \beta_{(j)k} \operatorname{logit}(X_{i} \beta_{(j)}) \times [1 - \operatorname{logit}(X_{i} \beta_{(j)})],$$

where h indexes explanatory variable h in X_i and element h in β_j . From this expression, it is clear that our neural network model allows much more range in the marginal effects of independent variables across dyads than do the linear probability or logit models: The effect varies not only with π_i across dyads but also with X_i and all the logit curves. Although each element in the summation is limited in size, the combination of all the terms can produce wide differences.

More generally, what makes neural network models attractive as statistical models is that they provide a class of functional forms that can approximate any hypothetical relationship between π_i and X_i , given a large enough choice for the number of logit functions

M (Hornik 1990; White 1992, 12-28).6 Although the functional form in equation 3 has a fair number of adjustable parameters (M(k + 2) + 1), in comparison to other flexible functions that also have general approximation capabilities, such as those based on polynomial spline or trigonometric functions (e.g., Gallant 1981), neural networks normally require far fewer parameters to model the same level of complexity (Barron 1993). They are often described as occupying a middle range between standard parametric models, with a small number of parameters, and nonparametric models; with almost infinite flexibility (Ripley 1996). Neural networks allow for a wide variability of marginal effects, and their flexibility and general approximation capabilities far outperform standard logitbased interaction models.

Is there a middle ground between the simple logit model and the more complicated neural network approach? One possibility is generalized additive models (GAMs), which are more flexible than logit because they permit nonmonotonicities in the probability of a conflict, but they do not allow relationships as rich as neural networks because most interactions are disallowed (Beck and Jackman 1998). Our extensive experiments with these and other approaches caused us to conclude that only neural network models capture the full nature of the substantive relationships in these data. Of course, one could go even farther, toward more flexible relationships, such as through the use of nonparametric methods. Perhaps future researchers will find some of these techniques appropriate, but to date we have not found that the additional flexibility is warranted. Finally, we do not believe that neural network models should in all cases replace logit models in quantitative studies of international conflict, but our results seem to indicate they have a place in the toolkit of international relations researchers. Further work is needed to build theories based on the structure uncovered, and other research may indeed discover more parsimonious methods.

Issues in Neural Network Modeling

The use of neural networks involves the same issues of model selection, estimation, interpretation, and inference as for any other statistical model. Some of these issues are more common in models with flexible functional forms, like ours, and so demand special attention. For example, in equation 3, how do we choose the size of M? How do we compare results from the different models? For a given model, how are the parameter values determined? How are they inter-

preted? How does statistical inference proceed, and how do we handle uncertainty?

Recent developments in Bayesian methods for neural networks allow disciplined treatment of each of these questions. We adopt the Bayesian framework with normal approximation (MacKay 1992a, 1992b, 1992c, 1994), which is discussed in the Appendix. Here we highlight some of its basic ideas in connection with the questions raised above.

The central feature of the Bayesian approach is to treat everything as probabilistic. Hence, instead of estimating the "true" values of some fixed parameters, we look for their posterior distributions given the data. Model building and inference is then a process of updating our beliefs about the world using the information we receive from empirical data. In the normal approximation framework for neural networks, we assume normal prior distributions for the parameters B and y that are centered on zero; that is, we believe it more likely that the parameters take smaller values than larger ones. This prior merely reflects the common belief that a certain degree of "smoothness" in the underlying data-generating function is likely (smaller parameter values result in smoother functions, since the logit transformation is flattened as the parameter values decrease). We then apply Bayes's theorem to evaluate the posterior distribution of the parameters used to evaluate the predictive distribution of the dependent variable (by integrating out the parameters). For complex functions like neural networks, there are no analytical solutions for the integration, and Monte Carlo methods of sampling the distribution are computationally very burdensome. Hence, we follow MacKay (1994) and approximate various intermediate distributions with normals, since they are analytically easy to treat and simple to sample (see the Appendix). This enables us to arrive at the posterior probability of π_i , given our data.

How to Select M and Avoid Overfitting. In conventional (non-Bayesian) neural networks, selection of M, the number of logit curves, is an important issue, as too large M can cause "overfitting." Overfitting is a danger with any statistical model, but especially so for very flexible forms. A model that is too flexible picks up idiosyncrasies unique to a particular data set rather than the structural features of the world that pertain to out-of-sample data. The danger is always real, and many attempts have been made to develop procedures that protect against it. Of course, "underfitting" (or missing relationships of interest) is a danger as well, so there is good reason to think that M > 1.

In the Bayesian framework, the presence of smooth priors for the parameters significantly alleviates the problem of overfitting. Instead of searching for the set of parameter values that maximize the model performance on the training data, we look instead for solutions that at the same time punish model complexity. More important (in principle at least), in the Bayesian paradigm no single model is the correct one. Rather, because each alternative is correct with some probability, different models can be compared by examining the

⁶ The large variety of neural network models includes many that are even more general than equation 3, as they allow additional levels of hierarchy, stacking functions within functions, and feed-backward effects (as in the so-called recurrent neural networks [Holland 1998], which are appropriate for modeling time series data). In addition, one can choose functions other than logit or even mix several different functions in the same analysis. Neural network models also can be used with stochastic components other than Bernoulli to model different types of dependent variables.

measures of evidence for them.⁷ In practice, however, there is a fairly straightforward test of whether one is finding structure or overfitting ephemera. The procedure is to set aside a portion of the data as a "test set," fit the model to the remaining data (the "training set"), and see whether the forecasts hold up.⁸ Because there is always the tendency to iterate back and forth between fitting models to the training set and verifying the model in the test set, we set aside two test sets, the second of which we did not consult until all exploratory work was completed. This second test set was used only to compare the performance of the chosen neural network specification and the corresponding logit specification.

Interpretation. The neural network literature is concerned almost exclusively with pattern recognition and forecasting, and the issue of interpreting the effect of the explanatory variables on π_i has not received much attention. In political science research, however, interpretation of causal effects is equally critical. The problem is that the functional form is so flexible, and the estimated relationships between π_i and X_i can be so complicated, that the parameters $(\beta_{(1)}, \ldots, \beta_{(M)})$ and $\gamma_0, \ldots, \gamma_M$) are almost impossible to interpret directly. We have created a graphical device that enables us to produce highly interpretable results. For example, we can plot the expected value of (and confidence intervals for) the probability of conflict by one or two explanatory variables, while holding the remainder constant at chosen values. Of course, since neural networks allow estimation of different effects for different observations, the values held constant are critical features of interpretation. The identical methods can be used with regression models but are not needed because the effect parameters are constant, and so the whole functional relationship can be easily summarized with a single number. We elaborate on these methods of interpretation later.

Of course, in practice, interpretation is a problem for any statistical model beyond the very simple linear additive setup. That is why we introduce several graphical procedures for interpreting the results of neural networks.

Possible Objections to Neural Networks

Neural networks come with considerable baggage; some criticism is deserved, some not. Before going into

⁷ There are theoretical results that can help with overfitting. For example, Neal (1996) proves that a Bayesian neural network can use an infinite number of logit curves without causing improper behavior in the output function, provided that the prior variances are properly scaled. He suggests one should choose the most complicated model that is computationally feasible (the largest possible M) and scale the variance of the priors so that it is related to model complexity. This way, one has a model capable of extracting as much information as possible, but the data are taxed to the same degree. Unfortunately, this requires use of extremely time-consuming computational algorithms, as discussed in the Appendix.

*Validation with training and test sets can be improved in theory with such techniques as cross-validation, which breaks the data set into all possible splits. In practice, most users keep a single test set aside.

a detailed analysis using neural nets in the study of conflict, it would be helpful to put to rest a few issues that may come to mind when the term "neural networks" is used.

First, neural networks are sometimes treated as a black box for classifying very complex data patterns in the absence of theory (e.g., handwriting recognition). In contrast, we hypothesize that for international conflict data there are massive nonlinear interactive effects, and only the confluence of many causal factors leads to a nontrivial increase in the probability of war. This allows us to interpret the output of the model in a way that is useful for the international relations scholar, not simply as a black box that does a good job of classifying which observations are more or less likely to be conflictual.

Second, why neural networks? Would simpler and more well-known interactive methods work better? The answer is no. Linear or logit models can include multiplicative interactions, but these have not worked well in practice. Even bivariate multiplicative interactions have not performed well because of multicolinearity. Our evidence indicates that interactions in international conflict are considerably complicated. We need a method that can deal with massively nonadditive interactions, not one that can be grudgingly "tricked" into allowing for a few simple interactions.

Third, early research in neural networks stressed nonstatistical pattern recognition, but we rely on more recent work that puts neural networks on a firm statistical foundation. We use more complex variants of well-known statistical models, and they come with all the standard apparatus for validating and comparing models and avoiding unnecessarily complex specifications. If our conjecture is false, and simple logit performs as well as complex neural net models, then the in-sample and especially the out-of-sample forecasts will clearly indicate this. The statistical basis of our work means we need not fear that neural nets are just very good at picking up in-sample idiosyncrasies in the data.⁹

Fourth, we guard against overfitting by relying extensively on out-of-sample forecasts to validate our models. If neural nets do not detect "real" properties of conflict data, merely idiosyncratic patterns, then they will yield good in-sample but poor out-of-sample forecasts. Many of the problems associated with early nonstatistical neural network models can be avoided by using only models with a firm statistical basis and then evaluating the performance of all models with out-of-sample forecasts.

In brief, neural nets have a venerable history with

⁹ The simple logit is not formally nested within the neural nets we use, but in the data analysis presented below, the forecasting performance of the neural network is so overwhelmingly superior that any criteria for discriminating between nonnested alternatives would clearly choose it over the logit. This does not imply the choice of neural network specification, or any other statistical specification, is a purely mechanical task. Art is involved in any model choice, and no less so in the choice of a particular neural net specification. But the out-of-sample forecast test guards against being "too artful."

numerous success stories. Many criticisms apply to the early nonstatistical variants. Modern neural nets are as firm a part of statistics as are its simpler variants, starting with the logit. As long as we use techniques that allow empirical work to discern whether the additional complexity is both necessary and useful, there should be no reason to fear and much to gain from the newer, more sophisticated, methods.

FORECASTING AND EXPLAINING POSTWAR CONFLICT

In this section we discuss the data and model, forecasting performance, and causal structure in the model results.

Data and Model

In order to test our conjecture, we use the standard dyad-year design, with the same data and variables employed repeatedly in the scholarly literature. This eliminates the possibility that the improved performance of our model is due to better data. For similar reasons, we limit ourselves to politically relevant dyads (PRDs), that is, pairs of essentially contiguous states or with at least one major power, on the grounds that non-PRDs are unlikely to engage in militarized conflict. We thus focus on the harder test of finding variable effects among this more selected homogeneous set of dyads.

Specifically, we use data compiled by Richard Tucker (1997) from a variety of sources. ¹⁰ The set contains 23,529 dyad-years between 1947 and 1989. The dependent variable is coded one for dyad-years with a new "militarized interstate dispute" (MID), zero otherwise (Gochman and Maoz 1984; Jones, Bremer, and Singer 1996), with years of MID after onset dropped. MIDs are rare, occurring in only 976 (4.1%) of dyad-years.

The explanatory variables include dummies for whether the dyad contains geographically contiguous countries (Contiguous) and is allied in defense pacts, neutrality pacts, or ententes (Ally). The similarity of state preferences between two partners (Similarity) is measured, as usual, by the resemblance in their alliance portfolios. The similarity data we use here are from Tucker (1999) and are based on a measure developed by Signorino and Ritter (1999); the measure runs from −1 to 1, where 1 indicates maximal alliance portfolio similarity. The imbalance of power within the dyad (Asymmetry) is measured by the Ray and Singer (1973) index of relative capabilities of the dyadic partners. It is continuous, ranging from 0 to 1; 1 indicates that all military capability is held by one partner, whereas 0 indicates an exact division between the two.

The key variable in many conflict studies is the degree of democratization of the dyad. We use a slightly updated measure from the Polity III (Jaggers and Gurr 1996) data set. As is common, we measure

each partner's democracy by the difference between its "democracy" and "autocracy" scores. This differenced measure ranges from -10 (extreme autocracy) to +10 (maximal democracy). Conflict scholars have debated about the proper way to construct a single dyadic democracy index from the two measured democracy scores, but it is easy enough to enter both (Dem a and Dem b) into the analysis.¹¹

Finally, we use the number of years since the last conflict (*Peace Years*) as a measure of temporal dependence (Beck, Katz, and Tucker 1998). This variable controls for the possibility that conflict is more likely to erupt after previous disputes than after a long period of peace. As Beck, Katz, and Tucker show, the addition of this variable turns an ordinary logit analysis into a grouped proportional hazard model.¹²

We divided the data into an in-sample training set, 1947–85, which we use to fit the model, and a test/validation set, 1986–89, which was used only once to evaluate the forecasts. We fit the neural network model in equation 3 to the training set. Without looking at the test set, we experimented with setting M to various values. We did this by putting aside different portions of the 1947–85 data and trying different values of M (in the spirit of cross-validation). Only after we completed this testing did we look at the 1986–89 data. We also fit a standard logit model to the training set for comparison.

We found that an *M* set around 25 provided about the right level of flexibility and predictability for our data (see the Appendix). This seemingly large value is counterbalanced by our priors, which favor small values for all parameters. This means the model in total is a very flexible functional form that heavily favors smooth curves with few bends. Deviations from this smoothness only occur when the data provide clear evidence to the contrary. That is, unlike logit models and other low-dimensional parametric forms, when the data speak loudly enough, the fit responds.

¹⁰ The measures are more fully described m the Appendix. The data set is archived on the *APSR* web site.

¹¹ For each observation, we randomly assign A to one dyadic partner and B to the other, which is consistent with the hypothesized symmetric effect (and confirmed by our empirical analyses). The traditional approach of creating a single index similarly treats the two countries as symmetric and exchangeable. For simplicity of presentation, we chose not to do this for asymmetry as well, although it would be a reasonable approach to explore.

¹² For most complicated maximum likelihood models, such as for duration or count data, neural nets can be used to replace the simple specification that the underlying mean is a linear function of the data with one that the mean is a massively interactive function of the data. As of now, no off-the-shelf software can do this, but programming these more complicated models is straightforward if nontrivial. We focus here only on the binary outcome case.

¹³ The 1947-85 data were randomly divided into training and test sets of equal size. The random split was possible because the peace years variable induced conditional temporal independence. As a further check on the choice of *M*, the pre-1986 data were split again, and the choice of *M* was rechecked and validated with the new test set. We chose to do final validation on the data split temporally, that is, the last four years of the data set, to provide a tougher test for the neural net forecasts. The use of this hold-out set also makes our validation look more like a true forecasting exercise.

Year	Conflict: % Correct		Number	Peace: % Correct		Number
	LogIt	NN	of 18	Logit	NN	of 0s
1947–85	0	25.3	892	100	99.58	20,155
1986	Ō	18.5	27	100	99.83	584
1987	Ô	14.3	28	100	98.98	587
1988	Ô	23.1	13	100	99.34	609
1989	Ô	12.5	16	100	99.51	618
1986-89	.ŭ	16.7	84	100	99.42	2,398
Total	ñ	24.6	976	100	99.57	22,553

Forecasts

Table 1 gives one view of the comparative forecasting performance of the logit and neural network (NN) models. In the table, we divide the forecasts into conflict (1) and peace (0). The left-hand portion reports success at forecasting conflict when it occurred, and the right-hand portion gives success at forecasting no conflict when there was none. As is clear, since the logit model never forecasts that a conflict will occur in any one dyad (i.e., the probability never reaches 0.50), it forecasts incorrectly for all the cases of conflict and correctly for all the cases of peace. This is no great success, of course, since the optimistic claim that conflict will never occur is correct 96% of the time!

The table indicates that the neural network model performs substantially better than the logit, using the identical set of explanatory variables. It is nearly as good as the logit at predicting peace, with all probabilities exceeding 99% correct. More important, when military conflict occurs, the neural network model makes a successful forecast 16.7% of the time. This is not high in an absolute sense, but it is much better than the logit success rate of zero. Given the high costs of military conflict and the tremendous benefits of knowing ahead of time when a war will occur, this improved forecasting performance could be of significant policy value. More significant, from our perspective, is that the model's forecasting performance confirms a durable causal structure. The relatively high percentage of successful predictions is within a reasonably narrow range, from 12.5% to 23.1%, for each of the four out-of-sample years, which further confirms the overall result with separate, although not independent, observable implications. Furthermore, most of these figures are lower than their fit to the training set, which is as it should be if we expect not only structure but also change in the real world. The model predicts 25.3% of conflicts correctly in-sample but only 16.7% correctly out-of-sample, which indicates either slight overfitting (the difference is just outside the 95% confidence interval) or change in the real world after 1985.¹⁵

Table 1 demonstrates that the neural network model discriminates far better than the logit model by assigning very different probabilities of international conflict to the available dyads. It does not indicate whether either model's probability values are correct except for above and below the 0.50 mark. For example, if we observe 1,000 dyads with a 0.10 probability of going to war, none of these individual pairs would be predicted to fight, but we would expect to see 100 wars from somewhere in the set. We now evaluate the extent to which the two model's predictions have this desirable characteristic.

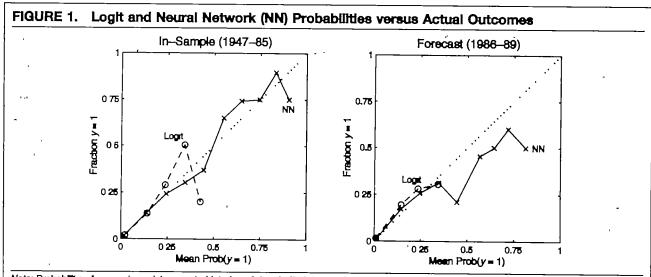
We begin by computing predicted probabilities for each dyad from the logit and neural network models. We then sort these into bins of 0.1 width: [0, 0.1), [0.1, 0.2), ..., [0.9, 1]. Within each bin, we compute the mean predicted probability (which presumably will be near its respective midpoint) as well as the observed fraction of 1s in each bin. We compare the two to check the fit of the model in the training set and to evaluate the forecasts in the test set. Figure 1 plots these numbers for both statistical models.

The in-sample graph in Figure 1 shows that the predicted probabilities and observed fraction of conflict match fairly closely for the neural network model. The logit model is reasonably close as well when the mean probability is 0.25 or less, but it does much worse for higher (i.e., more interesting) predicted probabilities. This is especially important from our perspective: The logit model not only predicts peace breaking out all over but also becomes more inaccurate as the probability of conflict increases, even though these cases are very rare. ¹⁶ In contrast,

¹⁴ If the costs of misclassifying wars versus peace differ, then decision theory indicates that one should merely change the threshold of prediction from the 0.50 that is used here and in virtually all the literature. But that would merely increase the predictions of 1 and correspondingly decrease the predictions of 0 with no other changes in the statistical model and/or interpretation, so we retain the traditional 0.50 cutoff. Figure 1, below, demonstrates that the neural net, but not the logit, fits well for any chosen threshold. The 0.50 threshold seems the most appropriate to us, but none of our comparisons depend on it, and there is no choice of threshold that would make the logit's performance close to the performance of the neural net.

¹⁵ The logit model is the standard in international relations, but it is interesting to compare our results to that of a GAM. Using identical explanatory variables, the GAM forecast slightly better than logit but much worse than a neural network. Using the same comparisons as in Table 1, the GAM correctly predicted 5.6% of all in-sample disputes (and 99.9% of all nondisputes). Similarly, the GAM equivalent of Figure 1 showed performance that was slightly better than the logit but much worse than the neural network.

¹⁶ The one possible exception is the last mean probability bin for the logit, which has very few observations, so the fraction of conflicts has much higher sampling variability. (We chose not to add error bars for each point so that the graphs are easier to read; the precision with which each point is estimated is higher at the left than the right of the graph.)



Note: Probabilities from each model are sorted into bins 0.1 wide (0, 0.1; 0.1, 0.2, ...) and averaged. These averages are plotted horizontally and the actual fraction of ones in the bin are plotted vertically. A line for a model that fits the data will differ from the (dotted) 45 degree line only by random chance. In both the in-sample graph (on the left) and the forecast out-of-sample graph (on the right), the fit is better for NN than logit, and, in addition, NN predictions exact for much higher probability bins than logit predictions.

when the neural network model gives a probability, it is a reasonably accurate assessment of the odds of a conflict occurring within the sample. Of course, an equally important difference between the two models is the much more acute discriminatory power of the neural network model, which we can see because the logit never yields any predicted probability of conflict above 0.50.

Whereas the in-sample graph in Figure 1 evaluates the fit of the two models to the same data, the out-of-sample graph uses the same technique to evaluate forecasting success. Again, there is a reasonably close correspondence between the estimated probabilities and observed fraction of conflict for both models. They track each other very well for the lower probability bins, indicating the same high level of success. (The fact that the logit model did not fit well to the last two points in-sample but did fit the out-of-sample plot seems to be a lucky coincidence.) More interesting is that whereas the logit model has no forecasts in the high-probability region. the neural network model tracks the observed fractions reasonably well even for the sparsely populated high probability bins. One possible problem is the noticeable overestimation of conflict for the neural network model in this high-probability region (which indicates that our forecasts could be improved), but the observed numbers are both fairly small and not that far off.

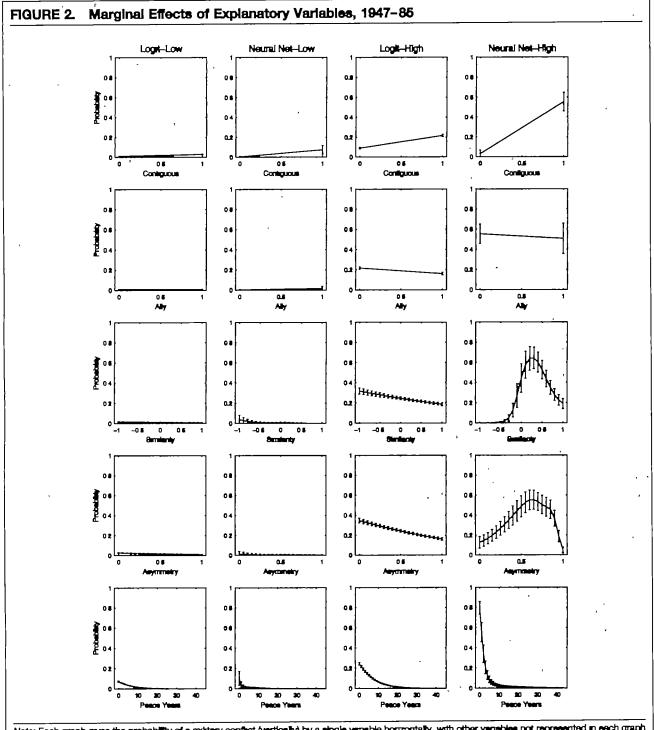
We believe these forecasts are quite solid, and although many uncertainties remain, they seem to be far better than any previously produced. They are also more accurate than many scholars thought would be possible. This clearly indicates that the neural net model we use is superior to the logit model in our data.

Causal Structure

We believe our model reveals clear evidence of structure in international conflict data. This evidence is consistent with that predicted by our conjecture about models of conflict. To interpret the results, we created what we call marginal effect graphs that plot the probability of conflict by one explanatory variable, holding all the others constant at a designated value. Since our conjecture about conflict studies holds that the effect of most variables will be larger (i.e., more discriminating) when the ex ante probability of war is greater, we hold constant the other variables at two values: high and low probability of conflict. (For simplicity, we compute these by the median of each explanatory variable among observations, where Y = 1 and Y = 0, respectively.)

Figure 2 presents these graphs for the logit and neural network model for each of the explanatory variables except democracy (which we will discuss separately). The low and high controls for the logit model appear in columns one and three, and for the neural nets these are given in columns two and four. Each explanatory variable appears in a separate row; the vertical lines on the graph are one-standard-error bars surrounding the predicted probability. (Examining results as we do, by grouping dyads according to the ex ante probability of conflict, creates no selection bias, nor does selecting cases based on the probability of conflict, because the analysis has already been conducted with all dyads.)

Substantively, the neural network analysis in Figure 2 shows a few similarities to conventional logit analysis, but it also demonstrates features consistent with our conjecture. As can be seen by comparing the first and third columns, logit analysis allows for greater effects of the explanatory variables when the ex ante probability of conflict is high, but the differences are small, and virtually all the logit effects are substantively small. Even in the high ex ante case (column 3), only contiguous and peace years have a notable substantive effect on the probability of conflict. Thus, the logit model presented here is similar to the typical logit model of conflict: democracy (see Figure 3).



Note: Each graph gives the probability of a multipry conflict (vertically) by a single variable horizontally, with other variables not represented in each graph held constant at designated values that govern high or low ex ante probabilities of conflict. Note how the effects are much larger for the neural network than logit columns, and especially large for high ex ante probability of conflict dysids under neural networks.

contiguity, and peace years have statistically significant, though substantively small, effects on conflict, whereas the other variables have statistically insignificant (or marginally significant) and very small substantive influence.

In contrast, the neural network model produces much larger changes in the effect of any variable as we move from a low to a high ex ante probability of conflict (compare the second and fourth columns). This is

particularly obvious for some of what normally might be considered "control" variables. For example, contiguity has a strong effect on the probability of a dispute in the logit analysis, but this effect is more than doubled for the high-probability case in the neural network analysis. The effect of contiguity is so strong that it is not hidden by the logit analysis, but allowing for complex interactions shows that it has the extremely strong influence that we would expect but that previous researchers were unable to demonstrate.

The neural network analysis also reveals the importance of duration dependence (peace years) when the ex ante probability of a dispute is high. Although the logit analysis finds some evidence of this dependence, it is quite modest. For high ex ante probability of war situations, with a conflict occurred last year, the probability of another under logit analysis is less than 0.25, compared to almost 0.80 under neural network analysis. It takes about a decade for this probability to recede to nearly zero. Clearly, both analyses give a similar pattern of decay, but the more flexible NN gives a much higher maximum probability of a dispute. The effect of the duration of peace on the probability of a dispute is clearly underestimated in the logit analysis. Duration dependence is sufficiently strong to emerge in the logit analysis to some degree, but the lack of interactions in that model does not allow us to discern how critical time is in forecasting future disputes.

NN also reveals that two important determinants of disputes, similarity and asymmetry, have a very strong but nonmonotonic effect on probability. The logit analysis assumes that all effects are monotonic and so cannot detect these kinds of relationships. The NN analysis reveals a stronger and more complex association between these variables and conflict.¹⁷

Our findings of nonmonotonic effects are consistent with the more game-theoretic studies of Bueno de Mesquita and Lalman (1992) and Signorino (1999). Both similarity and asymmetry are the two standard measures of the benefits and costs of conflict. For example, these theories assume that as one side acquires comparatively more military capability, the probability of conflict will rise and then fall, but no logit model in the literature or here supports this claim for either asymmetry or similarity, or even indicates that either has results of any kind of important size. In contrast, the NN results reveal both are very important in high ex ante probability dyads in just the manner predicted by theory. Indeed, in the high ex ante probability dyads, a large change in either similarity or asymmetry can increase or decrease the probability of conflict by more than 50 points.

Finally, NN models show a clear influence of dyadic democracy on the probability of conflict. The logit analysis, which averages effects over all dyads, shows a small but significant pacific effect of democracy. The linear nature of the logit model requires that the effects of each partner's democracy scores be additive. The

contour plots implied by the NN model, given in Figure 3, show that the pacific effect of democracy is nil when the ex ante probability of conflict is low but is strong when the probability is high. In the latter case, the maximal influence of a movement on the democracy scale is about 40 points, much larger than any effect found in the logit analysis. The nonmonotonicity and nonadditivity allowed by the NN model reveals that the pacific effects are strongest when both partners score high on the democracy scale and are much smaller substantively in the rest of the plot. Interestingly, the most quarrelsome dyads are those in which both partners have a middling democracy score.¹⁸

Dyads containing an extreme autocracy are more likely to fight than are very democratic dyads, but they are less likely to fight than dyads with middling democracy. This effect is found only in the NN analysis, which both allows for massive interactions and nonmonotonic effects of individual variables. In other words, democracy does have pacific effects, but only among dyads that are otherwise likely to be conflictual and only among the most democratic nations. A portion of that effect is strong enough to emerge even in the logit model, which averages small and large effects, but its substantial influence is seen only in the NN analysis. Thus, democratic peace theorists such as Maoz and Russett (1994) appear to be right, although they heavily underestimate the pacific effect when democracy is important and overestimate it in cases in which democracy matters little.

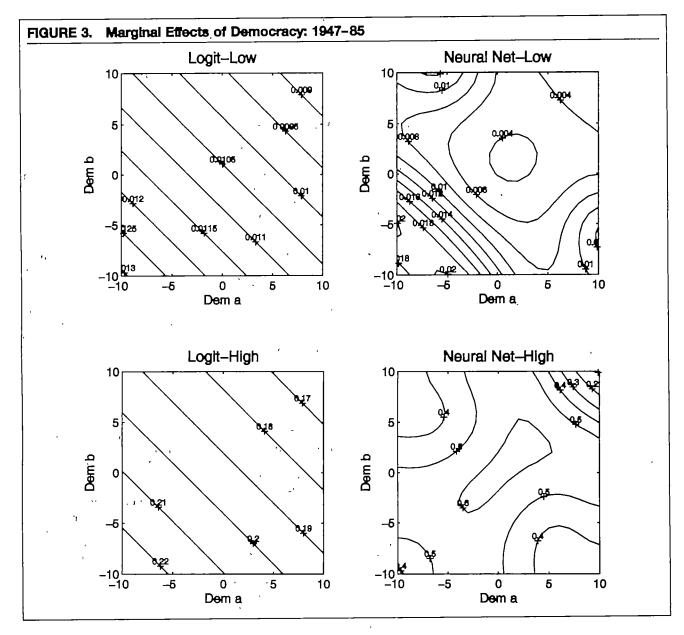
The simple plots we provide here cannot, of course, demonstrate the full structure of the neural network or its power. That is more clearly shown in its ability to forecast disputes much better than any linear or additive analysis. Yet, these plots do show that our results are stronger than those found by more standard methods and that neural nets can find contingent causal structures missed by the simpler, uncontingent, logit model.

CONCLUDING REMARKS

We have proposed a simple conjecture that appears to explain a significant and well-known problem with international conflict studies. The conjecture is consistent with a variety of implications we can observe from the literature. It also suggests a set of specific problems with the most common statistical model used in the field. To evaluate our conjecture, we adapted a statistical model that better matches the features of our conjecture and, as a consequence, the substantive concerns of quantitative and qualitative researchers. The result is a superior statistical model of international conflict. It appears to be the only large-N statistical model that estimates the probability of any international conflict at some level higher than 0.50

¹⁷ As a diagnostic, we also applied generalized additive models to the data. GAMs reveal nonmonotonic effects for these two variables, but their influence, which is averaged over all cases, is substantively much smaller than the effects found by the neural net for the high ex ante probability cases. Furthermore, the GAM effects, which are the "average" of the NN effects over all combinations of the other independent variables, are dissimilar to the NN effects shown. This must be the case if the NN effects at the differing combinations of the independent variables are different; the GAM cannot accommodate this. Of course, logit models can be "tricked" into allowing for nonmonotonicities, although these must be specified in advance As with linear regression, in practice it is hard to get good logit estimates of nonmonotonic effects because of multicolinearity and the arbitrariness of nonmonotonic specifications.

¹⁸ In the postwar data there are relatively few dyads in the interior of the plot. Therefore, the confidence intervals, which would be difficult to show in this type of graph, are much wider in the middle region. Since the confidence intervals for Figure 3 are a function of several model parameters, they would be most easily computed using the simulation technique described in King, Tomz, and Wittenberg 2000.



(ours exceeds 0.90 for some cases). We are able to predict about 17% of conflicts from data on the years prior to the conflict.

This forecasting result can only be driven by an underlying structure of international politics that stays relatively stable over time. Confirming the existence of and understanding this structure has been a holy grail in quantitative conflict studies, and we believe our neural network approach represents progress toward this goal. Along with the graphical tools we introduce, these models may have the potential to uncover structure in other areas.

Neural networks are computationally and intellectually complex, but they are no more than extensions of standard interactive models. While early neural network research often seemed to overfit the data, new Bayesian analysis can surmount much of that problem. It seems unlikely that the effect of any variable commonly used in the field is completely independent of the levels of other variables, as standard approaches

assume. Neural networks are designed to unlock such complicated structures. There is no question that they do a wonderful job of recognizing patterns in other disciplines, but they also can find complicated structural regularities in standard international relations conflict data.

APPENDIX: BAYESIAN METHODS FOR NEURAL NETWORK MODELS

The Model

The basic neural network model we estimate is given in equation 3. To it we add a standard Bayesian setup to shrink the parameter space.¹⁹ We do this by adding two levels of hierarchy. First, we assume independent normal distributions $N(0, 1/\alpha_k)$ for each group of the parameters (and hence the

¹⁹ The explanatory variables are normalized before being input to the neural net to improve computational accuracy. All interpretations in this article are based on the original, unnormalized measures.

index h), with one for the constant terms β_0 , one for each element of β other than β_0 , and one for the set of elements of γ . In addition, for each of the α_k elements, which control how strongly any parameter is influenced by its prior to be small, we assume an (uninformative) improper uniform prior distribution (which is one way of expressing a degree of ignorance or uncertainty about which values are more or less likely).

As discussed in the text, we select M in the model by comparing the test set performance of models with different M's. M should be large enough for the model to be adequately complex and is theoretically unrestricted in magnitude in the Bayesian setting. In the interest of model parsimony and computational efficiency/feasibility, we follow the general practice of searching for the smallest possible M that gives satisfactory performance and restricting the total number of parameters in the resulting model (M(k + 2) + 1, where k is the number of independent variables) to not exceed n/10. To this end we experimented with M set at 5, 10, 15, 20, 25, 30, 35, 40, 50.20 Since the test/validation set performance of models with M around 25 was obviously better than around 20, and an M larger than 25 did not yield significant improvement, we set M at 25.21

The Posterior Distribution of the Parameters

By Bayes's theorem, the posterior distribution is, of course, the product of the prior distributions and the likelihood function:

$$P(\gamma, \beta, \alpha | Y) \propto \left(\prod_{k=1}^{k+2} N(0, 1/\alpha_k) \right) \left(\prod_{i=1}^{n} \pi_i^{Y_i} (1 - \pi_i)^{1-Y_i} \right), \tag{4}$$

where π_i , the probability of conflict, is defined in the second and third lines of equation 3.22

Ideally, we would be able to draw random samples of γ , β , and σ directly from this posterior distribution to compute quantities of interest. Indeed, this has been accomplished with a hybrid version of Markov Chain Monte Carlo (MCMC) methods (Neal 1996). Yet, runs that provide exact draws from the posterior with MCMC methods take an inordinately long time to complete. The method also has all the usual problems caused by a lack of agreement on how to assess stochastic convergence in MCMC algorithms.

After trying Neal's approach, we felt that more time to experiment with different specifications to understand the data was necessary, so we adopted the normal approximation approach (MacKay 1992a, 1992b, 1992c, 1994).²³ It approximates the posterior distribution of the model parameters as multivariate normal, which makes analytic solutions possible. As in standard maximum likelihood estimation, the variance matrix of the parameter estimates is found by inverting the Hessian.²⁴

Posterior Probabilities of Conflict

One of our goals is to generate forecasts of international conflict. The other is to see how these forecasts would change in accord with various configurations of the explanatory variables, X_i . For both goals, we need to specify the posterior probability of the forecasts.

Conceptually, computing the forecast posterior is simple and in principle can be accomplished by the usual simulation methods that apply to virtually every other statistical model (see King, Tomz, and Wittenberg 2000). That is, draw random samples of β , γ , and α from their posterior distribution in equation 4 (or their asymptotic normal approximation), insert them into the functional form in the second and third lines of equation 3 to compute π_i , and take a random draw from a Bernoulli distribution with this parameter (the first line of equation 3). In practice, we use MacKay's (1994) faster analytical approximations to accomplish the same task.

The Data

The data set used here was provided by Richard Tucker (1997), and corresponds to the data used in Beck and Tucker (1998). The data set consists of observations on PRDs from 1947-89 (or a shorter period if earlier data was either not available or the dyad became a PRD after 1947, usually because one partner became independent later than 1947). The peace years variable until the first dispute was coded as the time since a dyad became a PRD, that is, the first year a dyad was at risk of a dispute was the year it became a PRD. Dyads that involved a major power in a region in which it had little or no interest (e.g., China in Latin America) were excluded; operational rules for this exclusion are in Tucker (1997). Note that excluding these dyads affects the affinity measure. These coding rules yielded 23,529 dyad-years. Both alliance data (for the similarity measure) and national capabilities data (for the asymmetry measure) were taken from data sets created by the Correlates of War project; this project also defined the domain of nation states which were used to construct the PRDs. The actual alliance data were provided by J. David Singer, the capabilities data were from Singer and Small 1993 and the system membership data were taken from the Peace Science Society (International) web site (http://pss.la.psu.edu/DATARES.HTM).

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²⁰ Estimation was via the EM algorithm that alternates the error function minimization with respect to γ and β given α_k and the value updating of α_k given γ and β . Optimization used conjugate gradients, with initial weights randomized and all training data (no noise added to inputs) presented in a batch. The cutoff criterion for the training process was an error tolerance of 0.00001.

²¹ Given our large sample size, this seemingly large M actually yields a ratio of parameters to data points that is smaller than typical and considerably smaller once we take into account the prior.

²² Note that the model is technically unidentified, although in an inconsequential manner. Switching γ_1 with γ_2 and $\beta_{(1)}$ with $\beta_{(2)}$ yields identical values for π . This causes no problems for our computation of the various marginal effects of the independent variables. It could cause problems in applying standard optimization algorithms, but computationally efficient techniques that have been developed to evaluate the gradient work well despite this problem (Bishop 1995, 141).

²³ The software we used to estimate the model ("Bigback5" by David MacKay) is available at http://wol.ra.phy.cam.ac.uk/mackay/bigback/bigback5.tar.gz.

²⁴ In view of current computational limitations, we feel that the normal approximation approach to Bayesian neural nets provides good performance and is, at present, more useful than the MCMC approach. But, of course, new computational breakthroughs may change matters, and users are encouraged to explore alternative methods as they become feasible.

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Coordination, Moderation, and Institutional Balancing in American Presidential and House Elections

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oters have been coordinating their choices for president and House of Representatives in recent presidential election years, with each voter using a strategy that features policy moderation. Coordination is defined as a noncooperative rational expectations equilibrium among voters, in which each voter has both common knowledge and private information about the election outcome. Stochastic choice models estimated using individual-level NES data from 1976–96 support coordination versus a model in which voters act nonstrategically to moderate policy. The empirical coordinating model satisfies the fixed-point condition that defines the common knowledge expectation voters have about the outcome in the theoretical equilibrium. The proportion of voters who split their ticket in order to balance the House with the president has been small but large enough to affect election outcomes. Moderation has usually been based on voters' expectations that the president will be at least an equal of the House in determining postelection policy.

o Americans coordinate their votes for president and for Congress? Suppose a voter knows that policy outcomes are compromises between the positions taken by the president and Congress and believes the two political parties push for distinct policy alternatives. Then the voter believes that different combinations of party control of the presidency and Congress will produce different policy outcomes. If the voter cares about the policy outcomes, then the voter's choices among candidates should depend on the combination of party control the choices will help bring about. Some voters would do best to vote a party-line, straight ticket, while others would do best to split their ticket to try to bring about a "moderate" policy outcome, that is, an outcome in some sense between the parties' positions (Alesina and Rosenthal 1989, 1995, 1996; Fiorina 1988, 1992, 73-81; Ingberman and Villani 1993). To cast the most effective vote, each voter should consider how likely it is that the election will produce each possible combination of party control. Coordination occurs when each voter makes the best possible assessment of what the election outcome will be and then uses that assessment to choose candidates in the way most likely to produce the best possible result for the voter.

Moderation does not necessarily imply coordination.

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Moderation refers to the relationship between the policy outcome intended by the voter and the parties' policy positions. There is moderation if the intended policy outcome is an intermediate combination of the parties' positions. Coordination refers to a relationship among different voters' choices among candidates. There is coordination if each voter's choice is in a strategic sense in equilibrium with every other voter's choice. I interpret this to mean three things: Each voter's strategy for choosing among candidates is in equilibrium with every other voter's strategy for choosing among candidates; each voter's beliefs about every other voter's preferences and strategy are compatible with the voter's own strategy; and each voter's beliefs are compatible with every other voter's beliefs.

Coordination among voters is not the same as cooperation among them. In a financial market, economic agents who possess private information and act strategically may coordinate their decisions to buy or sell based on the system of prices that all agents know, without the need for sets of agents to reach binding prior agreements about how to act. The information that the agents have in common may be enough to let the market operate in a state of strategic equilibrium among agents (e.g., Grossman 1989). In the theoretical model I develop in this article, voters do not cooperate; each voter is able to make an equilibrium strategic choice among candidates, in accurate anticipation of the aggregate result of the choices all other voters intend to make, by using information that all voters know. Alesina and Rosenthal's (1989, 1995, 1996) theory of moderation features coordination-each voter's choice depends on an expected election result that everyone knows and that is an equilibrium—but Fiorina's (1988, 1992, 73-81) theory does not (Alesina and Rosenthal 1995, 66-71). In Fiorina's theory, voters do not act strategically at all. Voters act strategically in

¹ Studies of electoral coordination often focus on the "wasted vote" logic of Duverger's Law, which is said to keep the number of parties receiving votes small (e.g., Cox 1997; Fey 1997; Palfrey 1989). I examine coordination that relates not to the number of parties—two are assumed—but to possible moderation in voting for president and the House of Representatives.

Alesina and Rosenthal's treatment, but no one has any private information about the election outcome. In the model developed here, different voters have beliefs about the upcoming election results that are very similar but not exactly the same in equilibrium. The similarity is a result of the common knowledge they have, while the differences that remain are due to private information each voter has about concerns that affect the voter's preferences among candidates.

I use National Election Study (NES) data from presidential election years 1976 through 1996 to estimate a stochastic choice model that closely matches the theoretical model. Based on the empirical results, I argue that in recent years American voters' choices for president and the House of Representatives have been coordinated, with each voter using a strategy that features policy moderation. In both the theoretical model and the empirical model, each voter forms preferences about the candidates based on a personally distinct idea about what the parties' policy positions are. The coordination takes the form of a rational expectations equilibrium among voters. The notion of voter equilibrium here is similar to the concept of a "fulfilled expectations" equilibrium (Kreps 1977) developed for elections by McKelvey and Ordeshook (1984, 1985a, 1985b).²

The models of moderation-with-coordination developed by Alesina and Rosenthal (1989, 1995, 1996) have implications for the midterm loss phenomenon and for election-related fluctuations in economic phenomena such as growth. The latter have been tested using aggregate time-series data, with mostly positive results (Alesina, Londregan, and Rosenthal 1993; Alesina and Rosenthal 1995; Alesina, Roubini, and Cohen 1997).

Evidence that individuals coordinate has been lacking. Several authors report individual-level (Alvarez and Schousen 1993; Born 1994a) or aggregate-level (Burden and Kimball 1998; Frymer 1994) tests aimed at Fiorina's (1988; 1992, 73–81) theory of moderation, with controversially (Born 1994b; Fiorina 1994) negative results. As previously noted, Fiorina's theory does not posit coordination among voters. The tests of his theory have not examined whether voters' expectations about the election outcomes affect their choices. I use the NES data to estimate a stochastic choice model that implements Fiorina's theory and test it against the coordinating model.

The heart of the formal model of coordination that I develop and test empirically is a fixed-point theorem that defines the common knowledge belief that all voters have about the upcoming election results. The election results are summarized in the values of two aggregate statistics: (1) the proportion of the two-party

vote to be cast nationally for Republican candidates for the House and (2) the probability that the Republican presidential candidate will defeat the Democrat. Each voter cares about those values because they affect the loss each voter expects to experience from policies the government will adopt after the election. Each voter chooses among candidates so as to minimize that loss. Therefore, each voter's belief about the aggregate values affects the voter's choices. In general, if there is a single pair of values that every voter believes the aggregate statistics to have and every voter chooses based on that belief, then the aggregate result of all their choices may be expected to differ from the values they all believed. In that case, beliefs and expected actions are inconsistent with one another. The fixedpoint result demonstrates that a pair of aggregate values exists for which there is no such inconsistency: When every voter chooses based on belief in the fixed-point aggregate values, the aggregate result to be expected from all their choices is the very same pair of aggregate values. In that case, beliefs match expected actions. Common knowledge of the fixed-point values is not quite enough to establish an equilibrium, because the common knowledge does not include private information each voter has about the voter's actual choice. If each voter's belief equals the common knowledge values adjusted by amounts that correspond to the voter's choices, then the belief each voter has about the aggregate result is consistent with the choices the voter knows it will make. Despite knowing that every other voter is also acting on the basis of beliefs that differ slightly from the common knowledge values, no voter knows any other voter's private information, and so no voter can do any better than to use the common knowledge values as its aggregate expectation for what everyone else is going to do. Because all voters are situated similarly, it is common knowledge that every voter is forming beliefs in that way. Thus there is an equilibrium in which every voter has a slightly different expectation regarding the upcoming election results.

The fixed-point result that determines what voters' beliefs are in equilibrium in the theoretical model imposes a constraint on the statistical model to be used to estimate the parameters of the model with survey data. The empirical model is defined to correspond as closely as possible to the theoretical model. Both models use the same functional forms for each voter's loss function as well as the same specifications for the statistical distribution of the random disturbances that affect the candidate choices each voter makes. Hence, both models specify the same stochastic choice model for each voter. The special constraint is that the estimates of the parameters of the empirical model must be such that the aggregate election summary statistics estimated from the survey data satisfy the fixed-point condition. Stochastic choice models of electoral behavior have not previously imposed such a constraint, and their introduction here is an important innovation in the empirical model specification. The empirically determined fixed-point values are the estimates for the aggregate values that are common knowledge in equilibrium in the theoretical model. A distinc-

² McKelvey and Ordeshook's (1984, 1985a, 1985b) equilibrium concepts are similar in that they use rational expectations, but they differ technically because their models feature purely spatial utility rather than probabilistic voting, assume the existence of a single policy dimension (although some voters are uninformed about candidates' positions), and have each voter voting for only one office rather than two. Enclow and Hinich's (1982, 1983a, 1983b) probabilistic vote choice models also emphasize voters' expectations regarding other voters' votes.

tion between the empirical and theoretical models is that, because of limitations on what we can observe with survey data, the empirical model treats the fixedpoint values as the belief that every voter has regarding the upcoming election outcome. The empirical model does not refine each voter's belief to take the voter's private information into account.

The theoretical and empirical models use assumptions about what individual voters know that are unquestionably unrealistic and unbelievable. It may help to put both the theory and the empirical findings in what I think is the most productive perspective if I say something about this at the outset. In the Conclusion, I briefly outline how in reality a coordinating voting equilibrium must depend on a collection of institutions to aggregate and broadcast information that the current model treats as common knowledge among voters. My analysis is therefore implicitly conditional on the existence of an appropriate institutional context. In a way that I touch on briefly in the Conclusion,3 I think voters possess a variety of beliefs about things the current model treats as common knowledge; the values that are common knowledge in the current model are more like the mean of the distribution of individuals' beliefs than they are values that every individual actually possesses. Even if the current model correctly characterizes the mean, it understates the variability across individuals.

A MODEL OF COORDINATING VOTING WITH MODERATION

The model treats the election as a game among all voters, assumed to be a large number. Voters act noncooperatively. The model focuses on the choices each voter makes between two candidates for president and two candidates for a House seat. The votes for president occur in the context of the Electoral College. There are many House districts, but each voter votes in only one of them. There are two parties, Democratic and Republican, and each race has one candidate running for each party. Both whether an incumbent is running and the incumbent's party vary over districts. Each voter's preferences regarding pairs of candidates depend on the expected election outcome and, therefore, on the choice strategy every other voter is using. Equilibrium occurs when, given everything each voter knows-including the voter's own intended choice and accurate expectations regarding other voters' strategies-no voter expects to gain by using a different strategy.

The preferences each voter has regarding the candidates are based in part on spatial comparisons between the voter's ideal point and the policies the voter believes will result from various election outcomes. Those policies are functions of the policy position the voter expects each party will act on after the election. The expected postelection policy position for each party is a combination of two positions that may differ: the position the voter associates with the party on the

basis of previous turns in office and previous campaigns, and the position the voter thinks the party's current presidential candidate has adopted. All presidential candidates do not have the same ability to move their parties to the position the candidate supports. For each election, the expected party position is a weighted average of the candidate's position and the prior party position. The more influential the candidate, the greater is the weight of the candidate's position.

I use ϑ_{Di} , ϑ_{Ri} , ϑ_{PDi} , and ϑ_{PRi} to denote values in the interval [0, 1] that voter $i, i = 1, \ldots, N$, has in mind at election time for the prior position of the Democratic party (ϑ_{Di}) and Republican party (ϑ_{Ri}) and for the position of the Democratic presidential candidate (ϑ_{PDi}) and Republican presidential candidate (ϑ_{PRi}) . The Democratic positions need not be to the "left" of (i.e., numerically less than) the Republican positions. The policy positions voter i expects the Democratic party and the Republican party to act on after the election are, respectively,

$$\theta_{D_{\mathbf{I}}} = \rho_{D} \vartheta_{PD\mathbf{I}} + (1 - \rho_{D}) \vartheta_{D\mathbf{I}}, \quad 0 \le \rho_{D} \le 1, \quad (1a)$$

$$\theta_{Ri} = \rho_R \vartheta_{PRi} + (1 - \rho_R) \vartheta_{Ri}, \quad 0 \le \rho_R \le 1.$$
 (1b)

If the Democratic party's position is expected to be very close to that of its presidential candidate, then the weight ρ_D is near one. If the Democratic party's position is expected to remain close to its previous value, then ρ_D is near zero. The parameter ρ_R has an analogous relationship to the position voter i expects the Republican party to act on after the election.

The policy expected to result from each possible election outcome depends on four factors: the parties' expected policy positions (just defined); the position expected to be supported in Congress; the president's strength in comparison to the House; and which party's candidate is elected president. To represent the expected position of Congress, I simplify by ignoring both the Senate and all internal structure in the House, such as seats, committees, bills, and rules; the expected position of the House is simply a weighted average of the expected positions of the two parties. Each party's weight is equal to the proportion of the vote that i expects to be cast nationally for the party's House candidates. Using \bar{H}_{l} to denote the expected Republican proportion, the expected position of the House is $\bar{H}_i \theta_{Ri} + (1 - \bar{H}_i) \theta_{Di}$.

The postelection policy expected given any particular value of \bar{H}_l is then a weighted average of the expected position of the House and the expected position of the president's party. The weight of the president represents the president's strength in comparison to the House. For each value of \bar{H}_l there are two expectations for postelection policy, depending on the president's party:

$$\begin{split} \boldsymbol{\delta}_{D_i} &= \alpha_D \boldsymbol{\theta}_{D_i} + (1 - \alpha_D) [\bar{H}_i \boldsymbol{\theta}_{R_i} + (1 - \bar{H}_i) \boldsymbol{\theta}_{D_i}], \\ & 0 \leq \alpha_D \leq 1, \quad \textbf{(2a)} \\ \boldsymbol{\delta}_{R_i} &= \alpha_R \boldsymbol{\theta}_{R_i} + (1 - \alpha_R) [\bar{H}_i \boldsymbol{\theta}_{R_i} + (1 - \bar{H}_i) \boldsymbol{\theta}_{D_i}], \\ & 0 \leq \alpha_R \leq 1. \quad \textbf{(2b)} \end{split}$$

³ See especially note 28.

Policy θ_{Di} is expected to occur if a Democrat is president, and policy θ_{Ri} is expected to occur if a Republican is president. The weights, α_D and α_R , represent the strength a president from each party is expected to have. The value $\alpha_D=1$ means that a Democratic president is expected to dictate policy, so that the legislature would play no role, while $\alpha_D=0$ means that the legislature is expected to determine policy, with the president being irrelevant. The interpretation of α_R is analogous for a Republican president. The functional forms of θ_{Di} , and θ_{Ri} are essentially the same as the simplest policymaking formalism considered by Alesina and Rosenthal (1995, 47–8).

The preference each voter has for each possible election outcome is measured by the loss the voter expects given that outcome. The loss depends on the absolute discrepancy between the voter's ideal point, denoted $\theta_i \in [0, 1]$, and the policy expected given the election outcome: The absolute discrepancy is $|\theta_i - \theta_{Di}|$ with a Democratic president and $|\theta_i - \theta_{Ri}|$ with a Republican president. A voter's expected loss always increases as the absolute discrepancy between the voter's ideal point and the expected policy gets larger.

I use a functional form for the loss that is flexible in two ways. First, an exponent q>0 allows the loss to be a concave (0 < q < 1), linear (q=1), or convex (q>1) function of the absolute discrepancy between θ_i and each expected policy. Second, a variable β_i , $0 < \beta_i < 1$, represents the possibility that voters do not treat the two discrepancies equally. If $\beta_i < \frac{1}{2}$ ($\beta_i = \frac{1}{2}$, $\beta_i > \frac{1}{2}$), voter i weights a discrepancy associated with a Republican president more than (the same as, less than) a discrepancy of the same magnitude associated with a Democratic president. The voter's expected losses with, respectively, a Democratic and a Republican president are

$$\begin{split} \lambda_{D_i} &= \beta_i |\theta_i - \hat{\theta}_{D_i}|^q + \xi_{D_i}, \\ \lambda_{R_i} &= (1 - \beta_i) |\theta_i - \hat{\theta}_{R_i}|^q + \xi_{R_i}, \end{split}$$

where the continuous random variables ξ_{Di} and ξ_{Ri} represent other things besides the policy-related discrepancies that affect the voter's expected loss.

At the time of the election, the voter does not know for sure which candidate will be elected president. I assume that the voter computes an expectation for the expected loss, by using the probability that each candidate will win in a standard expected-value formula. \bar{P}_i denotes voter i's expectation for the probability that the Republican wins. The voter's expected loss is

$$\lambda_{i} = (1 - \bar{P}_{i})\beta_{i}|\theta_{i} - \tilde{\theta}_{Di}|^{q} + \bar{P}_{i}(1 - \beta_{i})|\theta_{i} - \tilde{\theta}_{Ri}|^{q} + \xi_{i},$$
(3)

where $\xi_{i} = (1 - \bar{P}_{i})\xi_{Di} + \bar{P}_{i}\xi_{Ri}$.

The specifications of equations 2a, 2b, and 3 have functional forms that are quite similar to those used by Alesina and Rosenthal (1995), but there is an important difference in what the current model says about the information that voters have. The specifications of equations 2a, 2b, and 3 differ from Alesina and

Rosenthal's (1995) theory in allowing the expected policies θ_{D_i} and θ_{R_i} and the expected election outcomes \bar{H}_i and \bar{P}_i to vary over voters. In the theory of Alesina and Rosenthal (1995, 1996), the expected policies and expected election outcomes have values that all voters have identically in common. The variations in the values over voters in the current model mean that the current model endows voters with private information in a way that the theory of Alesina and Rosenthal (1995, 1996) essentially does not do. The current model, therefore, in an important way generalizes the approach taken by Alesina and Rosenthal (1995, 1996).

A voter chooses the pair of candidates—one candidate for president and one for the House-that minimizes the voter's expected loss, λ_i . I measure the effect on λ_l of choosing any one pair, as follows. Let $\bar{P}_{l,R}$ denote the expected probability that the Republican presidential candidate wins if the voter chooses the Republican, and let $\bar{P}_{i,D}$ denote the probability that the Republican wins if the voter chooses the Democrat. Because the effect of a single voter's choice on P_i is very small, the effect on λ_i of the voter's choosing the Republican presidential candidate rather than the Democrat is well approximated by $(\bar{P}_{i,R} - \bar{P}_{i,D})d\lambda_i/d\bar{P}_i$. If $(\bar{P}_{i,R} - \bar{P}_{i,D})d\lambda_i/d\bar{P}_i$ is positive, then voting for the Republican increases the voter's expected loss. Likewise, let $\bar{H}_{i,R}$ $(\bar{H}_{i,D})$ be the proportion of the national two-party vote received by Republican House candidates if the voter chooses the Republican (Democrat) running in the voter's district. Then $(\bar{H}_{l,R} - \bar{H}_{l,D})d\lambda_i/d\bar{H}_i$ closely approximates the effect on λ_i of the voter's choosing a Republican House candidate. Sums and differences of such quantities measure the effect on λ_i of each of the four possible pairs of candidate choices. I use $\lambda_{i,p,k}$ to denote such a measure of the effect of voter i's choosing the presidential candidate of party p and the House candidate of party h. For simplicity, I ignore the differences in voter population sizes across states. Instead, I assume that $\ddot{P}_{i,R} - \bar{P}_{i,D}$ and $\ddot{H}_{i,R} - \bar{H}_{i,D}$ are constants that are the same for all voters: $b_P = \ddot{P}_{i,R} - \bar{P}_{i,D}$ and $b_H = \bar{H}_{i,R} - \bar{H}_{i,D}$. The effects are

$$\begin{split} \lambda_{i,RR} &= b_P \frac{d\lambda_i}{d\bar{P}_i} + b_H \frac{d\lambda_i}{d\bar{H}_i}, \quad \lambda_{i,RD} = b_P \frac{d\lambda_i}{d\bar{P}_i} - b_H \frac{d\lambda_i}{d\bar{H}_i}, \\ \lambda_{i,DR} &= -b_P \frac{d\lambda_i}{d\bar{P}_i} + b_H \frac{d\lambda_i}{d\bar{H}_i}, \quad \lambda_{i,DD} = -b_P \frac{d\lambda_i}{d\bar{P}_i} - b_H \frac{d\lambda_i}{d\bar{H}_i}. \end{split}$$

Choosing the candidate of party p for president and the candidate of party h for the House increases the voter's expected loss if $\lambda_{i,ph} > 0$ and decreases the voter's expected loss if $\lambda_{i,ph} < 0$.

To compute an explicit rule for the voter's choice, I make two more simplifying assumptions. I assume that β_i is not a function of either \bar{P}_i or \bar{H}_p so that $d\beta_i/d\bar{P}_i =$

⁴ Let $\lambda_{i,R}$ denote voter i's loss if i chooses the Republican candidate, and let $\lambda_{i,D}$ denote the loss if i chooses the Democrat. Obviously, $\lambda_{i,R} - \lambda_{i,D} = (P_{i,R} - P_{i,D})(\lambda_{i,R} - \lambda_{i,D})/(P_{i,R} - P_{i,D})$, if $P_{i,R} - P_{i,D} > 0$. But if $P_{i,R} - P_{i,D}$ is very small, then $(\lambda_{i,R} - \lambda_{i,D})/(P_{i,R} - P_{i,D}) \sim d\lambda_{J}dP_{i}$.

 $d\beta/d\bar{H}_l = 0$. And I assume there are constants b_{HP} and b_{PH} such that $b_{HP} = d\bar{H}/d\bar{P}_l$ and $b_{PH} = d\bar{P}/d\bar{H}_l$. The effects of the possible choices on the voter's expected losses are then

$$\lambda_{t,RR} = (b_P + b_H b_{PH}) w_{Pt} + (b_H + b_P b_{HP}) w_{Ht} + \xi_{RRt},$$
(4a)

$$\lambda_{L,RD} = (b_P - b_H b_{PH}) w_{Pi} + (b_P b_{HP} - b_H) w_{Hi} + \xi_{RDi};$$
(4b)

$$\lambda_{t,DR} = (b_H b_{PH} - b_P) w_{Pt} + (b_H - b_P b_{HP}) w_{Ht} + \xi_{DRi};$$
(4c)

$$\lambda_{t,DD} = -(b_P + b_H b_{PH}) w_{Pt} - (b_H + b_P b_{HP}) w_{Ht} + \xi_{DD},$$
(4d)

with

$$\begin{split} w_{P_l} &= (1 - \beta_l)|\theta_l - \tilde{\theta}_{Rl}|^q - \beta_l|\theta_l - \tilde{\theta}_{D_l}|^q; \\ w_{H_l} &= q(\theta_{D_l} - \theta_{R_l})[(1 - \alpha_R)\bar{P}_l(1 - \beta_l)|\theta_l \\ &- \tilde{\theta}_{R_l}|^{q-1}\operatorname{sgn}(\theta_l - \tilde{\theta}_{R_l}) + (1 - \alpha_D)(1 - \bar{P}_l)\beta_l|\theta_l \\ &- \tilde{\theta}_{D_l}|^{q-1}\operatorname{sgn}(\theta_l' - \tilde{\theta}_{D_l})], \end{split}$$

where $\operatorname{sgn}(x) = -1$ if x < 0, $\operatorname{sgn}(x) = 0$ if x = 0, and $\operatorname{sgn}(x) = 1$ if x > 0. The variables ξ_{RRI} , ξ_{RDI} , ξ_{DRI} , and ξ_{DDI} are defined in Appendix A. The voter's choice rule is: choose the candidate combination that corresponds to the smallest of the four values, $\lambda_{I,RR}$, $\lambda_{I,RD}$, $\lambda_{I,DR}$, and $\lambda_{I,DD}$. If we represent the two candidates the voter chooses by a random variable Y_I that takes values in the choice set $K = \{RR, RD, DR, DD\}$, then we may write the voter's choice rule as

$$Y_{i} = \underset{ph \in \mathcal{K}}{\operatorname{argmin}} \lambda_{i,ph}. \tag{5}$$

Equation 5 defines a strategy for each voter in a large-scale game in which all voters participate. The moves available to each voter are the four possible pairs of candidate choices. I assume that all voters move simultaneously, that each voter plays the game noncooperatively, and that each voter knows that every other voter is playing the game the same way. Both w_{Pl} and w_{Hl} depend directly on the parties' shares of the national House vote (\bar{H}_l) , and w_{Hl} depends directly on the parties' chances of winning the presidency (\bar{P}_l) . The voter's best choice therefore depends on what every other voter is going to do. The strategy defined by equation 5 is an equilibrium if it is the rule that minimizes each voter's expected loss when each voter assumes that everyone else is using the same rule.

For such an equilibrium to exist, each voter must assign values to \bar{H}_i and \bar{P}_i in a way that accurately corresponds to the choices every other voter is likely to make according to equation 5. To be able to do that, each voter must know something about the losses other voters expect to incur from the possible election outcomes. With sufficient information about those losses,

each voter can anticipate what other voters will do in response to each electoral circumstance that may arise. Knowledge of that response pattern allows each i to determine the mutually consistent values of \bar{H}_i and \bar{P}_i . The pair (\bar{H}_i, \bar{P}_i) is mutually consistent if, given everything i knows, i chooses in such a way that—taking i's own choice into account—the proportion Republican that i expects among votes for the House is \bar{H}_i and the probability with which i expects the Republican presidential candidate to win is \bar{P}_i . In equilibrium, all pairs (\bar{H}_i, \bar{P}_i) , $i = 1, \ldots, N$, are mutually consistent.

I assume it is common knowledge (Fudenberg and Tirole 1991, 541-6) that every voter i has an expected loss λ_i as defined by equation 3, and that every i is choosing candidates so as to minimize λ_i . I assume that the common knowledge includes the values of all parameters, including ρ_D , ρ_R , α_D , α_R , q, b_P , b_H , b_{PH} , and any parameters in β_i , ξ_{RRi} , ξ_{RDi} , ξ_{DRi} , or ξ_{DDi} . It is then common knowledge that, for some values of the policy position and other variables, equation 5 is every voter's choice rule.

No voter knows the values of the ideal point, the party or presidential candidate policy positions, or the other variables on the basis of which any other voter is choosing via equation 5. Each voter does know the probability distribution of those values. Let Z_i be an ordered set that includes all the observable variables in $\lambda_{i,RR}$, $\lambda_{i,RD}$, $\lambda_{i,DR}$, and $\lambda_{i,DD}$. That includes θ_i , ϑ_{Di} , ϑ_{Ri} , ϑ_{PDi} , and ϑ_{PRi} ; any variables that make up β_i ; and the component z_{phi} of each ξ_{phi} , $ph \in K$, that may be observed using some generally known technology (e.g., an opinion survey). There are $M \ll$ N mutually exclusive and exhaustive groups of voters, denoted V_k , k = 1, ..., M. For every voter $i \in V_k$, the election-time value of Z_i is generated, independently across individuals, by a process that takes values in a set \tilde{Z} and has unimodal probability measure f_k , $\int_{\tilde{Z}}$ $df_k(Z_i) = 1$. Let ε_{phi} denote the component of ξ_{phi} not covered by Z_i , $ph \in K$. These variables—collectively, the disturbance $\varepsilon_i = (\varepsilon_{RRi}, \varepsilon_{RDi}, \varepsilon_{DRi}, \varepsilon_{DDi})$ —have a joint probability distribution that is the same for every voter. The joint cumulative distribution function is

$$\Pr(\varepsilon_{RR_1} < x_{RR}, \varepsilon_{RD_1} < x_{RD}, \varepsilon_{DR_2} < x_{DR}, \varepsilon_{DD_1} < x_{DD})$$

= $F_{PH}(x_{RR}, x_{RD}, x_{DR}, x_{DD}),$

where F_{PH} is a generalized extreme value (GEV) distribution, independent of each f_k . I assume that M, Z, f_k , $k = 1, \ldots, M$, the number of voters in each group (M_k) , and the fact that ε_i is identically and independently distributed as F_{PH} are all common knowledge.

A GEV distribution arises in a natural way as a model for the disturbance vector ε_r . Suppose that, as

See Maddala 1983 for an introductory discussion of GEV choice models.

⁵ Strictly speaking, to define P_i , with reference to the Electoral College, I will assume that the number of voters in each of the M groups in each state is common knowledge.

time goes by, voter *i* randomly encounters a large number of pieces of information about the alternative candidate pairs but remembers only the best piece of information about each pair. That is, the voter remembers the information about each pair of candidates that makes the loss from that pair seem the smallest. For a wide range of random processes that may generate the information each voter encounters, the distribution of the information the voter remembers about each pair is a GEV distribution (Galambos 1987, 286–314; Resnick 1987, 263–90).

I assume that the pieces of information each voter randomly encounters have a special form of dependence. Information about the two-Republican candidate pair (RR) arrives independently of information about the two-Democrat pair (DD), and the information about those two pairs arrives independently of information about either of the two mixed pairs (RD or DR); but information about one mixed pair does not arrive independently of information about the other mixed pair. In this case, ε_{RRi} is independent of ε_{DDi} , and ε_{RRi} and ε_{DDi} are both independent of ε_{RDi} and ε_{DRi} , but ε_{RDi} and ε_{DRi} vary together. The most natural way to think about this pattern is that a voter accumulates information about each party and also about the possibility of casting a split-ticket vote, but in so doing the voter tends not to distinguish sharply one split-ticket alternative from the other.

To specify the GEV distribution that such a pattern of dependence produces, start by defining

$$x_{RRI} = (b_P + b_H b_{PH}) w_{Pi} + (b_H + b_P b_{HP}) w_{Hi} + z_{RRi};$$
(6a)

$$x_{RDi} = (b_P - b_H b_{PH}) w_{Pi} + (b_P b_{HP} - b_H) w_{Hi} + z_{RDi};$$
(6b)

$$x_{DRi} = (b_H b_{PH} - b_P) w_{Pi} + (b_H - b_P b_{HP}) w_{Hi} + z_{DRi};$$
(6c)

$$x_{DD_i} = -(b_P + b_H b_{PH}) w_{P_i} - (b_H + b_P b_{HP}) w_{H_i} + z_{DD_i}.$$
(63)

Using
$$v_{phi} = \exp\{-x_{phi}\}, ph \in K$$
, define $G_i = v_{RRi} + (v_{RDi}^{1/1-\tau} + v_{DRi}^{1/1-\tau})^{1-\tau} + v_{DDi}, \qquad 0 \le \tau < 1$

Parameter τ measures the dependence between ε_{RDI} and ε_{DRI} . If ε_{RDI} and ε_{DRI} are independent, then $\tau=0$. The GEV distribution function is $F_{PH}(-x_{RRI}, -x_{RDI}, -x_{DRI}, -x_{DDI}) = \exp\{-G_i\}$.

Given Z_i but ε_i known only to have the distribution F_{PH} , the choice rule of equation 5 implies that the probability that voter i chooses each candidate pair is

$$\Pr(Y_i = ph|Z_i) = \frac{v_{phi}}{G_i} \frac{\partial G_i}{\partial v_{phi}}, \quad ph \in K \quad (7)$$

(Börsch-Supan 1990; McFadden 1978; Resnick and Roy 1990). Define $\mu_{phl} = \Pr(Y_l = ph | Z_l)$, $ph \in K$. The probability that i votes Republican for the House is $\eta_i = \mu_{RRi} + \mu_{DRi}$, and for president, $\pi_i = \mu_{RRi} + \mu_{RDi}$.

To characterize the conditions under which mutually consistent pairs (\bar{H}_i, \bar{P}_i) exist when each voter i knows all its own attributes, that is, knows Z_i and ε_i , I first consider what happens when each voter knows only to which group the voter belongs. Because M, M_k , \bar{Z} , f_k , and F_{PH} are common knowledge, no voter then has any information that would produce an expectation about the election outcome different from what the common knowledge would imply. No voter has any relevant private information. Therefore, if a set of mutually consistent pairs exists, the expectations (\bar{H}_i, \bar{P}_i) will be the same for all voters (Aumann 1976; Nielsen et al. 1990). Let (\bar{H}, \bar{P}) denote the common value that all the pairs have in this case.

Knowing only the group V_k to which voter i belongs, and therefore knowing only the range Z and probability measure f_k of the variables in Z_i , every voter determines the same probability for i to choose each candidate pair, by using f_k to integrate over the unknown data:

$$\Pr(Y_i = ph | i \in V_k)$$

$$= \int_{\tilde{Z}} \Pr(Y_i = ph \mid Z_i) df_k(Z_i), \quad ph \in K. \quad (8)$$

Define $\bar{\mu}_{phk} = \Pr(Y_i = ph | i \in V_k)$, $ph \in K$, and $\bar{\eta}_k = \bar{\mu}_{RRk} + \bar{\mu}_{DRk}$. Using f_{k_i} to denote the measure for the group to which i belongs, the expected House outcome is

$$\bar{H} = \underbrace{\int_{\bar{Z}} \cdots \int_{\bar{Z}} \left(N^{-1} \sum_{i=1}^{N} \eta_{i} \right) \prod_{i=1}^{N} df_{k_{i}}(Z_{i}),}_{N \text{ times}}$$

by common knowledge,

$$=N^{-1}\sum_{k=1}^{M}\sum_{i\in\mathcal{V}_{k}}\int_{\tilde{Z}}\eta_{i}\,df_{k}(Z_{i}), \qquad \text{by independence,}$$

$$=\sum_{k=1}^{M}\frac{M_k}{N}\bar{\eta}_k. \tag{9}$$

⁷ It is possible to specify GEV distributions in which there is also dependence among all the pairs that include a candidate from one party. That is, ε_{RR} , ε_{RD} , and ε_{DR} are dependent, or ε_{RD} , ε_{DR} , and ε_{DR} are dependent. In an analysis reported in Mebane 1998, using the same NES data, tests show neither party-based pattern of dependence to be present, as long as party identification is included among the variables in $(z_{RR}$, z_{RD} , z_{DR} , z_{DD}). Given those empirical results, I simplify the current model by ignoring the possibility of party-based dependence.

⁸ To accommodate the combination of continuous and discrete variables that Z_i may contain, the integral in $\Pr(Y_i = ph | i \in V_k)$ may be thought of as a Riemann-Stieltjes, or if necessary Lebesgue-Stieltjes, integral (Billingsley 1986, 230).

The expected probability that the Republican candidate wins the presidency is more complicated, because \bar{P} is the probability that the Republican wins a majority of the Electoral College. Let C_j denote the number of Electoral College votes for state $j, j = 1, \ldots, S$, with $\sum_{j=1}^{S} C_j = C = 538$. Let $C^{[r]}$ denote the set of all subsets s of states such that $\sum_{j \in s} C_j = r$. The expected presidential outcome⁹ is

$$\vec{P} = \frac{\sum_{r=C/2}^{C} \sum_{s \in C^{(r)}} \left(\prod_{j \in s} \bar{y}_{P_j} \right) \left[\prod_{j \notin s} \left(1 - \bar{y}_{P_j} \right) \right]}{\sum_{r=0}^{C} \sum_{s \in C^{(r)}} \left(\prod_{j \in s} \bar{y}_{P_j} \right) \left[\prod_{j \notin s} \left(1 - \bar{y}_{P_j} \right) \right]}, \quad (10)$$

where \bar{y}_{P_j} , defined in Appendix A as a function of $\bar{\pi}_k = \bar{\mu}_{RRk} + \bar{\mu}_{RDk}$, is the probability that the Republican candidate wins a majority of the vote in state j.

I assume that the functional forms of equations 9 and 10 are common knowledge. It then follows that the set of possible values of \bar{H} and \bar{P} is common knowledge. If only the group membership of each voter is known, then the mutual consistency condition requires that the \bar{H} and \bar{P} values must reproduce themselves when they are used to compute the functions $\bar{\mu}_{nhh}$, $ph \in K$, and hence equations 9 and 10. In technical terms, the pair of computed values must be a fixed point of the mapping $[0, 1] \times [0, 1] \rightarrow [0, 1] \times [0, 1]$ that equations 9 and 10 specify. In Appendix A (theorem 2) I show that such a pair (H, P) always exists (except on a set of measure zero). Necessary or sufficient conditions for \bar{H} and \bar{P} to be uniquely determined by the parameters of the model and the measures f_k are not clear.

The result when each voter i knows Z_i and ε_i may be expressed as a collection of small deviations from a fixed point (\bar{H}, \bar{P}) . Let $\bar{\eta}_{k_i}$ and $\bar{\pi}_{k_i}$ denote the group-specific probabilities for the group V_k to which i belongs. Let $\tilde{\mu}_{phi}$ indicate the value of Y_i of equation 5 when voter i knows Z_i and ε_i but for other voters has only the common knowledge: $\tilde{\mu}_{phi} = 1$ if $Y_i = ph$, $\tilde{\mu}_{phi} = 0$ if $Y_i \neq ph$, $ph \in K$. Define $\tilde{\eta}_i = \tilde{\mu}_{RRi} + \tilde{\mu}_{DRi}$ and $\tilde{\pi}_i = \tilde{\mu}_{RRi} + \tilde{\mu}_{RDi}$. The values of $\tilde{\mu}_{phi}$, $ph \in K$, depend on $H_i = H_{i\vec{\eta}_i}$ and $\bar{P}_i = \bar{P}_{l\vec{\eta}_i}$, where $H_{i\vec{\eta}_i}$ and $H_{i\vec{\eta}_i}$ are obtained by replacing one instance of $\tilde{\eta}_{k_i}$ in equation 9 with $\tilde{\eta}_i$ and one instance of $\tilde{\pi}_{k_i}$ in equation 10, via equation 18 (see below), with $\tilde{\pi}_i$:

$$\bar{H}_{i\eta_i} = \bar{H} + (\bar{\eta}_i - \bar{\eta}_k)/N, \qquad (11a)$$

$$\vec{P}_{\mathbf{rf}_i} = \bar{P} + g_i(\tilde{\pi}_i - \bar{\pi}_{\mathbf{k}}), \tag{11b}$$

where $g_i(\cdot)$ is the function that makes the adjustment from \bar{P} , $g_i(0) = 0$. Notice that, because i knows Z_i and ε_i , the vote choice is not random as far as i is concerned: Given \bar{H}_i and \bar{P}_i , one of the four choices in

K will certainly be best according to equation 5.10 An equilibrium set of choices Y_i and expectations (\bar{H}_i, \bar{P}_i) , i = 1, ..., N, is therefore given by the following.

THEOREM 1. There is a coordinating voter equilibrium if, with all voters using the same fixed point (\bar{H}, \bar{P}) computed from common knowledge, each voter i has $(\bar{H}_i, \bar{P}_i) = (\bar{H}_{H_i}, \bar{P}_{i+1})$ and $Y_i = ph$ for whichever of the four possible pairs of values $(\bar{H}_{H_i}, \bar{P}_{i+1})$ produces the smallest value of $\lambda_{i,ph}$, $ph \in K$, where $(\bar{H}_{i1}, \bar{P}_{i1})$ sets $\lambda_{i,RR}$, $(\bar{H}_{i0}, \bar{P}_{i1})$ sets $\lambda_{i,RR}$, $(\bar{H}_{i1}, \bar{P}_{i0})$ sets $\lambda_{i,RR}$, and $(\bar{H}_{i0}, \bar{P}_{i1})$ sets $\lambda_{i,RR}$.

Proof. Plainly the expectations $(\bar{H}_i, \bar{P}_i) = (\bar{H}_{i \uparrow i}, P_{i \uparrow i})$ that minimize $\lambda_{l,ph}$, $ph \in K$, match the choices voter i makes according to equation 5. And while each voter i knows that every other voter i' also has expectations $H_{i'}$ and $\bar{P}_{i'}$ as defined by equations 11a and 11b, \bar{H} and P are the best estimates of the election outcome—not taking into account the choices i will make—available to i in the absence of knowledge of the particular values of $Z_{i'}$ and $\varepsilon_{i'}$: Based on the common knowledge, the expectation of $N^{-1} \sum_{i=1}^{N} (\bar{\eta}_i - \bar{\eta}_{k_i})/N$ is zero, and $N^{-1} \sum_{i=1}^{N} g_i(\bar{\pi}_i - \bar{\pi}_k)$ converges in probability to zero as the size of the population of voters increases;11 there is no information beyond the common knowledge with which to produce estimates that have smaller variance. Since it is common knowledge that all voters are similarly situated (i.e., exchangeable), it is common knowledge that (\bar{H}, \bar{P}) is every voter's best estimate of the election outcome, not counting the voter's own Q.E.D.choices.

A MODEL FOR ESTIMATION WITH SURVEY DATA

For the coordinating voting model to be empirically estimable, the fixed point (\bar{H}, \bar{P}) that is the basis for the voter equilibrium must satisfy a condition of local stability. To understand what is needed, imagine that every voter's belief about (\bar{H}, \bar{P}) undergoes a small perturbation from the fixed-point values, say to $(\bar{H}^{(1)}, \bar{P}^{(1)})$. If each voter uses the perturbed values in equation 8 and hence evaluates equations 9 and 10, the result will in general be values $(\bar{H}^{(2)}, \bar{P}^{(2)}) \neq$

$$\begin{split} \int_{\hat{\mathbf{Z}}} \int_{\mathbf{R}^4} \left(\tilde{\mu}_{\mathbf{j}\mathbf{k}} - \bar{\mu}_{\mathbf{j}\mathbf{k}\mathbf{k}} \right) \, df_{\mathbf{E}}(\mathbf{E}_{\mathbf{i}}) \, df_{\mathbf{k}}(Z_{\mathbf{i}}) &= \int_{\hat{\mathbf{Z}}} \int_{\mathbf{R}^4} \tilde{\mu}_{\mathbf{j}\mathbf{k}} \, df_{\mathbf{E}}(\mathbf{E}_{\mathbf{i}}) df_{\mathbf{k}}(Z_{\mathbf{i}}) \\ &- \bar{\mu}_{\mathbf{j}\mathbf{k}\mathbf{k}\mathbf{k}} \int_{\hat{\mathbf{Z}}} \int_{\mathbf{R}^4} df_{\mathbf{E}}(\mathbf{E}_{\mathbf{k}}) \, df_{\mathbf{k}}(Z_{\mathbf{i}}) = \int_{\hat{\mathbf{Z}}} \mu_{\mathbf{j}\mathbf{k}} \, df_{\mathbf{k}}(Z_{\mathbf{i}}) - \bar{\mu}_{\mathbf{j}\mathbf{k}\mathbf{k}} = 0, \\ ph \in \mathbf{K}, \qquad \iota = 1, \ldots, N \end{split}$$

We have immediately that N^{-1} $\sum_{i=1}^{N}$ $\int_{\mathbf{Z}} \int_{\mathbb{R}^{4}} (\bar{\eta}_{i} - \bar{\eta}_{k_{i}})/N \ df_{\mathbf{z}}(\varepsilon_{i})$ $df_{k}(Z_{i}) = 0$ and, by Slutzsky's theorem based on the continuity of g_{i} and using $\mathbf{M} \ll N$, N^{-1} $\sum_{i=1}^{N}$ $\int_{\mathbf{Z}} \int_{\mathbb{R}^{4}} g_{i}(\mathbf{\hat{\tau}}_{i} - \bar{\pi}_{k_{i}}) \ df_{\mathbf{z}}(\varepsilon_{i}) \ df_{k}(Z_{i}) \stackrel{\Sigma}{\to} 0$.

P is the expected probability that the Republican wins or ties in the Electoral College. I ignore the complications involved in breaking a tie by votes in the House.

¹⁰ An exact tie between two $\lambda_{n,p,h}$ values, $ph \in K$, is a measure zero event that may be ignored.

¹¹ Let f_a denote the density of ϵ , on \mathbb{R}^4 (the real line four times). The

 $(\bar{H}^{(1)}, \bar{P}^{(1)})$. The fixed point (\bar{H}, \bar{P}) is locally stable if a sequence $(\bar{H}^{(1)}, \bar{P}^{(1)})$, $(\bar{H}^{(2)}, \bar{P}^{(2)})$, ... thus produced by repeated cycling through equations 8, 9 and 10 converges to (\bar{H}, \bar{P}) . Perturbations being inevitable, it is easy to see that any equilibrium that occurs in reality must have such a property: even the smallest change would set voters on a path leading rapidly away from any unstable pair (\bar{H}, \bar{P}) .

Assuming that the equilibrium does exist, that the likelihood function that defines the empirical model specification is correct, and that the variables that affect voters' choices in that specification are measured sufficiently well, the stability condition allows an iterative estimation algorithm such as the one described in Appendix A to converge to the parameter estimates that characterize the choices voters make in equilibrium. The need for the computed pair (\bar{H}, \bar{P}) to be a fixed point imposes an additional condition for the estimation algorithm to be judged to have converged. In addition to the usual conditions for having found parameter estimates that maximize the likelihood function, the estimated values (\bar{H}, \bar{P}) must be constant over successive iterations of the estimation algorithm. The requirement from the theoretical model that (H, P) be a fixed point imposes a fixed-point constraint on the maximum likelihood solution.

With NES survey data we can observe the candidate choices Y, reported by each sampled voter i and a number of variables that affect vote choices—a set of variables Z_i . We can use the observed Z_i values and a set of parameter estimates (not necessarily the maximum likelihood estimates [MLEs]) in equation 7 to compute estimated choice probabilities $\hat{\mu}_{phi}$, $ph \in$ K, for each sampled voter, and hence $\hat{\eta}_{k} = \hat{\mu}_{RR} + \hat{\mu}_{DR}$ and $\hat{\pi}_i = \hat{\mu}_{RRi} + \hat{\mu}_{RDi}$. I use such estimated probabilities to compute (H, P) for each set of parameter estimates without having to specify any particular set of groups V_k . The method is to use the sampling weight associated with each voter in each survey to estimate the totals in the formulas for \bar{H} and P. The sampling weight for voter i is proportional to $1/\omega_i$, where ω_i is the probability, determined by the sampling design, that i is included in the sample. Ratio estimates based on such weighted totals implicitly average over groups, without requiring explicit definition of the groups. To compute \hat{H} for a sample of n voters, I use

$$\hat{\bar{H}} = \frac{\sum_{i=1}^{n} \hat{\eta}_{i}/\omega_{i}}{\sum_{i=1}^{n} 1/\omega_{i}},$$
(12)

which is a design-consistent estimator that does not require any group information. 12

I use four different methods to compute \bar{P} , three of

which take the Electoral College into account. Those three methods are based on a ratio estimator of the proportion y_{Pj} of the vote the Republican presidential candidate is expected to win in each state j for which we observe data (NES samples do not have observations from every state). To define the ratio estimator, let \bar{v}_j denote the set of voters in the survey sample from state j. If \bar{v}_j is not empty, then the ratio estimator is

$$\hat{y}_{P_j} = \frac{\sum_{i \in \mathcal{V}_j} \hat{\pi} / \omega_i}{\sum_{i \in \mathcal{V}_i} 1/\omega_i}.$$
 (13)

For the first three methods to compute \hat{P} , I use functions of the \hat{y}_{Pj} values to estimate \bar{y}_{Pj} , the expected probability that the Republican wins a majority of the vote in j and therefore wins all the electoral votes from j. For states from which there are no sample observations $(\bar{v}_j = \emptyset)$, I assign all the electoral votes to the candidate who actually won the state: $\hat{y}_{Pj} = 1$ if the Republican won, and $\hat{y}_{Pj} = 0$ if the Democrat won. Let ζ_R denote the proportion of electoral votes the Republican will win. Given probabilities \hat{y}_{Pj} , its expectation is $\bar{\zeta}_R = \sum_{j=1}^{51} \hat{y}_{Pj} C_j / 538$.

Each of the first three methods uses a different approach to estimate $\Pr(\zeta_R > .5)$. Method one treats g_{Pj} directly as if it were a probability rather than a proportion, setting $\hat{y}_{Pj} = g_{Pj}$, then uses $\hat{\zeta}_R$ directly as the estimate for \bar{P} : With $\hat{y}_{Pj} = g_{Pj}$, for states with sample data, $\bar{P} = \bar{\zeta}_R$. Method two also uses $\hat{y}_{Pj} = g_{Pj}$ but defines \bar{P} as the upper tail of a beta distribution defined to match the variance of ζ_R given \hat{y}_{Pj} ; details are in Appendix A. Method three uses the sampling error of g_{Pj} to estimate $\hat{y}_{Pj} = \Pr(y_{Pj} > 5)$ via a normal approximation. Treating the parameter estimates as fixed numbers, the standard error of g_{Pj} is approximately $g_{pj} = [g_{Pj}(1 - g_{Pj})/(\Sigma_{1 \in V_i} 1/\omega_i)]^{1/2}$. Method three sets $\hat{y}_{Pj} = 1 - \Phi((5 - g_{Pj})/\sigma_{pj})$, where $\Phi(\cdot)$ denotes the standard normal cumulative distribution function, then uses \hat{y}_{Pj} in a normal approximation for $\Pr(\zeta_R > 5)$; details are in Appendix A.

The fourth method ignores the Electoral College and estimates \bar{P} as the simple proportion of votes expected to be received by the Republican: $\hat{P} = (\sum_{i=1}^{n} \frac{1}{\mu_i}/\omega_i)/(\sum_{i=1}^{n} \frac{1}{\omega_i})$. This is tantamount to using the Republican share of the (two-party) popular vote.

For two reasons, (\hat{H}, \hat{P}) is the best we can do to estimate (\bar{H}_i, \bar{P}_i) with survey data. Using $\hat{\eta}_i$ and $\hat{\pi}_i$ to estimate $\bar{\eta}_{k_i}$ and $\bar{\pi}_{k_i}$ in equations 11a and 11b would underestimate the magnitudes of $(\hat{\eta}_i - \bar{\eta}_{k_i})/N$ and $g_i(\hat{\pi}_i - \bar{\pi}_{k_i})$. And with $N > 10^8$, survey samples are too small to detect the effects of the deviations from (\bar{H}, \bar{P}) . For every sampled voter I therefore set $(\hat{H}_i, \hat{P}_i) = (\hat{H}, \hat{P})$.

Given $T \ge 1$ samples each containing n_t observations of the choices Y_t and variables Z_t , with each observation being chosen with probability ω_t from a large population $(\omega_t > 0, i = 1, ..., N_t)$ for each election year t = 1, ..., T, the parameters of the model may be estimated by maximum likelihood. Let

¹² Isaki and Fuller (1982) rigorously develop the relevant concept of consistency. See Sarndal, Swemson, and Wretman (1992, 166-85) for an overview.

Description	Theoretical Model Notation	Formal Definition	Empirical Model Notation	Formal Definition
Choice set	$K = \{RR, RD, DR, DD\}$	_	$K = \{RR, RD, DR, DD\}$	_
Voter /'s choice	Υ,	equation 5	Υ,	data
Observable effects of choices on voter /'s loss Probability that voter /	X _{RRI} , X _{RDI} , X _{DRI} , X _{DDI}	equations 6a-6d	X _{RRI} , X _{RD} , X _{DRI} , X _{DD}	eqs. 6a–6d, 17a–17d
makes each choice	PRRIO PROO PORIO PODO	equation 7	PRRI PRDI PDRI PDDI	equation 7
Common knowledge expected House election outcome	H	equation 9	Ĥ	equation 12
Common knowledge expected presidential election outcome Fixed-point condition	$P_{\ell_1} = \ell, H_{\ell_1} = j$	equation 10 Appendix A	$(\hat{\bar{H}}^{(j)},\ \hat{\bar{P}}^{(j)})=(\hat{\bar{H}}^{(j+1)},\ \hat{\bar{P}}^{(j+1)})$	four methods Appendix A

 $y_{phi} = 1$ if $Y_i = ph$ and $y_{phi} = 0$ if $Y_i \neq ph$, $ph \in K$. The log-likelihood is

$$L = \sum_{t=1}^{T} \sum_{i=1}^{n_t} \sum_{ph \in K} y_{pht} \log \mu_{pht}. \tag{14}$$

Iterations to estimate the parameter values by maximizing the log-likelihood recompute (H, P) separately for each year at each iteration, as described in Appendix A.

Table 1 displays the principal points of correspondence between the theoretical and empirical models. Both assume the same choice set, K, for each voter. The choices that each voter makes in the theoretical model are assumed to be the choices observed in the survey data. The models use the same definition, equations 6a-6d, for the observable effects that the choices voter i makes have on voter i's loss. Detailed specifications using NES data for equations 6a-6d's variables z_{ph} , $ph \in K$, appear in equations 17a-17d below. The definition of each voter's probability of making each choice, equation 7, is the same in both models. The functional forms for the expected election outcomes differ, due to the need to estimate \bar{H} and \bar{P} from survey samples, but in both models the aggregate statistics satisfy the fixed-point condition that defines the statistics' values as common knowledge in equilibrium in the theoretical model.

TESTS OF COORDINATION

I use two kinds of tests of whether voters coordinate. I check whether the estimated values of particular model parameters satisfy certain conditions necessary for coordination to exist, and I compare the performance of the coordinating voting model to a model that does not have coordination among voters but is otherwise similar.

If the president solely determines policy ($\alpha_D = \alpha_R = 1$), then there is no coordination among voters because $\tilde{\theta}_{Di} = \theta_{Di}$, $\tilde{\theta}_{Ri} = \theta_{Ri}$, and $w_{Hi} = 0$, so that

voters' strategies do not depend on \bar{P}_i or \bar{H}_i . A necessary condition for coordination is, therefore, that at least one of $\alpha_D < 1$ or $\alpha_R < 1$ is true. The test against the specification with $\alpha_D = \alpha_R = 1$ is important also because the constrained model is the familiar kind of model that includes a unidimensional spatial comparison along with nonspatial characteristics: In equations 4a-4d, $w_{Pi} = (1-\beta_i)|\theta_i - \theta_{Ri}|^q - \beta_i|\theta_i - \theta_{Di}|^q$ and $w_{Hi} = 0$. I use confidence interval estimates and likelihood-ratio (LR) tests to check whether the estimates for α_D and α_R differ significantly from the value that annihilates the possibility of coordination.

Other conditions necessary for the choice models to describe coordination are that q>0 and that both b_P and b_H are positive. If q=0, then we have the degenerate values $w_{Hi}=0$ and $w_{Pi}=1-2\beta_i$. If $b_P=b_H=0$, then whatever the discrepancies $|\theta_i-\tilde{\theta}_{Di}|^q$ and $|\theta_i-\tilde{\theta}_{Ri}|^q$ may be, they have no effect on voters' choices. The estimated parameters b_P and b_H do not equal the theoretical differences $\bar{P}_{i,R}-\bar{P}_{i,D}$ and $\bar{H}_{i,R}-\bar{H}_{i,D}$, but they are proportional to those values. So for compatibility with the derivation of $\lambda_{i,ph}$, $ph \in K$, we must have $b_P>0$ and $b_H>0$.

The alternative model to which I compare the coordinating voting model implements Fiorina's (1988; 1992, 73–81) noncoordinating (indeed, nonstrategic) theory of ticket splitting. According to that theory, each voter chooses the mix of party control of the presidency and the legislature—either unified or divided government—that would produce a policy outcome nearest the voter's ideal point, but those choices are not affected by the anticipated election results.

To focus the test as powerfully as possible on the existence or nonexistence of coordination, I formulate the noncoordinating vote choice model to resemble the coordinating model as closely as possible. I replace the terms involving w_{Pi} and w_{Hi} in the coordinating model

¹³ If $\text{var}(e_{phi}) = \frac{1}{6}\pi^2\sigma^2$, $ph \in K$ (see Johnson, Kotz, and Belakrishnan 1995, 12), then the estimated parameters correspond to $b_F = P_{i,R} - P_{i,D}$ and $b_H = H_{i,R} - H_{i,D}$ divided by σ .

with terms that are suitable for the noncoordinating theory but otherwise leave the model unchanged. In particular, for the noncoordinating model, policy outcomes under unified government are θ_{Di} or θ_{Ri} of equations 1a and 1b, while policy outcomes under divided government are

$$\begin{split} & \theta_{DR_i} = \alpha_D \theta_{D_i} + (1 - \alpha_D) \theta_{R_i}, \qquad 0 \le \alpha_D \le 1, \\ & \tilde{\theta}_{RD_i} = \alpha_R \theta_{R_i} + (1 - \alpha_R) \theta_{D_i}, \qquad 0 \le \alpha_R \le 1. \end{split}$$

 $\tilde{\theta}_{DRi}$ is the policy that voter *i* believes will occur with a Democratic president and Republican-controlled House, and $\tilde{\theta}_{RDi}$ is the policy with a Republican president and the House controlled by the Democrats. For the observed attributes of each choice define, instead of equations 6a-6d,

$$\begin{aligned} x_{RRi} &= b_{NC} \beta_{RRi} |\theta_i - \theta_{Ri}|^q + z_{RRi}; \\ x_{RDi} &= b_{NC} \beta_{RDi} |\theta_i - \delta_{RDi}|^q + z_{RDi}; \\ x_{DRi} &= b_{NC} \beta_{DRi} |\theta_i - \delta_{DRi}|^q + z_{DRi}; \\ x_{DDi} &= b_{NC} \beta_{DDi} |\theta_i - \theta_{Di}|^q + z_{DDi}. \end{aligned}$$

where $b_{NC} > 0$, and β_{RRi} , β_{RDi} , β_{DRi} and β_{DDi} are defined in Appendix A.¹⁴ The key feature of the model is that x_{RRi} , x_{RDi} , x_{DRi} , and x_{DDi} are functions of neither the expected Republican House vote proportion (\bar{H}_i) nor the expected probability that the Republican presidential candidate wins (\bar{P}_i) . Nothing in the noncoordinating model makes the choice of voter i depend on the choice or likely choice of any other voter; each voter responds solely to what each believes about the parties and the candidates.

The noncoordinating model has one more free parameter than the coordinating model does, but it does not nest the coordinating model. I use a likelihoodbased method for nonnested hypotheses (Dastoor 1985) to determine which model is superior. Let $L_{\rm C}$ and $L_{
m NC}$ denote, respectively, the log-likelihood functions for the coordinating and noncoordinating models, both having the form of equation 14, and let $L_{\rm C}$ and $L_{
m NC}$ denote, respectively, the values of $L_{
m C}$ and $L_{
m NC}$ computed using the MLEs for each model, estimated separately. If $L_{\rm C} > L_{
m NO}$ a test of whether the noncoordinating model may be rejected in favor of the coordinating model may be based on the MLE of ψ in the log-likelihood function $L_{\text{TEST}} = (1 - \psi)L_{\text{C}} +$ $\psi L_{NO} \ 0 \le \psi \le 1$. If $\hat{\psi}$ is significantly different from one, the noncoordinating model is rejected. If $L_{
m C}$ <

 $L_{
m NC}$ a test of whether the coordinating model may be rejected in favor of the noncoordinating model may be performed by reversing the roles of $L_{
m C}$ and $L_{
m NC}$ in $L_{
m TEST}$. Inability to reject either model may indicate that some voters are coordinating, with behavior best described by $L_{
m C}$ while others are moderating without coordinating, their behavior being best described by $L_{
m NC}$.

DETAILED CHOICE MODEL SPECIFICATIONS

To estimate the models I use data from the NES surveys of 1976, 1980, 1984, 1988, 1992, and 1996 (Miller and Miller 1977; Miller and the National Election Studies 1982, 1986, 1989; Miller et al. 1993; Rosenstone et al. 1997). If I pool the data over all years. Some parameters are constant over all years while others vary over years.

I use the observed responses to several seven-point (and one four-point) NES survey scales to measure θ_i , θ_{Di} , θ_{PDi} , θ_{Ri} , and θ_{PRi} . I use the variables' empirical cumulative distributions to code the responses in the [0, 1] interval. The idea is to make the responses comparable across the substantively different scales by using relative units of measurement. In formulating their theory in terms of a uniform distribution of voters' ideal points, Alesina and Rosenthal (1989; 1995, 22, 86) use relative units: The value of each ideal point and policy position corresponds to the cumulative proportion of support for that position among voters. McKelvey and Ordeshook (1985a) similarly use relative measurement of positions.

The codes I use for the NES scales are to be interpreted as measuring the proportion of all survey respondents who support a position as liberal as or more liberal than the indicated position. Each scale either refers to liberal-conservative ideological labels or pertains to a policy issue. Each of the values θ_i , ϑ_{Di} , ϑ_{Ri} , ϑ_{PDi} , and ϑ_{PRi} averages the values for the named referent-self, Democratic or Republican party, Democratic or Republican presidential candidate—over only the scales for which voter i placed all five referents on the scale. The values for different voters may therefore be computed using different subsets of the substantive scales in each survey. There is no assumption that every voter is using the same substantive policy dimension. Appendix A lists the scales used from each of the surveys and describes in detail the method used to compute codes for each scale.

I specify β_i to be a function of each voter's retrospective evaluation of the national economy. To measure economic evaluations I use a variable, EC_i, that is

¹⁴ Born's (1994a) implementation of Fiorina's theory is similar to the noncoordinating model, the differences being that Born constrains $\alpha_D = \alpha_R$; assumes uncorrelated disturbances ($\tau = 0$); fixes q = 1; uses a single, constant parameter for each of the coefficients here allowed to vary over voters via $b_{NC}\beta_{phi}$, $ph \in K$; measures ideal points and policy positions differently; and includes m z_{phi} variables to measure "Evaluation of President's Job Performance" but not retrospective economic evaluations. Born does not estimate the $\alpha_R = \alpha_D$ parameter simultaneously with the rest of his model but instead repeatedly reestimates the model for several values in the range .5 to .85.

¹⁵ Ingberman and Villani (1993) study a model in which there are both strategic and nonstrategic voters

¹⁶ Data and documentation for 1976-92 are from the "ANES 1948-1994 CD-ROM" of May 1995 (ICPSR CD0010). Data and documentation for 1996 were obtained by ftp from ICPSR in June 1997.

based on responses to a question asking whether the national economy has gotten worse or better over the past year. EC, ranges from -1 to 1; EC, =-1 corresponds to a judgment that the economy is "much worse," and EC, =1 corresponds to a judgment that the economy is "much better" (see Appendix B, 1). The definition of β_i is

$$\beta_i = (1 + \exp\{-b_{E0} - b_{E1}EC_i\})^{-1},$$
 (16)

where b_{E0} and b_{E1} are constants. One reason economic evaluations may affect β_i is that voters may believe a worsening economy calls for increased government intervention to protect or create jobs, or in other ways to help the vulnerable. A voter with such beliefs who thinks economic conditions are worsening may give more of the benefit of the doubt to the party with the stronger reputation for choosing such policies. During 1976-96, the Democratic party had a stronger reputation for choosing interventionist policies than did the Republicans (e.g., Palmer and Sawhill 1984). A voter with the indicated beliefs who thinks economic conditions are worsening may therefore weigh the policy discrepancy associated with a Republican president more heavily than the discrepancy with a Democrat $(\beta_i < .5)$. When economic conditions are improving, presumably it is the Democratic party that receives closer scrutiny ($\beta_i > .5$). Another possibility is that voters believe declining economic conditions call for reduced government intervention, or at least for a reduced rate of expansion, because that is when the country can least afford it. In this case, we should have $\beta_i < .5 \ (\beta_i > .5)$ for voters who think economic conditions are improving (getting worse).

The other observed attributes $(z_{phi}, ph \in K)$ that pertain to the vote choices include retrospective economic evaluations, individual partisanship, and House incumbent advantage.

Many have examined the effects that retrospective economic evaluations may have on vote choices (e.g., Fiorina 1981; Markus 1988). According to Alesina and Rosenthal (1995), such evaluations may be relevant to a voter's choice as an expression of two considerations: the voter's taste for macroeconomic outcomes (pp. 167-71); and the voter's judgment of the competence of the incumbent administration (pp. 191-5).17 Alesina and Rosenthal treat those considerations in the context of particular models that they specify to describe the relationship between output and inflation in the actual economy. The current analysis does not rely on any such model. Rather, I assume that each voter reports its evaluation when responding to the question used to measure EC. I multiply EC, by a variable (PP_i) that changes sign depending on the incumbent president's party: PP, = 1 if Republican; PP, = -1 if Democrat.

Party identification has long been known to affect vote choices, particularly ticket splitting (e.g., Camp-

bell and Miller 1957). Party identification is in part an index of tastes for policy outcomes and of judgments accumulated over time about the competence of the parties' successive administrations (Fiorina 1981; Franklin 1984; Franklin and Jackson 1983; Jackson 1975; Markus and Converse 1979; Page and Jones 1979). People who support different parties also tend to have different policy preferences and perceptions (Brady and Sniderman 1985). I measure party identification with six dummy variables that correspond to the levels of the NES seven-point scale measure of partisanship, using "strong Democrat" as the reference category: PID_{Di}, PID_{ID}, PID_{II}, PID_{IR}, PID_{IR}, and PID_{SRI} (see Appendix B, 2).

To take incumbent advantage into account, I use a pair of dummy variables that indicate whether a Democrat or Republican is running for reelection or whether there is an open seat: $DEM_i = 1$ if a Democrat is running for reelection in individual *i*'s congressional district, otherwise $DEM_i = 0$; $REP_i = 1$ if a Republican incumbent is running, otherwise $REP_i = 0$ (see Appendix B, 3). If $DEM_i = REP_i = 0$, then the district has an open seat. Alesina and Rosenthal (1995) use district-level data for 1950–86 to show that midterm cycle effects predicted by their theory operate pretty much independently of incumbency effects.¹⁸

To understand the functional form I use for z_{phi} , $ph \in K$, recall that in equations 6a-6d an increase in x_{phi} represents an increase in the loss voter i expects from choosing the pair of candidates $ph \in K$. G_i is specified to decrease as x_{phi} increases, via $v_{phi} = \exp\{-x_{phi}\}$, $ph \in K$, so that, by equation 7, the probability that voter i chooses a candidate pair decreases if the loss expected from choosing that pair increases. In equations 6a-6d, an increase in z_{phi} implies an increase in x_{phi} , $ph \in K$. So any variable that should increase the probability of choosing the candidate pair $ph \in K$ and that is included with an additive effect in z_{phi} should have a negative coefficient.

The functional form for z_{phi} , $ph \in K$, is

$$z_{RRt} = -c_{P0} - c_{H0} - c_{REP} REP_{i} - (c_{P1} + c_{H1}) PP_{i}EC_{i}$$

$$- c_{D}PID_{Di} - c_{ID}PID_{IDi} - c_{I}PID_{Ii} - c_{IR}PID_{IR,i}$$

$$- c_{R}PID_{Ri} - c_{SR}PID_{SR,i}; \qquad (17a)$$

$$z_{RDi} = -c_{P0} + c_{H0} - c_{DEM}DEM_{i} - (c_{P1} - c_{H1}) PP_{i}EC_{i}; \qquad (17b)$$

$$z_{DRi} = c_{P0} - c_{H0} - c_{REP}REP_{i} + (c_{P1} - c_{H1}) PP_{i}EC_{i}; \qquad (17c)$$

$$z_{DDi} = c_{P0} + c_{H0} - c_{DEM}DEM_{i} + (c_{P1} + c_{H1}) PP_{i}EC_{i} + c_{D}PID_{Di} + c_{ID}PID_{IDi} + c_{IP}PID_{Ii} + c_{IR}PID_{IR,i}$$

$$+ c_{R}PID_{Ri} + c_{SR}PID_{SR,i}, \qquad (17d)$$

¹⁷ Alesma, Roubini, and Cohen (1997) further review "rational retrospective voting" models.

¹⁸ The district-level analysis is subject to ecological inference biases.

where c_{P0} , c_{P1} , c_{H0} , and c_{H1} are coefficients constant in each year, and c_D , c_{ID} , c_I , c_{IR} , c_R , c_{SR} , c_{DEM} , and c_{REP} are coefficients constant over all years. Coefficient signs should be c_{P0} , $c_{H0} < 0$ and c_{P1} , c_{H1} , c_{DEM} , c_{REP} , c_D , c_{ID} , c_I , c_{IR} , c_R , $c_{SR} > 0$. The following examples help understand the signs expected for the coefficients.

First, consider the effects of party identification. Other things equal, we expect a strong Democrat to suffer a smaller loss from choosing Democratic candidates than from choosing Republican candidates. Ignoring the effects of incumbency and economic evaluations (that is, suppose $c_{REP} = c_{DEM} = c_{P1} = c_{H1} =$ 0), that is so in equations 17a-17d for presidential candidates if $c_{P0} < 0$; in that case $z_{DD_i} < z_{RD_i}$ and $z_{DRi} < z_{RRi}$. Likewise, it is so for House candidates if $c_{H0} < 0$; in that case $z_{DDi} < z_{DRi}$ and $z_{RDi} < z_{RRi}$. Other things equal, a strong Republican should experience a bigger loss from choosing a Democratic candidate than a strong Democrat would experience; hence, $c_{SR} > 0$. In omitting separate effects for the levels of party identification in the specifications for z_{RDI} and z_{DRI} , I am assuming that the partisan effects for presidential candidates are equal to those for House candidates, so that they cancel one another when a voter is evaluating the split-ticket alternatives.

Next consider the effects of incumbency status. Other things equal, the loss from choosing a Democratic House candidate should be smaller when that candidate is an incumbent; hence, $c_{DEM} > 0$. The motivation for $c_{REP} > 0$ is analogous.

Finally, consider the effects of retrospective economic evaluations. If there is a Republican president, one voter who thinks the economy has been going well and another voter who thinks the economy has been going badly, then a simple retrospective voting hypothesis says two things. First, other things equal, the voter who views the economy favorably should experience a smaller loss from choosing Republican candidates than does the voter with the unfavorable view. Second, other things equal, the voter with the more favorable view should experience a larger loss from choosing Democratic candidates than does the other voter. If there is a Republican president, we have $PP_{r} = 1$. A voter who thinks the economy has been getting better has $EC_i >$ 0, while a voter who thinks the economy has been getting worse has $EC_i < 0$. If $c_{P1} > 0$, then the voter with the positive view of the economy has values of z_{RR} , and z_{RD_t} that are more negative than those of the voter with the negative view. The voter with the positive view has values of z_{DRi} and z_{DDi} that are more positive than those of the other voter. If $c_{H1} > 0$, then the values of z_{RRi} and z_{DRi} are more negative for the voter with the positive view of the economy than they are for the other voter, while the values of z_{RD_i} and z_{DD_i} are more positive for the voter with the positive view than they are for the voter who views the economy unfavorably. With a Democratic president we have $PP_i = -1$, so that the orderings of the expected losses, given the voters' opinions about the economy, are reversed.

To measure vote choices Y_i (equivalently y_{phi} , $ph \in K$) I use the postelection choices reported by individuals who said they voted (see Appendix B, 4). The sample size of voters used, pooled over the six NES surveys, is 4,859 (by year, 1976–96, the sizes are 683, 627, 976, 719, 980, 874). Only those who voted for either a Democrat or a Republican for both president and House seat are included. Seven percent of such voters (386/5,245 cases) are omitted because they have data missing due to failure to measure the policy position, economic evaluation, or party identification variables (see Appendix B, 7).

MODEL ESTIMATES AND RESULTS OF TESTS OF COORDINATION

The coordinating and noncoordinating models produce similar results, but the former is superior. MLEs and standard errors (SEs) for the parameters of both models appear in Table 2.19 The coordinating model estimates in Table 2 use method two (beta approximation) to compute P. Most of the parameters that have the same interpretation in both models have statistically indistinguishable estimates. The MLEs for c_{P1} and c_{H1} in each year have the correct signs for the usual kind of retrospective voting effect in which candidates of the president's party lose votes among those who think economic conditions have gotten worse, although not all the estimates are statistically significant. The MLEs for c_{P0} , c_{H0} , c_D , c_{ID} , c_I , c_{IR} , c_R , and c_{SR} are appropriate for the usual effects of party identification—slightly larger in the noncoordinating model. The MLEs for c_{DEM} and c_{REP} point to a substantial incumbent advantage, slightly larger for Republicans than for Democrats. The function β , in the coordinating model and the corresponding functions in the noncoordinating model imply similar patterns of sensitivity to retrospective economic evaluations (discussed below).20 But the coordinating model log-likelihood ($L_{\rm C} = -3186$) is much greater than that of the noncoordinating model ($L_{\rm NC} = -3225$). Using L_{TEST} , the MLE for ψ is $\hat{\psi} = 0$, with a profilelikelihood 95% confidence interval (Barndorff-Nielsen and Cox 1994, 90) of (0, .05). The nonnested hypothesis test clearly rejects the noncoordinating model and does not reject the coordinating model.

For every year except 1976, the coordinating voting model passes the parameter-based tests of the conditions necessary for it to describe coordinating behavior. This entails rejecting the spatial model that arises when $\alpha_D = \alpha_R = 1$. Table 3 reports the LR test statistics for the constraint $\alpha_D = \alpha_R = 1$, imposed separately for

²⁰ Indeed, using the MLEs for each model, $\beta_i = \beta_{DR_i} + \beta_{DD_i}$.

¹⁹ Estimates were computed using SAS, PROC NLIN (SAS Institute 1989–95), with numerical derivatives. Over all years, the percentage correctly classified by "predicting" for each observation the pair of vote choices that has the highest probability using the parameter MLEs is 73.6% (by year: 68.7%, 67.0%, 74.4%, 75.9%, 74.6%, 78.3%). Over all years the average probability of the pair of choices actually made is .64 (by year: .58, .58, .65, .66, .65).

	Coord	Inating	Noncoor	rdinating		 Coordin 	ating	Noncoord	linating
parameter	MLE	SE	MLE	SE	parameter	MLE	SE	MLE	SE
7	1.027	.068	1.065	.098	τ	.392	.041	.371	.04
Β _Ρ .	3.916	.433	_	_	b _{E0}	.306	.111	_	_
b _{HP}	. 1.880	.492	_		b_{E1}^{20}	.478	.176	_	_
b _H	5.160	1.472	_		b _{RRE0}	_		294	.13
b _{PH}	.020	.080		<u> </u>	b _{RRE1}	_		–.717	.22
NO .	·	_	21.580	2.006	b _{RDE0}	_		096	.0
¹ D.76	1*	.150	.803	.082	b _{RDE1}	_	_	366	.1
1 _{D,80}	.405	.207	.693	.074	b _{DRE0}	_	_	.230	.0
² D,84	.774	.133	.883	.058	b _{DRE1}	_	_	193	.1:
Σ _{D,88}	.672	.158	.733	.066	C _{P0,76}	-1.054	.080	-1.128	.0
T _{D,92}	.978	.091	.849	.045	C _{P0,80}	849	.101	883	.1
² D,96	.830	.083	1*	.047	C _{P0,84}	647	.077	−.756	.0
1 _{8,76}	.757	.195	.979	.088	C _{P0,88}	895	.086	906	.0
² A,80	.949	.095	1*	.063	C _{P0,92}	891	.102	97 5	.1
¹ R,84	.544	.111	.792	.056	C _{P0,98}	-1:140	.088	-1.270	.0
1 _{R,88}	.739	.009	.915	.071	C _{H0,76}	944	.084	993	.0
~A,88 17,92	.557	.151	.873	.055	C _{H0,80}	909	.105	930	.1
~н,92 ^У R,96	.097	.257	.062	.120	C _{H0,84}	870	.081	953	.0
-н, э о _{D,76}	.991	200	1*	.174	C _{H0,88}	-1.064	.089	-1.090	.0
D,80	.861	.201	905	.199	C _{H0,92}	759	.092	−. 84 1	.0
² D,80 ⁰ D,84	.968	.226	1*	218	С _{НО,96}	854	.089	966	.0
	.783	208	.931	233	C _{P1,76}	.220	.130	209	.1
D,88	1*	.191	.890	.138	C _{P1,80}	.424	.129	.475	.1
D,92 D, 96	.742	.159	.678	.196	C _{P1,84}	.479	.128	.518	.1
-	.648	.295	.749	.239	C _{P1,88}	294	.157	.317	.1
⁰ R,76	1*	.193	1*.	.168	С _{Р1,92}	.346	.129	.355	.1
OR,80	.779	211	1*	.292	C _{P1,96}	.677	.150	.677	.1
⁰ R,84	.633	.258	.687	259	C ==	.221	.111	.114	.1
⁰ R,88	.558	.199	.861	.160	C _{H1,76}	.035	.111	.160	.1
D _{R,92}	1*	.217	.826	.314	C _{H1,80}	.008	.094	.066	.0
⁰ R,96	.902	.107	.980	.107	C _{H1,84}	.275	.123	.282	.1
D	.502 .774	.124	.836	.122	C _{H1,88}	220	.095	.190	.0
PID S	1.922	.138	2.056	.122	C _{H1,B2}	.325	.127	.404	.1
?	2.820	.128	3.022	.126	C _{H1,86}	1.015	.112	1.027	.1
PIR ,	2.884	.123	3.080	.120	C _{DEM}	1.325	.121	1.328	.1
C _R Csr	3.542	.142	3.803	.138	C _{REP}	1.020	. 12 1	1.020	• '

each year. It is rejected in every year except 1976. The 95% confidence intervals shown in Table 4 support the same conclusions. Both the LR tests and the confidence intervals in Table 4 have been adjusted in light of the fact that four parameters have MLEs on a boundary of the parameter space; see the discussion in Appendix A. Regarding the other necessary conditions, 95% confidence intervals computed as in Table 4 show q (.9, 1.1), b_P (3.1, 4.7) and b_H (2.4, 8.3) to be

Log-likelihood values: coordinating model, -3186.1; noncoordinating model, -3226.1.

positive and bounded well away from zero. As the values of $L_{\rm C}$ reported in Table 5 suggest, the coordinating model is superior to the noncoordinating model for all four methods of computing \hat{P} . The \hat{P} values from the beta approximation method are prima facie acceptable in the sense that $\hat{P} < .5$ whenever the Democratic candidate won the presidential race and $\hat{P} > .5$ whenever the Republican candidate won. The normal approximation method values for \hat{P} appear intuitively to be more plausible, in that they are more extreme—closer to zero or one—for

years such as 1984 when prospects for one of the candidates were widely believed to be hopeless. The normal approximation gives $\hat{P} > 5$ for 1976, however, and using the normal approximation produces a substantially more negative value for $\hat{L}_{\rm C}$ indicating a much worse fit to the data, than does the beta approximation method. Both the method that directly uses the expected proportion of electoral votes as \hat{P} and the method that simply uses the expected proportion of individuals votes perform virtually as well as the

²¹ The estimates from the normal approximation method are numerically the closest to the more extreme values that Alesina, Roubini, and Cohen (1997, 114–20, 135–7) obtain by using the statistics of a Brownian motion (running in reverse) to model sequences of Gallup poll results. A key assumption of the Brownian motion model, that the differences between successive polls are independent, is incompatible with the idea that the sequence of polls informs voters about the aggregate distribution of voters' preferences, and therefore it is incompatible with convergence results such as McKelvey and Ordeshook (1985a) and Nielsen et al. (1990) discuss.

TABLE 3. Likelihood-Ratio Test Statistics for the Constraint $\alpha_D = \alpha_R = 1$, by Year

Year	-2(L _{constrained} - L)
1976	1.8
1980	17.0
1984	25.4
1988	16.0
1992	12.8
1996	39.4

Note: The constraint is imposed separately for each year's α_D and α_R parameters. The critical value for a .06-level test using the χ_2^2 distribution is 5.99 (using χ_1^2 , for 1976, the critical value is 3.84).

beta approximation method in terms of their $\hat{L}_{\rm C}$ values. Both have $\hat{P} > .5$ for 1976, however.²²

The results in Table 5 may suggest that voters use proportions instead of probabilities in evaluating weighted averages such as equation 3. Perhaps it would be better to think of equation 3 as representing some kind of expected bargaining outcome, which would parallel the interpretation being applied to $H_i\theta_{Ri}$ + $(1 - \bar{H}_i)\theta_{Di}$ as the expected position of the House, rather than as an expected value. Or it may be, as many studies have suggested, that people do not use probability numbers quite as formal probability theory suggests they should. Note that during the period from September 15 to election day, the average value respondents to the 1984 NES Continuous Monitoring Survey gave for Reagan's chance of winning was .80, a value greater than the electoral vote proportion value $(\bar{P} = .70)$ but smaller than the normal approximation value ($\bar{P} = .93$) (see Appendix B, 8).

MODERATION AND INSTITUTIONAL BALANCING

With expected postelection policies θ_{D_l} and θ_{R_l} defined as in equation 2, moderation is almost always a feature of every voter's choices in the coordinating voting model. Unless $\alpha_D = \alpha_R = 1$, every voter intends to produce a policy outcome that is an intermediate combination of the parties' positions. The estimates for \bar{H} , in Table 5, show the expected position of the House, $\bar{H}\theta_{R_l} + (1 - \bar{H})\theta_{D_l}$, always to have been close to the midpoint between the parties' positions. The House position was expected to be somewhat closer to the Democratic position in 1976, 1984, 1988, and 1992, somewhat closer to the Republican position in 1996, and almost exactly at midpoint in 1980.

Such moderation, which is essentially built into the definitions of $\tilde{\theta}_{Di}$ and $\tilde{\theta}_{Ri}$, has no direct implication for the number of voters who may have been splitting their tickets to try to balance the House position with

that of the future president. One point that bears on the question of such institutional balancing is the fact that the values of \bar{P} and \bar{H} in Table 5 are positively, not negatively, correlated. The productmoment correlation is .09.23 One reason for the positive correlation is suggested by the MLEs for b_{HP} and b_{PH} . The latter is near zero and statistically insignificant, but $b_{HP} = 1.9.24$ Recall that, theoretically, $b_{HP} =$ dH/dP_{ν} , which should be interpreted as a relation among mutually consistent, equilibrium pairs (\bar{H}_i, \bar{P}_i) , $i = 1, \ldots, N$. Indeed, given equations 11a and 11b we have $b_{HP} = dH/dP$, a function of the common knowledge. The positive estimate for the derivative suggests that when the equilibrium value of \bar{P} increases, the value of \bar{H} that is mutually consistent with it tends to increase as well: An increase in the equilibrium expected probability that the Republican will win the presidency induces an increase in the equilibrium expected Republican proportion of the House vote.25 In contrast, the estimate of virtually zero for $b_{PH} =$ $d\bar{P}/d\bar{H}$ suggests that an increase in the expected Republican proportion of the House vote has no effect on the expected probability of a Republican presidential victory. What we have in this asymmetric relationship between expected presidential and House voting outcomes is a presidential coattail effect (Alesina and Rosenthal 1995, 104; Calvert and Ferejohn 1984; Ferejohn and Calvert 1983), most notably a coattail effect that characterizes the relationship between different equilibrium election outcomes.

The positive relationship between \bar{P} and \bar{H} reflects the patterns in which changes have occurred across election periods in ideal points, party positions, partisanship, economic evaluations, and incumbency. To assess the prevalence of balancing behavior, we need to focus on the motivation each voter has that arises solely from the policy-related components of $\lambda_{i,ph}$, $ph \in K$. In the notation of equations 6a-6d, those components are $(x_{phi} - z_{phi})$. Voter i has a policy-based incentive to balance the House with the president (or, equivalently, the president with the House) if one of the two split-ticket alternatives minimizes the policy-related component of the effect the voter's choice has on the voter's expected loss, that is, if $\underset{phi}{\operatorname{argmin}}_{ph \in K}(x_{phi} - z_{phi}) \in \{RD, DR\}$.

A necessary condition for voter i to have a policy-related incentive to balance is that the voter's ideal point falls between the policy positions the voter

²² Except for 1976, the simple vote proportions in Table 5 have values somewhat more extreme than the actual Republican proportion of the two-party popular vote, reflecting the overreporting of votes for the winner that occurs in NES postelection data. In order by year, the actual proportions are .489, .553, .592, .539, .465, and .453 (Miller 1997).

²³ With 1996 omitted, the correlation is .85. These correlations are for the beta approximation value of \tilde{P} . Results for the other methods are similar.

²⁴ The 95% confidence interval for b_{HP} , computed as in Table 4, is (9,2.8). ²⁵ It is important to keep clear that b_{HP} refers to local variations among equilibria. Numerical computations using the NES data and the parameter MLEs of Table 2 show that $\partial \hat{P}_{\ell,j}/\partial j < 0$ and $\partial \hat{H}_{\ell,j}/\partial \ell < 0$ for ℓ , $j \in (0,1)$. Moreover, the values $\hat{P}_{\ell,j} = \ell$ are decreasing in j, and the values $\hat{H}_{\ell,j} = j$ are decreasing in ℓ for ℓ , $j \in (0,1)$ (see Appendix A for the ℓ , j notation). The assumption of global linearity that $b_{HP} = d\hat{H}/d\hat{P}$ reflects is restrictive.

TABLE 4.	95% Confidence In	tervals for $lpha_D$ and $lpha$	$\alpha_{\!R}$		
Parameter	Lower Bound	Upper Bound	Parameter	Lower Bound	Upper Bound
$\alpha_{D,76}$.79	1*	α _{R.76}	.38	1*
$\alpha_{D.80}$.03	.83	$\alpha_{R.80}$.76	1*
$\alpha_{D.84}$.52	1*	$\alpha_{R.84}$.34	.79
$\alpha_{D,88}$.36	.98	$\alpha_{R.88}$.54	.99
$\alpha_{D,92}$.80	1*	$\alpha_{R,92}$	<i>.</i> 27	.89
$\alpha_{D,96}$.67	1*	$\alpha_{R,96}$	0 *	.66

Note: Estimates are based on tabulation of an asymptotic mixture distribution of the kind derived in Moran (1971) and Self and Liang (1987), under the hypothesis $\alpha_{D,76} = \rho_{D,92} = \rho_{B,90} = \rho_{B,90} = 1$. Asteriek indicates a boundary-constrained value.

expects the parties to act on after the election; that is, either $\theta_{Ri} > \theta_i > \theta_{Di}$ or $\theta_{Di} > \theta_i > \theta_{Ri}$. Table 6 shows the percentage of voters who have such an ordering in each year in the NES data. The figure jumps from 29% in 1976 to 45% in 1980, then varies between 43% and 52% in subsequent years. The number of voters having a policy-related incentive to balance is therefore potentially quite large.

But only a minority of those voters who could have such an incentive, given θ_i , θ_{Di} , and θ_{Ri} , actually do have such an incentive once the presidential candidates' anticipated strengths in relation to the House $(\alpha_D \text{ and } \alpha_R)$ and the expected election outcome (\bar{H}) and P) are taken into account. Table 7 shows the distribution of vote choices that minimize x_{phi} z_{phi} , $ph \in K$, in each year. The voters who have a policy-related incentive to balance are those for whom either $x_{RDi}-z_{RDi}$ or $x_{DRi}-z_{DRi}$ is the smallest of the four values of $x_{phi}-z_{phi}$, $ph\in K$. The percentage of such voters is smaller than 20% in every year and is less than or equal to 10% in three of the six years (1980, 1988, and 1996). Such percentages match the predictions of Alesina and Rosenthal's (1995, 103) theoretical model, in which only a small proportion of the voters who have ideal points between the positions of the parties are predicted to split their tickets in the presidential election year.

Partisanship, economic evaluations, and incumbency frequently outweigh policy-related balancing considerations in the overall determination of whether a voter casts a split-ticket vote. Moreover, the estimate for τ (95% confidence interval (.31, .47)) is large enough to suggest that a fair number of split-ticket votes are significantly motivated by other considerations that either enhance or reduce the appeal of both split-ticket alternatives without sharply distinguishing between them. Of those found to have a policy-related balancing incentive in the NES data, over all years, only 31% actually split their ticket. Only 22% of those who did not have a policy-related balancing incentive split their ticket. The Pearson chi-squared statistic for independence in the cross-classification of split-ticket versus straight-ticket votes by split-ticket versus straightticket policy-related incentives (measured $\operatorname{argmin}_{ph \in K}(x_{phi} - z_{phi})$ is 18.8 with 1 degree of freedom, which indicates a statistically significant association at any conventional test level.²⁶ Factors that may have little or nothing to do with a voter's policy preferences substantially affect the voter's choices. But both the number of voters who have a policy-related balancing incentive and the degree to which that incentive affects their choices are large enough to support a conclusion that policy-related balancing has often been an important determinant of election outcomes.

CONCLUSION

Recent American elections have featured coordination among voters, based on voters' intentions to produce postelection policy moderation between the president and the House. Inspired by policy concerns that have been mediated through a coordination-with-moderation mechanism, a small but significant proportion of voters have been motivated to vote a split ticket in order to increase the chances of institutional balance. Perhaps not in 1976, but in each presidential year election from 1980 through 1996, each voter's choices between the major party candidates for president and the House have been constrained by the anticipated election result, that is, by the aggregate result of the choices all other voters were about to make. In recent elections the strategies and beliefs of different voters have been strongly tied together.

For the most part, moderation has been based on voters' expectations that the president will be at least an equal of the House in determining postelection policy. Most of the α_D and α_R point estimates are greater than .5 (see Table 2), which suggests that voters usually believe the president to have more weight in policy outcomes than does the House. Jimmy Carter running for reelection in 1980 is an exception ($\hat{\alpha}_{D,80}$ = .4), but the most striking case of anticipating a weak president is Bob Dole in 1996 ($\hat{\alpha}_{R,96}$ = .1). One interpretation is that voters expected a Dole presidency to be driven by the centralized and confrontational House of Speaker Newt Gingrich.

²⁶ Overall, the coordinating voting model does not do especially well in classifying split-ticket voters separately from straight-ticket voters. Of the 597 voters "predicted" to split their ticket, 48% did so; of the 1,101 voters who split their ticket, only 26% were "predicted" to do so.

TABLE 5. Expected Probability that Republican President Wins (\bar{P}) and Proportion Republican in National House Vote (\bar{H}), for Alternative Estimates of \bar{P} , by Year

	Beta Approximation		Normal Approximation ^b		Electoral Vote Proportion ^o		Simple Vote Proportion ^d	
Year	Ê	Ĥ	Ê	Ĥ	Ê	Ĥ	Ê	Ĥ
1976	.497	.465	.557	.485	.525	.466	.515	.466
1980	.618	.505	.821	.502	.643	.503	.613	.505
1984	.620	.473	.933	.470	.695	468	.608	.473
1988	.540	.453	.740	.451	.628	.447	.553	.452
1992	.416	.431	.183	.431	.425	.431	.429	.431
1996	.430	.537	.317	.541	.431	.537	.448	.536
Ĺ	-31	86.1	-32	01.1	-31	86.8	-31	86.4

Note: Computed using the parameter MLEs for each method of computing \hat{P} and 1976–96 NES data. MLEs for the beta approximation method appear in Table 2.

In at least one respect, the moderating mechanism has worked to the disadvantage of Democratic candidates. The estimates for the weight variable β , (see equation 16) suggest that the Democratic party cannot choose policies that are more extreme than those of the Republican party without suffering electoral disadvantage, but the Republican party has often not been similarly restricted. Voters who think economic conditions have worsened treat the discrepancies between their ideal points and the policies they expect will occur with each party's president about equally: Someone who says the economy is "worse" has $\beta_i = .52$, and someone who says "much worse" has $\beta_i = .46.27$ But voters who think economic conditions have improved put much more weight on the discrepancy with a Democratic president: Someone who says the economy is "better" has $\beta_i = .63$, and someone who says "much better" has $\beta_i = .69$. Such a pattern is compatible with voters treating the Democratic party more skeptically in times of prosperity, because of its interventionist reputation, but not giving it any particular advantage when times are tough. Whatever its origin, the asymmetric treatment of the parties means that the Democrats have had less leeway than the Republicans to choose policy positions that are not at the center of the distribution of voters' ideal points.

The coordinating voting model is underdeveloped in at least one crucial respect: The model says nothing about how the equilibrium described by the fixed-point pair (\bar{H}, \bar{P}) and other common knowledge might come about. It is unreasonable to think that voters spontaneously agree on the equilibrium values. Indeed, it is unreasonable to believe that each voter independently keeps track of all the information needed to define and sustain an equilibrium. A coordinating voting equilibrium can exist only if institutions exist that can aggregate and broadcast the information everyone needs to know.

Such institutions exist. McKelvey and Ordeshook

show that polls (1984, 1985a) or interest groups and history (1985b) may support rational expectations voter equilibria even when many voters are poorly informed. It should be possible to build such institutions directly into a model much like the current one, with similar empirical implications but with weaker assumptions about what each voter knows about the distribution of voters. 28 More difficult to model would be an institution that brings everyone to agreement about the current values of the model's parameters. Informally it is reasonable to say that this may happen during the electoral campaign, but it is difficult to say anything specific without invoking assumptions at least as strong as the direct assumption that the parameters are common knowledge. 29

On the whole, it seems reasonable to say that the equilibria here modeled with virtually no institutional foundations are in reality sustained by a collection of institutions that may informally be summarized as "the campaign," "polls," and "pundits." The campaign car-

Text method two.

b Text method three.

^o Text method one.

^d Text method four.

²⁷ The values for β_i are computed using the MLEs for b_{E0} and b_{E1} .

²⁸ For instance, suppose the decision rule of equation 5 is common knowledge, but no voter knows the distribution of other voters' preferences, that is, no one knows the probability measures f_k or the distribution F_{PH} . If the unknown distributions are such that $\partial P_{\epsilon}/\partial j < 0$ and $\partial H_{\epsilon}/\partial \ell < 0$ for ℓ , $j \in (0, 1)$, then a sequence of preelection polls will converge to a fixed point (H, P). Nielsen et al. (1990) discuss convergence for a scalar aggregate statistic. To achieve convergence to a neighborhood of (H, P), it is not necessary for every voter to learn the result of every poll, nor for every voter to have the same probability of learning about each poll. If under the common knowledge assumptions of the current model the fixed point (H, P) would be unique and stable for the given population of voters, and if each voter has a positive probability of learning about each poll and updates consistently, then Bowden's (1987) results imply that aggregate pre-election poll results evolve according to a Markov process that converges to a stationary distribution with mean (\bar{H}, \bar{P}) . It seems obvious that if each voter uses Bayesian updating based on a suitable likelihood, then the mode of the distribution of each voter's beliefs converges over time to (\bar{H}, \bar{P}) (cf. McKelvey and Page 1990). The process may not converge by the time of any particular election, however.

²⁰ For instance, at least as strong would be an assumption that all voters have a common knowledge prior distribution for the parameters, from which they then update based on public campaign information.

Orderings of Ideal Points and Expected Party Policy Positions, by Year TABLE 6. Ordering θ_{Di} , $\theta_{Ri} > \theta_{i}$ Year $\theta_{D_l} > \theta_l > \theta_{R_l}$ $\theta_{R_l} > \theta_l > \theta_{D_l}$ $\theta_{l} > \theta_{Dl}, \; \theta_{Rl}$ $\theta_{I} = \theta_{DI} = \theta_{RI}$ 53.1 1.0 1976 2.9 25.9 17.1 1980 2.7 42.4 21.0 33.7 0.2 24.9 0.0 1984 1.8 50.5 22.8 -0.3 25.2 46.1 26.6 1988 1.7 2.9 39.7 29.2 27.5 0.8 1992 0.6 1996 3.9 44.0 29.1 22.5

Note: Entres show the percentage of voters in each year who have the indicated ordering of ideal point and expected party policy positions. Computed using the parameter MLEs in Table 2 and 1976-96 NES data. Each observation is weighted by the sampling weight 1/4.

ries the processes through which the expectations here modeled as (\bar{H}, \bar{P}) and the party and governmental struggles here modeled using parameters such as ρ_D , ρ_R , α_D , and α_R converge to equilibrium values. Polls and pundits allow each voter to monitor the expectations and struggles for power as they evolve, without the voter having to exert much personal effort. To learn the candidates' chances in the presidential race and the expected outcome in House races across the country, a voter need only invest a little time listening to pollsters say what they think will happen. Brief attention to media commentators may let a voter know how strong each presidential candidate may be expected to be, if elected, relative to the candidate's party and to the legislature. Having so easily learned the commonly accepted expectations for the election outcome and for the power relationships, all a voter then needs in order to coordinate with all other voters is knowledge of the components of the voter's own personal preferences: the voter's ideal point and what the voter thinks are the relevant policy positions of the parties and candidates; the voter's partisanship and evaluation of the economy; the presence or absence of an incumbent in the House race in the voter's district; and other factors here treated as random. If sufficient institutional support exists, it is not difficult for voters to coordinate.

TABLE 7. Vote Choices That Minimize the Effect on Expected Policy-Related Loss, by Year

	$\operatorname{argmIn}_{\rho h \in K}(x_{\rho h i} - z_{\rho h i})$					
Year	, RR	RD	DR	DD		
1976	50.0	17.2	0.0	32.9		
1980	60.3	0.8	6.0	. 32.9		
1984	40.7	14.7	0.0	44.6		
1988	57.6	1.3	8.7	32.4		
1992	35.8	10.2	0.7	53.2		
1996	45.5	5.6	0.1	48.8		

Note Entree show the percentage of voters in each year for whom the indicated vote choice imminizes $x_{DN} - x_{DN}, ph \in K$, which is the polecy-related component of the effect the voter's cohece has on the voter's expected loss. Computed using the parameter MLEs in Table 2 and 1978–98 NES data. Each observation is weighted by the sampling weight $1/a_F$. Voters with $\theta_I = \theta_{DI} = \theta_{RI}$ are omitted

APPENDIX A. PROOFS AND TECHNICAL MEASUREMENT ISSUES

Effects of Candidate Choices on a Voter's Expected Loss

Using $d\beta_i/d\bar{P}_i = d\beta_i/d\bar{H}_i = 0$, we have $d\lambda_i/d\bar{P}_i = w_{Pi} + w_{Hi}d\bar{H}_i/d\bar{P}_i + d\xi_i/d\bar{P}_i$ and $d\lambda_i/d\bar{H}_i = w_{Hi} + w_{Pi}d\bar{P}_i/d\bar{H}_i + d\xi_i/d\bar{H}_i$. The other variables used in equations 4a-4d are $\xi_{RRi} = b_P d\xi_i/d\bar{P}_i + b_H d\xi_i/d\bar{H}_i$, $\xi_{RDi} = b_P d\xi_i/d\bar{P}_i - b_H d\xi_i/d\bar{H}_i$, $\xi_{DRi} = -b_P d\xi_i/d\bar{P}_i + b_H d\xi_i/d\bar{H}_i$, and $\xi_{DDi} = -b_P d\xi_i/d\bar{P}_i - b_H d\xi_i/d\bar{H}_i$.

Probability That the Republican Wins the Presidency (Using Only Group Information)

Let V_{jk} be the set and $N_{jk} = |V_{jk}|$ the number of voters in state j who are members of group V_k . $\bar{V}_j = \bigcup_{k=1}^M V_{jk}$ is the set of all voters in state j, and the total number of voters in the state is $N_j = \sum_{k=1}^M N_{jk}$. Let $\bar{V}_j^{(r)}$ denote the set of all subsets of \bar{V}_j of size r. If Z_i is known for all i, then the probability that the Republican gets a majority of the votes in state i is

$$\begin{split} \bar{y}_{J_{j}}^{*} &= \frac{\sum_{r=\lceil N_{j}/2 \rceil}^{N_{j}} \sum_{r \in \bar{V}_{j}^{(*)}} \left(\prod_{r \in s} \pi_{i} \right) \left[\prod_{r \in V_{j} \setminus s} \left(1 - \pi_{i} \right) \right]}{\sum_{r=0}^{N_{j}} \sum_{r \in \bar{V}_{j}^{(*)}} \left(\prod_{r \in s} \pi_{i} \right) \left[\prod_{r \in V_{j} \setminus s} \left(1 - \pi_{i} \right) \right]} \\ &= \frac{\sum_{r=\lceil N_{j}/2 \rceil}^{N_{j}} \sum_{r \in \bar{V}_{j}^{(*)}} \prod_{k=1}^{M} \left(\prod_{r \in s_{k}} \pi_{i} \right) \left[\prod_{r \in V_{jk} \setminus s_{k}} \left(1 - \pi_{i} \right) \right]}{\sum_{r=0}^{N_{j}} \sum_{r \in \bar{V}_{j}^{(*)}} \prod_{k=1}^{M} \left(\prod_{r \in s_{k}} \pi_{i} \right) \left[\prod_{r \in V_{jk} \setminus s_{k}} \left(1 - \pi_{i} \right) \right]}, \\ s_{jk} = s \cap V_{jk}. \end{split}$$

The probability that the Republican candidate gets half or more of the Electoral College votes is

$$\bar{P}^{\star} = \frac{\sum_{r=C/2}^{C} \sum_{s \in C/2} \left(\prod_{j \in s} \bar{y}_{j_{j}}^{\star} \right) \left[\prod_{j \in s} \left(1 - \bar{y}_{j_{j}}^{\star} \right) \right]}{\sum_{r=0}^{C} \sum_{s \in C/2} \left(\prod_{j \in s} \bar{y}_{j_{j}}^{\star} \right) \left[\prod_{j \in s} \left(1 - \bar{y}_{j_{j}}^{\star} \right) \right]}.$$

Replacing each π_l in $\bar{y}_{P_l}^*$ with the group-specific probability $\bar{\pi}_{P_l}$ $i \in V_{P_l}$ gives \bar{P} of equation 10 with

$$\bar{y}_{j,j} = \frac{\sum_{r=[N_j/2]}^{N_j} \sum_{r\in\bar{V}_j^{(r)}} \prod_{k=1}^{M} \bar{\pi}_k^{[s_k]} (1-\bar{\pi}_k)^{[V_{jk}\setminus s_{jk}]}}{\sum_{r=0}^{N_j} \sum_{r\in\bar{V}_j^{(r)}} \prod_{k=1}^{M} \bar{\pi}_k^{[s_k]} (1-\bar{\pi}_k)^{[V_{jk}\setminus s_{jk}]}},$$

$$s_{jk} = s \cap V_{jk}. \tag{18}$$

Since each Z_i is independent and $M \ll N$, Slutzky's theorem implies that $\int_{\bar{Z}} \cdots \int_{\bar{Z}} \bar{P}^* \prod_{i=1}^N df_{k_i}(Z_i)$ converges in probability to \bar{P} as $N \to \infty$ in any way such that, for every j and k, $N_{jk} \to \infty$ or $N_{jk} = 0$. In terms of asymptotic approximation error.

$$\underbrace{\int_{\bar{Z}} \cdots \int_{\bar{Z}} \bar{P}^{+} \prod_{i=1}^{N} df_{k_{i}}(Z_{i}) = \bar{P} + O_{P}\left(\max_{j,k \ N_{jk} \neq 0} N_{jk}^{-1/2}\right)}_{N \text{ times}}.$$

 \bar{P} therefore differs negligibly from $\int_{\bar{Z}} \cdots \int_{\bar{Z}} \bar{P}^{+} \prod_{i=1}^{N} df_{k}(Z_{i})$ if N_{ik} is large for all j and k.

Existence of Fixed-Point Pairs (H, P)

Let Ψ be a bounded set of positive Lebesgue measure that is the parameter space for $\lambda_{\iota,p,h}$, $ph \in K$. Let $\bar{\pi}_{k\ell_J}$ and $\bar{\eta}_{k\ell_J}$ denote the values of $\bar{\pi}_k$ and $\bar{\eta}_k$ when evaluated using $P = \ell$ and $\bar{H} = \mathfrak{J}$ for some $\ell \in [0,1]$ and some $\mathfrak{J} \in [0,1]$. Let \bar{P}_{ℓ_J} denote the value of \bar{P} as defined by equation 10 when $\bar{\pi}_k = \bar{\pi}_{k\ell_J}$, and let \bar{H}_{ℓ_J} denote the value of \bar{H} as defined by equation when $\bar{\eta}_k = \bar{\eta}_{k\ell_J}$.

THEOREM 2. For almost every $\psi \in \Psi$, there exist values ℓ , $j \in (0, 1)$ such that, simultaneously, $P_{\ell_1} = \ell$ and $\bar{H}_{\ell_2} = j$.

Because $\bar{\pi}_{k\ell_1}$ varies continuously as a function of ℓ for each k and P_{ℓ_1} is a continuous function of the $\bar{\pi}_{k\ell_1}$ values, there is at least one fixed point of P_{ℓ_1} as a function of ℓ for each fixed value of j, that is, there is at least one value ℓ such that \bar{P}_{ℓ_1} ℓ . Because $\bar{P}_{0,1} > 0$ and $\bar{P}_{1,1} < 1$, the number of fixed points is odd, except on a subset of Ψ of measure zero. For q>1there is similarly an odd number of fixed points $\bar{H}_{\ell_1} = \jmath$ for each ℓ . If q=1, the step functions in $w_{H_{\ell}}$ imply that H_{ℓ_1} does not vary continuously as a function of j, so if q = 1 there is not necessarily a fixed point $H_{\ell_1} = j$. If 0 < q < 1, then there is a discontinuity at infinity in w_{Hi} , whenever $\theta_i = \theta_{Di}$ or $\theta_i = \theta_{Ri}$. But for a finite number N of voters, the number of points, of discontinuity in both cases is finite.31 So the values of ℓ for which there is no fixed point $H_{\ell_1} = 1$ form a set of measure zero: for any $\varepsilon > 0$ there is a value ℓ' , $|\ell - \ell'|$ $< \varepsilon$, such that there is a fixed point $\bar{H}_{\ell'j} = j$ for some $j \in [0, 1]$ 1]. Now consider the graph of the set of fixed points $P_{\ell_1} = \ell$ as J varies over [0, 1] and the graph of the set of fixed points $\bar{H}_{\ell_1} = 1$ as ℓ varies over [0, 1]. When q > 1 both graphs are continuous and so necessarily intersect. That is, there is necessarily at least one pair of values (ℓ, j) such that $P_{\ell, j} = \ell$ and $H_{\ell_1} = J$. If $0 < q \le 1$, then each graph is in general not continuous, because of the discontinuities in w_{Hi} , but each graph does have a finite number of continuous components. In this case, the graphs need not intersect. But because the number of points of discontinuity is finite, the set of parameter values for which there is no intersection has measure zero. For finite N > 0, the argument holds for arbitrary finite sets of probability measures f_k .

Q.E.D.

Survey-Based Probabilities That the Republican Wins the Presidency (P)

Let C_{NR} denote the number of electoral votes assigned to the Republican according to the $\bar{v}_j = \emptyset$ convention, and let C_{ND} denote the number thus assigned to the Democrat. The proportion of the remaining electoral votes that the Republican needs to win is $\zeta_{\pi} = (269 - C_{NR})/(538 - C_{NR} - C_{ND})$. Beta approximation method. For states with sample data, set $\hat{y}_{P_j} = \hat{y}_{P_j}$. Compute the variance of ζ_R as $\sigma^2_{\xi_R} = (\Sigma_{j-1}^S \ C_j^2)^{-1} \ \Sigma_{j-1}^S \ \hat{y}_{P_j} (1 - \hat{y}_{P_j}) C_j^2$. To evaluate $\Pr(\zeta_R > .5)$, use the beta distribution on [0, 1] obtained by matching moments:

$$\hat{\vec{P}} = \int_{L_{\pi}}^{1} \frac{\zeta_{R}^{p_{1}-1}(1-\zeta_{R})^{p_{2}-1}}{B(p_{1},p_{2})} d\zeta_{R}, \tag{19}$$

where $p_1 = \bar{\zeta}_R^2 (1 - \bar{\zeta}_R)/\sigma_{\xi_R}^2 - \bar{\zeta}_R$ and $p_2 = \bar{\zeta}_R (1 - \bar{\zeta}_R)^2/\sigma_{\xi_R}^2 - (1 - \bar{\zeta}_R)$ match the mean $\bar{\zeta}_R$ and variance $\sigma_{\xi_R}^2$ (Johnson, Kotz, and Balakrishnan 1995, 222), and $B(p_1, p_2)$ denotes the beta function. If $p_1 \leq 0$ or $p_2 \leq 0$, then the beta distribution is inappropriate. Normal approximation method. Set $\hat{y}_{p_1} = 1 - \Phi((.5 - \hat{y}_{p_1})/\sigma_{y_1})$ and evaluate $\Pr(\zeta_R > .5)$ as $\bar{P} = 1 - \Phi((\zeta_R - \bar{\zeta}_R)/\sigma_{\xi_R})$.

Iterative Computation of MLEs and Expectations (\hat{H}, \hat{P})

Given estimates for the parameters and estimates $\hat{H}^{(i)}$ and $\hat{P}^{(i)}$ from iteration step ι , use equation 7 to compute probability values $\hat{\mu}_{PAI}^{(i)}$, $ph \in K$, and then $\hat{\pi}_{i}^{(i)}$ and $\hat{\eta}_{i}^{(i)}$ for each voter. For each state j with $\bar{\nu}_{i} \neq \emptyset$ in the referent year, use equation 13 to compute $\hat{y}_{P_{j}}^{(i)}$ and hence compute an updated estimate $\hat{P}^{(i+1)}$ (e.g., using mean $\bar{\zeta}_{k}^{(i)}$, variance $\sigma_{kk}^{(i)}$, and equation 19). Use equation 12 to estimate $\hat{H}^{(i+1)}$, with n being the total sample size of voters for the referent year. Use $\hat{H}^{(i+1)}$ and $\hat{P}^{(i+1)}$ and the parameter values of step ι to apply one step of Gauss-Newton maximization to the log-likelihood, in order to get new parameter values for use in iteration $\iota + 1$. Iterate until, for successive iterations ι and $\iota + 1$, $(\hat{H}^{(i)}, \hat{P}^{(i)}) = (\hat{H}^{(i+1)}, \hat{P}^{(i+1)})$ and the parameter estimates satisfy the first- and second-order conditions for a local maximum of the log-likelihood. In each Gauss-Newton step, each row of the Jacobian matrix of the parameters is weighted by $1/\omega_{i}$, 3^{2} , $1/\omega_{i}$ is scaled to sum to the total sample size \hat{n} in each year, where \hat{n} includes all survey respondents, both voters and nonvoters (see Appendix B, 5).

For a fixed set of parameter estimates, the sequence $(\hat{H}^{(1)}, \hat{P}^{(1)})$, $(\hat{H}^{(2)}, \hat{P}^{(2)})$, \cdots converges if, to extend the notation of the paragraph on the existence of fixed-point pairs, $\hat{P}_{\ell_{J}}$ is decreasing in J and $\hat{H}_{\ell_{J}}$ is decreasing in ℓ for J, $\ell \in (0, 1)$.

 $^{^{30}}$ Confine $\alpha_D,\,\alpha_R\in[0,\,1).$ As discussed in the text, $\alpha_D=\alpha_R=1$ is incompatible with coordination.

³¹ If the ideal point and policy position variables are continuous, then the set of discontinuous has measure zero.

²² In PROC NLIN I used _WEIGHT_ to weight the Jacobian matrix (SAS Institute 1990, 1148).

Estimates on a Boundary of the Parameter Space

Table 2 shows $\alpha_{D,76}$, $\rho_{D,92}$, $\rho_{R,80}$, and $\rho_{R,96}$ to have MLEs equal to 1.0, on the conceptual boundary of the parameter space. The boundary is not a "natural" boundary (Moran 1971b) for the probability model: $L_{\rm C}$ is a proper (although meaningless) likelihood for values of α_D , ρ_D , or ρ_R outside [0, 1]. The on-boundary values do not imply problems with the consistency of the MLEs (Moran 1971a): The model is identified and the MLEs globally maximize $L_{\rm C}$ subject to $0 \le$ α_D , α_R , ρ_D , $\rho_R \le 1$. But the MLEs' asymptotic distribution is complicated (Moran 1971b; Self and Liang 1987). To regularize the model for the LR tests, in all the constrained models I fix equal to 1.0 the parameters with on-boundary MLEs. With this method the test statistic for $\alpha_{D.76}$ = $\alpha_{R,76} = 1$ has only one degree of freedom. None of the statistics in Table 3 are close to the critical value for a test based on χ_2^2 (χ_1^2 for 1976), so slight variations from χ_2^2 in the statistics' distributions would not change any conclusions. Unfortunately, it is computationally infeasible to bootstrap the LR test statistics.

The asymptotic distribution of the MLEs when parameters fall on a boundary of the parameter space is a mixture of censored multivariate normal distributions (Moran 1971b; Self and Liang 1987). For the coordinating voting model, the hypothesis $\alpha_{D,76} = \rho_{D,92} = \rho_{R,80} = \rho_{R,96} = 1$ implies a distribution that is a mixture of 16 censored distributions. I use a bootstrap (20,000 resamples) of the score vectors associated with the MLEs of Table 2 to tabulate that mixture distribution and estimate the confidence intervals of Table 4. With a few substantial exceptions (e.g., $\alpha_{R,88}$) the confidence intervals do not differ greatly from the intervals obtained by using normal theory with the MLEs and SEs in Table 2 (i.e., MLE \pm 1.96 SE).

Comparison Weights in the Noncoordinating Vote Choice Model

The weight of the spatial comparison associated with each form of government is

$$\beta_{j} = \frac{\exp\{-b_{jE0} - b_{jE1}EC_i\}}{1 + \sum_{j' \in [RR,RD,DR]} \exp\{-b_{j'E0} - b_{j'E1}EC_i\}},$$

$$j \in \{RR,RD,DR\}$$

$$\beta_{DD_i} = 1 - \beta_{RRi} - \beta_{RD_i} - \beta_{DR_i}$$

for coefficients b_{jE0} , b_{jE1} , $j \in \{RR, RD, DR\}$. See the text preceding equation 16 for the definition of EC₁. If all forms of government are treated symmetrically, $\beta_{RRi} = \beta_{RDi} = \beta_{DRi} = \beta_{DDi} = \frac{1}{4}$.

Measuring Ideal Points and Policy Positions

To measure θ_i , ϑ_{Di} , ϑ_{PDi} , ϑ_{RD} , ϑ_{RR} , and ϑ_{PRi} , I use a collection of sets of five seven-point (and one set of five four-point) placement scales. Each set includes one scale for self, one for each party, and one for each presidential candidate, all referring to the same substantive policy description (see Appendix B, 6). All scale items are oriented so that the "liberal" position during the given time period has the lower number.

The general motivation for the response codes is the observation that any continuous random variable X can be mapped onto a scale such that the values of the scale have probability mass spread uniformly over [0, 1]. Simply map

each value x of the variable to the value of the variable's distribution function, $F_{\mathcal{X}}(x) = \Pr(X < x)$. If X is multivariate with components X_1, \ldots, X_k , then the mapping from vectors (x_1, \ldots, x_k) to a uniform distribution on [0, 1] may be based on the distribution function $F_{\mathcal{X}}(x) = \Pr(X_1 < x_1, \ldots, X_k < x_k)$. I do not try to estimate the joint, five-dimensional distribution function for the five scales of each substantive description. Rather I crudely approximate the joint distribution by computing the simple average of the observed one-dimensional marginal distributions.

To assign a code in the [0, 1] interval to the response levels of each scale, I first compute the cumulative response proportions for the five scales in the set for each substantive description. Let $0 = r_{sj0} \le r_{sj1} \le \cdots \le r_{sj6} \le r_{sj7} = 1$ denote the successive cumulative proportions for substantive description s (e.g., s = "Liberal/Conservative") and scale $j \in \{S, D, R, PD, PR\}$, S = self, D = Democratic party, R = Republican party, PD = Democratic candidate, and PR = Republican candidate. For each $m \in \{0, \ldots, 7\}$, I compute $r_{sm} = (r_{sSm} + r_{sDm} + r_{sRm} + r_{sPDm} + r_{sPRm})/5$. For all five scales of type s, the numerical code used for original response category $m \in \{1, \ldots, 7\}$ is $\bar{r}_{sm} = (r_{s,m-1} + r_{sm})/2$. To determine values for θ_i , θ_{Di} , θ_{PDi} , θ_{Ri} , and θ_{PRi} for each voter, I average the score for the corresponding scale j over all (and only) the substantive descriptions for which the person responded to all five scales for that substantive description.

APPENDIX B. DATA NOTES

1. Economic Evaluations (EC₁). For 1976 the question wording is: "Would you say that at the present time business conditions are better or worse than they were a year ago?" For 1980, 1984, and 1988 the question wording is: "What about the economy? Would you say that over the past year the nation's economy has gotten better, stayed about the same, or gotten worse?" For 1992 the initial part of the question was changed to read: "How about the economy." For 1996 the initial part was: "Now thinking about the economy in the country as a whole." For all years except 1976, the responses are coded "much worse" (-1), "somewhat worse" (-5), "same" (0), "somewhat better" (5), and "better" (1). For 1976 only three levels of response were recorded, coded here as "worse now" (-5), "about the same" (0), and "better now" (5). The NES variable numbers for each year are 3139 (1976), 150 (1980), 228 (1984), 244 (1988), 3532 (1992), and 960386 (1996).

2. Party Identification (PID_{Di}, PID_{ID}, PID_{ID}, PID_{IR}, PID_I

3. Incumbency Status (DEM₁, REP₁). For 1976, incumbency status variables were built from note 17 in the codebook file nes1976.cbk. The variable numbers for the other years are 740 (1980), 59 (1984), 50 (1988), 3021 (1992; with errors corrected as indicated in the codebook file nes92int.cbk), and 960097 (1996).

4. Vote Choices (y_{ph}). The variable numbers are 3665 and 3673 (1976), 994 and 998 (1980), 788 and 793 (1984), 763 and 768 (1988), 5609 and 5623 (1992), and 961082 and 961089 (1996).

5. Sampling Weights $(1/\omega_i)$. In the NES data, $1/\omega_i$ is the number of adults in each household, which is multiplied by a time-series weight in years (1976, 1992, 1996) when part of

the sample belonged to a multiyear panel cohort. I rescaled the number of adults and time-series weight variables to give each a mean of 1.0 over the whole of each survey sample. The variable numbers are 3003 and 3021 (1976); 43 (1980); 70 (1984); 91 (1988); 29, 3076, and 7000 (1992); and 960046A and 960005 (1996).

6. Placement Scales $(\theta_i, \vartheta_{Di}, \vartheta_{PDi}, \vartheta_{Ri}, \vartheta_{PRi})$. Here are the brief substantive description and variable numbers for each set of scales for each year. The label "reversed" indicates an item whose categories were reordered to reverse the original ordering of 1 to 7 (or for Abortion in 1996, 1 to 4). 1976: Government Guaranteed Job and Living Standard, 3241-5; Rights of the Accused, 3248-52; School Busing to Achieve Integration, 3257–61; Government Aid to Minorities, 3264-8; Government Medical Insurance Plan, 3273-7; Liberal/Conservative Views, 3286–90; Government Guaranteed Job and Living Standard, 3758-62; Urban Unrest, 3767-71; Legalization of Marijuana, 3772-6; Change in Tax Rate, 3779–83; and Equal Rights for Women, 3787–91. 1980: Liberal/Conservative, 267-9, 278, 279; Defense Spending, 281-3, 286, 287; Government Services/Spending (reversed), 291-3, 296, 297; Reduce Inflation/Reduce Unemployment (reversed), 301-3, 306, 307; Liberal/Conservative Views, 1037-9, 1053, 1054; Government Aid to Minorities, 1062-4, 1073, 1074; Getting Along with Russia, 1078-80, 1089, 1090; Equal Rights for Women, 1094-6, 1105, 1106; and Government Guaranteed Job and Living Standard, 1110-2, 1121, 1122. 1984: Liberal/Conservative Placement, 119-21, 123-5, 127-9, 131-3, 135-7; Liberal/Conservative, 369, 371-4; Government Services/Spending (reversed), 375-9; Minority Aid/No Aid, 382-6; Involvement in Central America (reversed), 388-92; Defense Spending, 395-9; Social/Economic Status of Women, 401–5; Cooperation with Russia, 408–12; and Guaranteed Standard of Living/Job, 414-8. 1988: Liberal/Conservative, 228, 231, 232, 234, 235; Government Services/Spending (reversed), 302-4, 307, 308; Defense Spending, 310-2, 315, 316; Government-Funded Insurance, 318-22; Guaranteed Standard of Living/Job, 323-5, 328, 329; Social/Economic Status of Blacks, 332-4, 337, 338; Social/ Economic Status of Minorities, 340-2, 345, 346; Cooperation with Russia, 368-70, 373, 374; and Women's Rights, 387-91. 1992: Ideological Placement, 3509, 3514, 3515, 3517, 3518; Government Services/Spending (reversed), 3701–5; Defense Spending, 3707-11; and Job Assurance, 3718-22. 1996 (omitting the variable number prefix 960): Liberal/Conservative, 365, 369, 371, 379, 380; Government Services/Spending (reversed), 450, 453, 455, 461, 462; Defense Spending, 463, 466, 469, 477, 478; Abortion (reversed), 503, 506, 509, 517, 518; Jobs/Environment, 523, 526, 529, 535, 536; and Environmental Regulation, 537, 538, 539, 541, 542,

7. Missing Data. The loss of cases from ignoring third-party votes substantially affects only presidential votes. Among the 7,198 cases in which any vote cast was for a Democrat or a Republican, 1,426 cast a vote for only one office (either president or House member). Among the 5,772 who said they voted for Democratic or Republican candidates for both offices, 527 were excluded because the incumbent in their district was unopposed. Of the remaining cases, 379 were missing data for the ideal point, policy positions or economic evaluation (295 missing only one or more position variables, 58 missing only EC, 26 missing both); seven more cases were missing data for party identification.

8. Reagan's Chance of Winning. The Continuous Monitoring Survey is Miller and the National Election Studies (1985). The item wording (var. 1001) is: "As you know, Ronald Reagan has been nominated by the Republican party. What chance do you think he has of winning the 1984 presidential election? We will use a scale that goes from 0 to 100 where 0

represents NO chance of winning the presidency, 50 represents an even chance, and 100 represents certain victory." In computing the mean I weighted observations in proportion to the number of adults (var. 16) and in inverse proportion to the number of nonbusiness telephones (vars. 1129, 1131, 1133) in the household. The text reports the mean divided by 100.

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Senate Representation and Coalition Building in Distributive Politics

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The Senate's equal representation of states shapes coalition building in distributive politics. The great variation in state population means that some states have far greater need for federal funds than others, but all senators have equal voting weight. As a result, even though all senators' votes are of equal value to the coalition builder, they are not of equal "price." Coalition builders can include benefits for small states at considerably less expense to program budgets than comparable benefits for more populous states. Building on formal models of coalition building, two hypotheses are developed and tested. First, coalition builders will seek out less costly members to build supportive coalitions efficiently. Second, the final outcomes of distributive policy will more closely reflect the preferences of small-state senators than large-state senators. The hypotheses are tested by examining the 1991 and 1997–98 reauthorizations of federal surface transportation programs. The findings support both hypotheses.

Mr. President, I am not here asking that Arizona receive something extra, unlike a lot of people who are still negotiating in the cloakroom here. I am not asking for money for special projects. But I am asking why it is that the donor States—the States that send more than they receive—can't eventually hope to get some equity in this program. . . This reminds me a little bit of the "Animal Farm" story of George Orwell of 1946. It turns out that all the animals in the barnyard were equal, except that some were more equal than others. That is the way it is with the States of the Union here.

—Senator Jon Kyl (R-AZ), Congressional Record, May 22, 1998, \$5399

The frustration expressed by Senator Jon Kyl, suggesting that some states are "more equal than others," is in one important sense correct. Although all states are represented equally in the Senate, they differ widely in population. At the extremes, Wyoming has a population smaller than most congressional districts, whereas California is more populous than many nations. Such differences mean that states also vary greatly in their need for federal funds. The cost of governmental programs, after all, largely depends on the number of people served by them, notwithstanding some economies of scale. Despite differences in need for federal funds, however, each state has equal representational power in the Senate. This article investigates how apportionment shapes the politics of distributive policymaking in the Senate. This issue has been overlooked in the congressional literature, even though scholars have devoted a great deal of study to the ways institutional structures—such as the distribution of power through the committee and party leadership systems shape distributive policy outcomes (Arnold 1979, 1990; Carsey and Rundquist 1999a, 1999b; Ferejohn 1974; Fiorina 1989; Hall and Grofman 1990; Levitt and Snyder 1995; Rundquist and Griffith 1976; Stein and Bickers 1995; Strom 1975;

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Wilson 1986a, 1986b). While focusing on the effects of internal congressional organization, however, scholars have ignored Senate apportionment, another basic feature of the institution.

Apportionment shapes Senate distributive policymaking for two reasons. First, senators representing small states have more to gain from procuring a given amount of federal dollars than do senators who represent large states (Lee and Oppenheimer 1999, 123–8). A federal grant of \$5 million, for example, has a far greater effect in Wyoming than in California. Such a grant yields greater electoral benefits for senators who represent small states, both in terms of their statewide visibility and the percentage of residents benefited. Senators representing large states, by contrast, have to procure substantially larger sums in order to obtain the same electoral benefits. All else being equal, procuring these larger amounts will be more difficult, especially in times of fiscal restraint. As a result, small-state senators are better positioned to benefit from distributive politics: Put simply, they need fewer federal dollars for their state but enjoy equal representational power. Second, Senate apportionment affects the incentives of coalition builders in distributive policymaking. The tremendous differences in state population create a unique coalition-building dynamic: All senators' votes are of equal value to the coalition builder, but they are not equal in price (Lee and Oppenheimer 1999, 140). Senators who attempt to build winning coalitions within budgetary constraints can accommodate the funding demands of small-state senators at a lower cost than those of large-state senators. As a result, coalition leaders have strong incentives to include senators representing small states in their coalitions.

Research has shown that the representational advantages of small states translate into public policy. Small states receive more per capita across many federal policy areas and in distributive programs generally (Atlas et al. 1995; Lee 1998; Lee and Oppenheimer 1999). As in most distributive programs, this advantage is clearly evident in federal transportation policy. Less populous states receive more federal transportation dollars per capita than more populous states, even after controlling for differences in need for these

funds.¹ Analyses of aggregate budgetary outlays, however, do not examine how apportionment shapes internal Senate politics by influencing the way senators construct distributive coalitions and the funding policies that eventually prevail.

To investigate the effects of Senate apportionment on coalition building in distributive policymaking, I examine the major surface transportation reauthorizations of the 1990s. In 1991 and 1998 Congress overhauled all transportation programs, devising formulas to distribute \$151 billion and \$216 billion, respectively, among the states. The primary issue at stake during these reauthorizations was distributional equity among the states (Dilger 1998). How did Senate coalition leaders build support for their proposals to distribute federal transportation funds? Which states emerged as the winners and losers? To answer these questions, I compare the compositions of the alternative coalitions developed in the Senate and assess which coalitionbuilding efforts were most successful. The data strongly support the hypotheses advanced. I find that coalition leaders in the Senate build coalitions more efficiently by drawing on the support of small-state senators. These efficient coalitions are more successful. In 1991, the more efficient coalition won outright. In 1998, small states exercised a greater influence on the outcome of the legislation, with the final allocations more closely reflecting small-state senators' preferences than those of their large-state colleagues.

BUILDING COALITIONS IN THE SENATE

With very few exceptions (Evans 1994), most of the scholarly work on legislative coalitions has been in formal theory. In game theoretic terms, the Senate politics of writing formulas to distribute federal funds is best conceptualized as a "divide-the-dollars" game, more specifically, as a constant-sum, 2 100-person game with transferable utility. In developing a formula, legislators must choose among alternative proposals for allocating funds, a situation in which one state's gain is another state's loss.

Riker (1962) advanced the famous proposition that

The recent donor-donee state debate over transportation funds is just a way to frame the controversy over transportation spending per capita. The amount a state pays in gas and other times to the Highway Trust Fund is a function of state population. Multivariate regression analysis confirms that the ratio of federal highway dollars a state received to the dollars it paid in taxes to the Highway Trust Fund over the entire post-1956 period is strongly related to state population. Regressing this ratio on state population—while controlling for the number of interstate and primary highway miles in the state—reveals a large negative, statistically significant (p < .001) relationship Less populous states have had a higher ratio of highway receipts to highway taxes than have more populous states (Lee 1997, 195–200).

rational, utility-maximizing players in divide-the-dollars games will form only minimum-winning coalitions. If a given amount is to be divided among the players, then the larger the winning coalition, the smaller is each winner's share. Thus, a minimum-winning subset of any larger-than-minimum-winning coalition will always have incentives to eject superfluous members to maximize the payoff to themselves. Riker's model has proven to be quite resilient, "appearing as either a key prediction or as an essential assumption of almost all formal models of coalition formation" (Groseclose and Snyder 1996, 303).4 Since Riker, however, most scholars studying legislative divide-the-dollars games have focused on the stability of game outcomes—principally on the problem of overcoming majority rule cycles5rather than on the composition of winning coalitions and the distribution of payoffs among their members.6 An exception to this pattern lies in the sequential choice models of Baron and Ferejohn (Baron 1989, 1991, 1995; Baron and Ferejohn 1989a, 1989b). Sequential choice models of divide-the-dollars games modify Riker's conclusions by demonstrating that different types of procedural rules lead to different distributions of funds: Benefits are distributed to minimal majorities under closed rules but to larger-than-minimal majorities under open rules. Given the difficulty of cutting off debate in the contemporary Senate (Binder and Smith 1997), these findings comparing open and closed rules strongly suggest that successful coalitions in the Senate will need to include large majorities indeed.

A key limitation of these formal models is that they do not address the ways in which Senate apportionment shapes coalition building. Formal approaches to coalition building have generally modeled divide-the-dollars games as symmetric, meaning that "the value of a coalition is a function only of the number of players in it" (Hardin 1976, 1203; Shepsle 1974, 506). In symmetric games, the payoff that a coalition can provide to its members is "dependent solely on the size of

² In constant-sum games, the total value of the game always sums to a constant. They are "strategically identical to zero-sum games" (Morrow 1994, 102).

³ Many political games cannot be modeled using the transferable utility assumption, which is only appropriate when "the game concerns the allocation of some set of *m* resources or goods and that coalition members can trade these goods among themselves" (Ordeshook 1986, 317, see also 318–21; Koehler 1984).

⁴ Groseclose and Snyder's model shows that minimum winning coalitions are not always cheapest when vote buyers move sequentially. Their model, however, only applies to attuations in which a legislator who favors the existing policy status quo uses bribes or favors to sway the minimum necessary number of members away from a coalition builder who is seeking to change existing policy. As a result, their model does not apply to a situation such as that faced by senators during funding formula disputes, in which those who favor policy changes and those who favor the status quo must all write reauthorizing legislation in such a way as to "buy" votes (Groseclose and Snyder 1996, 312).

The problem of cycling occurs in divide-the-dollars games because no stable majority prefers any particular way of distributing the funds to every other possible distribution. Plott (1967) and McKelvey (1976) demonstrated that there are no equilibria in multidimensional choice settings (also see Arrow 1951; Buchanan and Tullock 1967; Riker 1982). Drvide-the-dollars games have as many dimensions as players; therefore, legislative voting on this type of policy issue will not, in theory, produce stable allocational outcomes.

⁶ For a review of the literature on majority rule cycles and equilibrium outcomes, see Shepsle and Weingast 1994. Generally speaking, social choice theorists have explored how legislative norms and institutions—including the committee system, party organizations, restrictive rules in voting, and norms of reciprocity—can provide stability and counteract majority-rule cycles.

the coalition, not on the specific persons that make up the coalition" (Ordeshook 1986, 322). As discussed above, however, Senate apportionment means that senators from large and small states demand dramatically different amounts of federal funds. Senate coalition builders will thus find that small-state senators' demands can be accommodated at much lower cost to the coalition. The payoff a coalition can provide its members thus depends on the specific players included, not merely on the number.

Although the existing formal models of coalition building do not readily apply to the Senate, a rational choice framework is very useful for analyzing the writing of formulas to distribute federal funds. It is important to note that statutory formulas determine how most federal domestic assistance funds are distributed (Advisory Commission on Intergovernmental Relations 1995, 14). In writing formulas, senators determine the share of the total funds each state will receive, often without knowing how much money will be appropriated for the program. Senators, like members of Congress generally, perceive that providing particularized benefits to their state advances their reelection interests. Senators take a great deal of interest in the size of their state's share and work hard to make it as large as possible. One perennial coalition leader on transportation formulas, Senator John Warner (R-VA), for example, said simply: "All of us know that fighting for our individual states is that responsibility which is foremost" (Cong. Rec. 1998, S1827).

A successful coalition builder must make the best offer to a large enough group of senators to ensure passage on the Senate floor. From the point of view of rational, utility-maximizing senators, the best offer is the one that allocates a share of the funds to their constituents greater than any other proposal. Former Senator Robert Dole (R-KS) explained how he developed a funding formula by working from these assumptions about senators' behavior:

Under the formula of the chairman of the Appropriations Committee, 33 States received a part of the \$8.2 billion total; in other words, he had 66 votes. My original formula gave more money to only 18 states, so I only had 36 votes. I was 30 short. However, with the able assistance of an overworked master of numbers... we found a way to change this formula, and by this morning I had 54 votes and the distinguished chairman of the Appropriations Committee had 46 votes (Cong. Rec. 1991, S8104).

Dole's point is clear: To produce a winning formula a senator must distribute the funds so that at least a majority of senators will prefer that formula to the existing alternatives. This, Dole observes, is done by giving "the right information to the people downtown," and then "they will give us the right numbers" (Cong. Rec. 1991, S8104). In short, senators strategically choose formula factors (and weights to give them) so that at least a majority of states will receive a larger share of the funds than under the other formulas proposed.

In the Senate, moreover, coalition builders cannot use restrictive rules to protect their proposals from alternatives. As in the example above, the chair of the

Appropriations Committee, Senator Robert Byrd (D-WV) could not protect his proposal from alternatives. Coalition leaders in the Senate, including those who serve on the committees of jurisdiction, usually cannot prevent dissatisfied senators from offering alternative proposals or using their individual prerogatives to obstruct. The successful coalition builder, then, must make the best offer to a very large group of senators. If there is strong opposition to the coalition leader's proposal, even from a small minority of senators, then the coalition will need at least 60 senators who will vote for cloture.

No coalition leader, however, could ever make the best possible offer to a winning supermajority of senators. In theory, given the lack of limits on debate and amendments, senators could endlessly devise new formulas that would distribute more funds to their own state and offer at least 60 senators more than they would receive under any previous proposal; in other words, majority-rule cycling is always a possibility.7 In practice, however, not every senator engages in coalition building on every issue. Senators have limited time, staff, and information. As a result, Hall (1996, 2) concludes: "Participation in Congress is seldom universal," and "it is never equal." Given the obstacles to participation, a limited number of alternative formulas will be offered during any given formula dispute, principally by members of the relevant committee(s)-in the case of transportation formulas, by members of the Senate Committee on Environment and Public Works. Although it is beyond the scope of this project to ascertain the factors that cause senators to become coalition leaders in the first place (see Hall 1996), such limits on participation reduce the incidence of cycling in divide-the-dollars games.

Even if they do not propose alternatives during funding formula disputes, senators will still prefer the formula that offers their state the largest share. Coalition builders almost always provide clear charts designating the share each state will receive, so determining which formula will provide their state the most funds requires virtually no commitment of senators' resources. In their comments on the floor, senators are quite frank about the importance of these charts. Senator Dale Bumpers (D-AR), for example, explained his intention to vote for a formula even though he felt that it was not the best alternative in public policy terms because "it looks as though Arkansas could get around \$50 million a year more. You do not have to be a rocket scientist to know I am going to vote for that" (Cong. Rec. 1991, S7746).

⁷ It is unlikely that Baron and Ferejohn's equilibria would emerge in a legislature with rules like the Senate's because two days must lapse after a cloture petition is filed before a cloture vote can be taken. The Baron and Ferejohn (1989a, 1184) open-rule model, by contrast, specifies that when a member moves the previous question, "the motion is equivalent to bringing the motion on the floor to an immediate vote," thus denying other coalition leaders the chance to build an opposing majority to defeat the measure. Opposing coalition leaders under Senate rules can use the time between filing and voting on a cloture petition to build a coalition that defeats the cloture motion by allocating more funds to members of the cloture-supporting coalition.

In the same vein, coalition builders will propose formulas that provide generous allocations to their own state. They want to form a successful coalition around a proposal that allocates the largest feasible percentage of funds to their own state. Given this goal, rational, utility-maximizing senators will seek to build winning coalitions at the lowest cost, leaving the maximum amount of funds for their own state. Working from this assumption, Riker (1962) argues that political coalitions will be as small as is necessary to win: The smaller the coalition, the more benefits the winners can share among themselves. All else being equal, larger coalitions in distributive policy are indeed more expensive. But in the Senate, all else is never equal. One consequence of Senate apportionment is that it is simply more expensive to include some states in a distributive coalition than others.

In transportation policy, as in most federal programs, a state's need for funds correlates closely with state population (U.S. General Accounting Office 1995). Senators who seek to meet these greater needs must procure greater amounts of funds in order to obtain the same electoral benefits for themselves that senators representing smaller states receive from procuring smaller amounts of funds. For example, if a senator from Wyoming were able to procure an additional \$20 million in transportation funds in a formula fight, this would be an increase equivalent to nearly \$40 additional per constituent, quite a windfall. In California, the \$20 million increase would only mean an additional 62 cents per constituent. Such considerations make it less expensive for coalition builders to accommodate small-state senators' demands. As a result, smaller coalitions in the Senate are not necessarily less expensive for the coalition builder to construct, and larger coalitions are not necessarily more expensive. The cost of building a coalition depends on the specific states included, not merely on their number.

Rational coalition leaders who attempt to build coalitions at the lowest cost will thus seek out small-state senators' support. If multiple coalition leaders pursue this strategy, then there will be a "bidding war" for small-state senators. As long as these senators' states can be added to the coalition at a lower cost than others, these senators will continue to be more attractive to coalition builders. Rational coalition builders, however, will not compete to obtain the support of the senators who represent states that need more funds, such as California, New York, and Texas.8 Accommodating the demands of these senators would consume too much of the coalition leader's budgetary resources.

The theory of Senate coalition building advanced here deviates from the rational choice approach in one important respect. Formal theorists always assume that those who are not part of the winning coalition will receive nothing (see, e.g., Baron and Ferejohn 1989a;

Riker 1962). The reason is that players are assumed to maximize one goal only, self-interest. Within that framework, legislators are expected to procure as much for their state as possible, without concern for other goals. Members of Congress; however, have multiple goals, and making good public policy is one of them (Fenno 1973; Hall 1996, 68–71; Sinclair 1989, 144–52). Concern for good public policy constrains Senate coalition builders from exploiting losing coalitions to the fullest extent imaginable. A transportation program that allocated no funds to twenty states, for example, would hardly be good public policy. Although the pursuit of reelection will lead senators to prefer formulas that distribute more to their state than less, their interest in good policy will check them from excluding the losers from any share of the funds.

In sum, the theory of coalition building advanced here is fundamentally one of rational choice. Senators are expected to support the proposals that offer their state the greatest share of funds. Coalition leaders will build support for their formula by making the best offer to a winning coalition of senators. In seeking to benefit their own state, coalition leaders will attempt to build winning coalitions "efficiently." Because of differences in state need for funds, the support of some senators can be gained at a lower cost to the program budget than that of others. Rational coalition builders will accordingly seek the votes of these senators. Thus, the following hypotheses will be tested:

Hypothesis 1. Successful coalition builders will seek out less costly members to increase the size of their coalition efficiently.

HYPOTHESIS 2. The final outcome of formula fights will more closely reflect the preferences of small-state senators than large-state senators.

WHY TRANSPORTATION POLICY?

Surface transportation programs provide a good opportunity to study the dynamics of distributive policymaking. One reason is that Congress has jealously guarded its control over the distribution of transportation funds, which is done almost entirely by statutory formulas. Another is that demand for transportation funds is great across all areas of the country, unlike other programs, such as water projects and community development. Every state has roads, highways, and bridges, and thus all senators are concerned with the distribution of transportation dollars. Any differences in senators' success in procuring funds in this area are more likely to reflect differences in Senate influence than in state demand for funds (see Rich 1989).

A third reason is that the politics of transportation is intensely distributive, relatively uncomplicated by controversies over ideology or nondistributive issues. As Senator Trent Lott (R-MS) remarked during the 1991 fight over the transportation funding formula: "It's not a partisan issue. It is even hard to get a fix on how regional an issue it is. It really boils down to how well you do or how poorly you do under the formula" (Cong. Rec. 1991, S7988). Finally, transportation poli-

Because a state's need for federal funds is a function of its population, large-state senators are defined as those from states that are underrepresented in the Senate according to the standard of one person, one vote. Small-state senators are those from states that are overrepresented in the Senate according to this standard.

cymaking provides a unique view into the dynamics of Senate coalition building because coalition leaders always indicate which states they seek to include. The funding formulas for transportation are very complex, with many "adjustments" that greatly affect the amounts states receive.9 This complexity enables senators to construct their coalitions strategically, targeting funds to particular states.10 Both because of the complexity and because senators are so concerned with geographic distribution when considering transportation programs, coalition leaders provide charts that explicitly indicate how much each state will receive under their proposed formula. Using these charts, I am able to reconstruct the composition of transportation formula coalitions better than in other areas for which information about winners and losers under various proposals is not so readily available.

OPERATIONAL DEFINITIONS

A coalition builder is a senator who offers a proposal. This concept is relatively easy to operationalize because bill sponsorship is a matter of public record. The composition of legislative coalitions is considerably more difficult to discern because most proposals introduced in Congress are never brought to a vote, and thus most senators never go on the record as supporters or opponents. Assuming that senators support the formula that is most generous to their state, however, a coalition can be defined as those senators whose state would receive more under one proposal than under any alternative offered.

To determine the composition of coalitions, I simply compare competing proposals. Sometimes coalition builders in formula fights will assume different amounts of money to be distributed with the formula. For comparison, I simply refer to the percentages allocated to each state under the proposals. It is clear from senators' comments that this approach is substantively sound. Percentage shares matter more to senators during formula fights than specific dollar amounts, as the latter are contingent on the final appropriation. During the 1998 transportation formula dispute, for example, senators Arlen Specter (R-PA) and Rick Santorum (R-PA) voted against the bill even though it would have greatly increased the amount of funds for Pennsylvania over the previous level. As Specter explained, "the pending bill would reduce Pennsylvania's share of the total highway formula from the 4.32 percent share under the original ISTEA law (FY 92-97) to 3.79" (Cong. Rec. 1998, S1849).¹¹ To address this concern, senators who propose a formula typically include on their charts each state's percentage share in addition to the dollar amount each would receive given a specific total to be distributed.¹²

To determine the "efficiency" of coalitions, I compare the average amount of funds that coalition builders allocate to the states in their coalition: The more efficient is the coalition, the lower the cost of states in the coalition. Every formula, of course, is designed to allocate all the funds authorized for the program. During formula fights, however, each alternative redistributes only some portion of the funds by comparison to the other formulas proposed or in existence. From the perspective of coalition builders, the most efficient use of these funds is to build the least expensive winning coalition, leaving the most funds to concentrate on their own state.

THE 1991 TRANSPORTATION REAUTHORIZATION

The 1991 Intermodal Surface Transportation and Efficiency Act (ISTEA) marked the last time the interstate highway construction program was reauthorized. Senators recognized that this legislation would define priorities and set precedents for federal highway policy in the postconstruction period. Senator Daniel Patrick Moynihan (D-NY), chair of the subcommittee with jurisdiction over highways, had a vision for the future of surface transportation policy. A critic of previous federal policy, Moynihan sought to emphasize urban planning, environmental protection, and mass transit. Under his leadership, the committee endorsed a proposal to consolidate federal highway categorical programs into a massive block grant, the Surface Transportation Program, which would enable states to spend highway funds on transportation programs of their own choosing, including mass transit.

Despite the sweeping policy changes at issue, the principal subject of contention during the 1991 reauthorization was the geographic distribution of funds. Both the Senate and the Environment and Public Works Committee approved without controversy the major program consolidations and policy changes that Moynihan favored (Mills 1991b, 1575; 1991c, 1367). Deciding how to distribute federal funds, however, occupied both the committee and the Senate throughout the lengthy consideration of this legislation. Floor debate on the bill lasted for ten days, sometimes going

⁹ For a description of federal transportation formulas, see U.S. General Accounting Office 1995. For the most recent transportation formulas adopted, see the conference report accompanying the Transportation Equity Act for the 21st Century (H. Rept. 105–550).

¹⁰ It may be more difficult to construct coalitions artfully in some programs than in others. As Arnold (1979, 217) writes: "In theory it is possible for Congress to write a formula that seemingly allocates benefits according to objective criteria (e.g., per capita income) but that actually allocates them according to political criteria. In practice it is very difficult. Congress would first have to select objective criteria that were good surrogates for all the political criteria, and then would have to devise a complicated weighting scheme." In transportation policy Congress does this. The GAO report (1995, 1) found that "underlying data used in the formulas are to a large extent irrelevant," because allocations are largely determined by "adjustments."

¹¹ Similarly, Senator Carl Levin (D-MI) complained about an amendment that would increase the dollars received by Michigan but reduce its share: "My state gets a smaller percentage under the committee amendment than it does under the underlying bill. You can add all the money you want, which is what the committee did, but the problem is still going to remain in terms of the percentage of the contribution unless something else happens here" (Cong. Rec. 1997, S1298).

¹² See, for example, Cong. Rec. 1991 (S7271), 1997 (S10771, S10767), and 1998 (S1315).

late into the night (Mills 1991d, 1653). How did coalition leaders build support for their proposal? First, a comparison reveals that they constructed coalitions more efficiently by targeting funds to less populous states. Second, because small states' votes could be obtained at a lower cost, the outcome of the formula fight more closely reflected the preferences of small-state senators than large-state senators.

Composition of the 1991 Coalitions

Two coalition builders attempted to determine the distribution of federal transportation funds in 1991, Moynihan and John Warner (R-VA). Moynihan used two strategies. First, he opposed creation of the National Highway System (NHS), a key initiative of the Bush administration to prioritize maintenance of the nation's most heavily used roads and highways. This was a concession to less populous states. Senators from small states strongly opposed the NHS initiative because these states contained a very small proportion of this mileage. Second, Moynihan rejected efforts to change the funding formulas for highway programs, generally maintaining the apportionments states already received under the categorical grants for highways.¹³ Explaining his formula, Moynihan conceded: "We have no rationale for the allocation formula we adopt in this bill, save that it is the existing one. It is the one that is in place" (Cong. Rec. 1991, S7746). This, too, was a concession to less populous states because they had long received back more in federal transportation dollars than they had paid in taxes to the Highway Trust Fund. In keeping these allocations, Moynihan maintained the existing highway coalition; he gained the support of all the members of the committee with the exception of four senators from states larger than average-Bob Graham (D-FL), Warner, Dave Durenberger (R-MN), and Howard M. Metzenbaum (D-OH)—who felt that their state was being shortchanged (Mills 1991c, 1368).

Warner expressed his intention to fight the formula "until we receive that degree of fairness and equity to which we think we are entitled in our respective states" (Cong. Rec. 1991, S7270). When the Senate committee reported the bill on June 6, Warner objected to the motion to proceed and announced his intention to offer an amendment that based the funding formula on the FAST proposal introduced in the House of Representatives (HR 2950). The FAST proposal adopted the administration's NHS policy and proposed a formula to direct funds to states with high levels of highway use. 15

As a consequence, the formula Warner favored would have increased the allocations to populous states and decreased those to less populous states. If Invoking imagery of D-Day on its 47th anniversary, Warner addressed Moynihan: "Our forces have landed... and we have established a beachhead"; he urged him to "come to the peace table as quickly as possible" (Cong. Rec. 1991, S7274).

Figure 1 shows the difference between the Moynihan and Warner proposals in terms of the amount each state would receive. 17 This difference was calculated by subtracting the amount allocated to each state under Moynihan's proposal from that allocated under Warner's proposal. When the point on the graph is above the zero line, the state would be better off under Warner's formula; when it is below the line, the state would be better off under Moynihan's proposal. This difference is plotted for each state in millions of dollars, with the states ranged on the x-axis from least to most populous.

The difference in the composition of the two coalitions is striking. Warner's bill would grant additional funds to nearly all the middle-sized and large states, compared to Moynihan's bill, while cutting funds to every state with fewer than five congressional districts. Of the 28 states with six or more congressional districts, 21 would have benefited from the Warner formula. Moynihan's bill combined benefits to a few populous states—Massachusetts, New Jersey, Pennsylvania, and New York—and to a couple of middle-sized states, Connecticut and Colorado, with benefits to all the least populous states.

With respect to Senate politics, however, the crucial difference between the two formulas illustrated in this figure is that there are more points below the zero line than above it. In other words, more states were better off under Moynihan's formula than under Warner's. Only 22 states would receive more dollars under Warner's formula, and 27 would receive less.¹⁸

Efficiency of Coalitions

Warner's formula would redistribute \$4.9 billion of the total amount to be allocated under the program by comparison to Moynihan's formula. In redistributing these funds, however, the Warner formula was more generous to only 22 states, which was not sufficient to defeat the Moynihan formula. By comparison, Moynihan's bill distributed this \$4.9 billion across more states, ensuring that 27 would receive more than under

¹³ Under the new block grant programs, every state would receive annually an average of the amount it had received over the previous five years for all transportation programs combined.

¹⁴ The Federal Aid Surface Transportation Act (FAST) was introduced in the House of Representatives by Representative Charles E. Bennett of Florida on June 7, 1991 (Cong Rec., E2089). The formula in this bill was uncontroversial in the House.

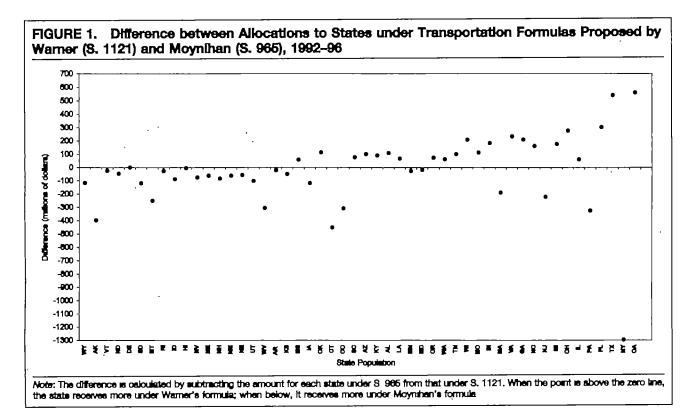
¹⁵ The formula allocated funds as follows: one-third on the basis of a state's proportion of the nation's total diesel fuel use, one-third on the basis of a state's proportion of the national vehicle miles of travel, and one-third on the state's proportion of national highway system lane mileage. The House report emphasized that the new formula's

emphasis on use "reflects the needs of the National Highway System" (U.S. House 1991, 22).

¹⁶ It should be noted, however, that under the formula passed in the House, more populous states would not receive higher per capita allocations than less populous states. The difference between the two formulas is simply in the greater per-capita advantage provided to small states in the Senate bill than in the House bill.

¹⁷ Data are from the Federal Highway Administration and were printed in the Congressional Quarterly Weekly Report (Mills 1991a, 1488).

¹⁸ Delaware receives virtually the same amount under both formulas. Under Warner's bill the state would receive only \$456,000 more than under Moynihan's bill. The difference between the two was not nearly so small for any other state.



the alternative. He was able to do this because the members of his coalition were largely small-state senators and thus on average less expensive to include than the members of Warner's coalition. As a result, the Moynihan coalition was more efficient, enabling the senator to garner a majority while reserving a substantial sum for New York. The mean state in Moynihan's coalition had a population of 2.5 million, considerably smaller than the average state (5 million). By including all the least populous states in his coalition, Moynihan added 23 of the 27 states in his group as beneficiaries at a cost of less than 2% of the program funds each. The mean state in Warner's coalition, by contrast, had a population of 7.6 million, considerably larger than the average state, and cost him 3.1% of the funds.¹⁹

This coalition-building efficiency enabled Moynihan to grant 26% of the \$4.9 billion redistributed funds to his own state. Under Warner's formula, by contrast, no state received a windfall of that magnitude. Virginia only received 6% of the redistributed dollars, an additional \$233 million, under Warner's formula. In short, working with the same overall amount of dollars as Moynihan, the Warner coalition benefiting middlesized and populous states at the expense of the least populous states was a political failure.

Outcome

Warner's formula, which later passed easily in the House, was not politically viable in the Senate. Although he supported Warner's bill, Senator Lloyd

Bentsen (D-TX) recognized during floor debate that it did not have the votes to win: "There is a basic inequity there [in Moynihan's bill] that has to be corrected. I do not know that we have the votes. I understand how one could artfully put together a coalition where there are more winners than losers... But at some point the question of equity has to come in" (Cong. Rec. 1991, 7540). Bentsen recognized that to win on highway formulas, one must have more "winners" than "losers." In the Senate, of course, states and not numbers of constituents are the "players" in the game. This means that, at minimum, a coalition builder must ensure that a majority of states will not lose funds. Warner's formula did not meet even that low threshold.

It is clear from Warner's comments on the Senate floor that he had hoped to construct a successful alternative to the committee bill. When he first objected to consideration of the Moynihan bill, Warner explained that he currently represented a group of nine states and anticipated "it will grow to twice that number and perhaps three times that number" (Cong. Rec. 1991, S7270). He sought to gain time by refusing to "relinquish one inch of ground, procedurally or otherwise" (Cong. Rec. 1991, S7270). During the following ten days, however, Warner failed to gain the support he needed because his formula increased funding for populous states, which precluded coalition-building efficiency and made it impossible to create even a minimal majority coalition.

The Senate vote on the FAST formula took place on June 18, the day before the Senate passed the committee bill. Senators knew that the Warner alternative would not be adopted (see *Cong. Rec.* 1991, S7983, S7993). Although the outcome was not in doubt, the

¹⁹ A t-test of difference of means reveals that difference in the composition of Moynihan's and Warner's formulas is statistically significant (t = 3.7, p < .001)

votes show a strong correlation ($\phi^2 = .85$, p < .001) with states' interests. The FAST formula failed 41 to 57: 98% of the senators whose state would be better off under the Moynihan formula voted against FAST; 93% of those whose state would be better off under FAST voted for it.²⁰

Warner's failure and Moynihan's success support the hypothesis that coalition builders have incentives to benefit less populous states when writing formulas. At a very low cost to the program budget, Moynihan could build a majority coalition and still provide a windfall to his own state, along with generous allocations to a few others. By comparison, Warner made a highly inefficient attempt to build a Senate coalition. Adding the larger states to his coalition simply did not leave enough money to draw in a majority of states. Because of this dynamic, the outcome of the 1991 formula fight more closely reflected the preferences of small-state senators than those of their large-state colleagues. Nearly every small state was a member of Moynihan's winning coalition, and most of the large states were members of Warner's losing coalition. It is important to note that Moynihan's formula prevailed in conference with the House, which left most large-state senators dissatisfied and ready to revisit the funding formula issue at the next reauthorization of the program.21

THE 1998 TRANSPORTATION REAUTHORIZATION

Just as in 1991, the key issue in the 1998 congressional debate over the transportation reauthorization—the Building Efficient Surface Transportation and Equity Act (BESTEA)—was the distribution of funds to states.²² The politics was more complex in 1998, however. Three members of the Senate Environment and Public Works Committee—Warner, chair of the Transportation and Infrastructure Subcommittee; Max Baucus (D-MT), ranking minority member of both the full committee and the subcommittee; and John Chafee (R-RI), chair of the full committee—each introduced

²⁰ The anomalous senators are as follows. Senator Dale Bumpers voted for FAST even though Arkansas would receive more under the committee bill because, he said, the Moynihan formula was "an anachronism" (Cong Rec. 1991, S7983). Even so, he noted that the amendment would not succeed. Washington's senators voted against FAST, as did Senator Alan Cranston (D-CA), even though their states would have received more funds under it. Even if all senators had voted in accord with their state's interests, FAST still would have failed.

²¹ It is not unusual for the House to concede to the Senate on funding formulas in conference committee. Because the recipients of funding formulas are almost always states, constituencies of senators are more directly affected by them than those of House members. As a consequence, senators take a greater interest in funding formulas than do House members (see Lee 1997, 100–5; Lee and Oppenheimer 1999, 192–4). A study of interchamber conflict in 42 formula grant programs reveals that the Senate's formula preferences prevail more than twice as often as those of the House (Lee 1997, 135; Lee and Oppenheimer 1999, 216).

²² The 1991 reauthorization expired at the end of fiscal 1996. As often happens, Congress was unable to reauthorize the legislation before its expiration and had to pass a short-term extension to keep highway funds flowing to the states.

distinct funding formulas and garnered substantial support among other members of the committee and in the chamber as a whole (Hosansky 1997, 1068).

Warner once again backed a formula in the Senate (S. 335) that would have increased allocations to most of the populous states.²³ By comparison, the Baucus formula (S. 532) would have allocated funds more equally across states, granting dramatic share increases to the less populous states in the Rocky Mountains and the Plains.²⁴ With relatively minor changes, Chafee's bill—known simply as the ISTEA Reauthorization Act (S. 586)—would have continued the allocation patterns in the 1991 Moynihan formula.

Compared to the 1991 act, all three formulas proposed share increases for at least 33 states; under every formula at least 33 states would receive a larger percentage of the total allocation than they had received under ISTEA.25 Chafee's formula made the least extensive adjustment to existing legislation-redistributing a relatively small proportion of states' funding shares under ISTEA-whereas Baucus and Warner proposed more far-reaching changes.²⁶ All three increased the shares for a supermajority of states relative to existing law by targeting a handful of populous states for the largest cuts: Warner and Baucus targeted Massachusetts (three percentage share reduction), New York (two percentage share), and Pennsylvania (one percentage share); Chafee focused on Massachusetts (two percentage share), Georgia, Pennsylvania, and Texas (0.5 percentage share each).27 These reductions may appear small, but \$216 billion was eventually authorized. Thus, a one percentage share cut is equivalent to a loss of \$2.16 billion in a state's 1998-2003 funding level compared to what it would have received if the previous share had been maintained. All three coalition builders took the funds gained from targeting a few populous states and distributed them across many others, thus broadly conferring percentage share increases. As a result of these

²³ His formula gave heavy weight to factors measuring the extent of highway use: vehicle miles traveled in the state, diesel fuel consumed in the state, and the state's annual contribution to the federal Highway Trust Fund.

²⁴ The formula placed less weight on highway use factors, greater weight on highway mileage, and included factors to measure population density (conferring bonuses on states with low densities) and freeze-thaw rates.

²⁵ All these formulas made use of adjustment factors to ensure that the distributive patterns reflected coalition builders' intentions. Coalition builders are not constrained by the outcomes produced by the formula factors they select; these merely provide a baseline before adjustments are factored in. As Chafee explained: "When you run something like that [the formula], you frequently end up with difficulties. Not everything comes out just the way you want it. So we made adjustments to the best of our ability" (Cong. Rec. 1998, S5392).

²⁶ Compared to the allocations under ISTEA, Chafee's formula redistributed 5% of the shares, whereas Warner and Baucus each redistributed approximately 8% of the shares.

²⁷ Share cuts are calculated by subtracting the percentage of the total funds that the state received under ISTEA from the percentage of the total funds the state would receive under each formula. Under ISTEA, for example, New York received 6% of the total transportation funds and would only receive 4% of the total under the Baucus formula, for a cut of two percentage shares.

TABLE 1. A Comparison of Coalitions and Coalition-Building Efficiency, 1997 Senate Surface Transportation Legislation

Outland Harder Telling					
	(R-VA)	Baucus (D-MT)	(R-RI)		
Varlable	(S. 335)	(S. 532)	(S. 586)		
Number of states in the coalition	13ª	22 ^b	15 ^c		
Population of average state in the coalition (in millions) Percentage of total funds	7.5	3.2	5.3		
allocated to average state in the coalition ^d Percentage increase in share	3.1	1.6	2.2		
for average state in coalition Percentage increase in share	15	26	5		
for coalition builder's constituents	16	25_	8		

Note: Data on states' shares are from the Federal Highway Administration and were published in Congressional Quarterly Weekly Report (Hosensky 1997, 1067).

"States: AR, 9C, KY, AL, WI, TN, IN, GA, NC, MI, OH, FL, TX.

"States: WY, ND, 8D, ID, NV, NE, NM, UT, KS, MS, IA, OR, OK, CO, AZ,

LA, MN, MO, WA, IL

"States" VT, AK, DE, NH, HI, ME, WV, CT, MD, MA, NJ, PA, NY, CA *Contition leaders' own states are excluded from these calculations.

strategic choices, it appears that any of the formulas-if presented alone on the Senate floor-could have garnered a filibuster-proof majority to defeat the status quo.

Efficiency of Coalitions

Yet, no Senate majority preferred any one of these formulas to the other two. The Senate divided roughly into thirds: 26 senators (13 states) preferred the Warner formula; 44 senators (22 states) preferred the Baucus formula; and 30 senators (15 states) preferred the Chafee formula. Under Senate rules, regardless of which formula the committee reported, other coalition builders could offer their alternative.

Just as in 1991, gaining the votes of senators from populous states required a larger proportion of funds than gaining the votes of senators from less populous states. Efficiently constructed coalitions thus contained fewer large states and more small states. Table 1 presents data on the composition and efficiency of these three coalitions: their size, the percentage of the total funds offered to coalition members, the funding increases offered over existing law, and the increases that coalition builders proposed for their own constit-

Table 1 indicates that the Warner coalition was the least efficient, that is, the average state in the coalition consumed a larger share of the program funds than the average state in either of the other two coalitions. The Warner group consisted of medium-sized and large states from the Midwest and South, and the average state had a population of 7.5 million and cost 3.1% of the funds. Relative to existing law, Warner proposed substantial funding increases for his coalition—a 15% increase in the average coalition state's share—and a 16% increase for Virginia. Given the high cost of the coalition's members, it is no surprise that the Warner group was also the smallest. Only 13 states preferred his formula to the others.

Like Moynihan's strategy for ISTEA, Chafee offered more funding for a few populous states—New Jersey, New York, and California—and for many small states. The average state in the Chafee coalition had a population of 5.3 million and cost 2.2% of the total funds. Accordingly, it was both more efficient and larger (15 states) than Warner's. There would be modest funding share hikes for the states in his coalition—5% on average—and an 8% increase for Rhode Island. These relatively lower funding increases are the result of the less dramatic shifts that Chafee proposed in existing funding levels.

The Baucus coalition was both the largest and the most efficient. It consisted of about half the small states, several smaller-than-average southern states, and no state more populous than Illinois; the average member had a population of only 3.2 million and only cost 1.6% of the program funds. Although Baucus and Warner redistributed the same percentage of the funds—about 8% of ISTEA—Baucus constructed his coalition far more efficiently than Warner. Even though Baucus proposed the largest funding increase for his own constituents of any coalition builder—a 25% increase over the status quo—the coalition's efficiency enabled him to build the largest group (22 states) and even offer members the largest funding increases of any coalition builder, an average of 26%.

Analysis of variance was conducted to determine whether the differences among the three coalitions are statistically significant. For the differences in the mean percentage of the funds allocated to the average state in the coalitions²⁸ and the mean percentage increases offered coalition states,29 the F-statistics are statistically significant. For the differences in the population of the average state in the coalition, the F-statistic is 2.7 (p <.10).

Outcome

Because no formula could command a majority, the three coalition leaders bargained in committee for seven months in an attempt to formulate a proposal that would prevail on the Senate floor. The same logic of coalition building continued to apply at this stage. Because of their lower funding demands, small-state senators remained more attractive as coalition members than other senators; they emerged as power brokers on the committee and strongly influenced the bill eventually reported. Baucus and three other smallstate senators, Dirk Kempthorne (R-ID), Harry Reid (D-NV), and Craig Thomas (R-WY), were "kingmakers in the committee, able to tilt the balance of power one way or the other" (Hosansky 1997, 1068).

The F-statistic is 3.1 (p < .05).

²⁹ The F-statistic is 8.2 (p < .01).

Kempthorne observed: "We've had lots of interest by different players...we're in a pretty good spot" (Hosansky 1997, 1068). Early in the bargaining process, Warner sought an alliance with Baucus. "I think there's a lot of mutuality," Warner said, "if we could get a merger, we have a very strong block of states" (Hosansky 1997, 1068).

Warner and Baucus did, in fact, ally around a new proposal (Weisman 1997b). This required a substantial sacrifice from Warner, who had to abandon his plan to ensure that every state would receive back at least 95% of its contributions to the Highway Trust Fund, a longstanding goal of large-state senators that Warner had supported for years. Explaining this on the floor, Warner said: "It had been our hope to reach 95 cents [on the dollar] but we soon recognized that we could not do it if we were to build a successful coalition and to properly recognize the individual requirements of certain geographic regions of the United States, primarily the western states" (Cong. Rec. 1997, S9254). The best that Warner could assure his large-state colleagues was a 90% return on their highway taxes.

After the Warner and Baucus coalitions converged, committee chair Chafee eventually gave them his support. Warner discussed the history of the alliance on the Senate floor: "As subcommittee chairman, I started out with a group called Step 21 [his initial formula] and then eventually we joined forces with a group headed by Senator Baucus—Stars 2000 is my recollection—and eventually our distinguished chairman joined us" (Cong. Rec. 1997, S10770). In joining the coalition, Chafee obtained a modest funding share increase for Rhode Island and for most of the other small states in his coalition. Chafee's large-state members were the principal losers under the formula eventually reported. Eighty percent of the funding share cuts imposed in the committee bill were taken from large-state members of Chafee's coalition.³⁰ The committee reported its bill to the Senate floor on October 1, 1997, by a vote of 18-0.

The Small-State Advantage in 1997–98 Formula Politics

Under the committee's formula (S. 1173), 35 states received funding share increases compared to the 1991 formula (ISTEA), and 15 states suffered cuts. Within both these groups, however, some states did better than others. A multivariate regression model was employed to determine the effect of state size on the outcome (n = 50). The dependent variable is States' Percentage Increase or Decrease in the Senate's 1998 Surface Transportation Bill (S1173) Relative to 1991 Levels.

The independent variable is *Reciprocal of State Population* (1/state population in millions). The transformation of state population is needed to reflect the

²⁰ The funding share reductions were taken from Massachusetts (40%), Pennsylvania (17%), New Jersey (7%), New York (6%), and Connecticut and Maryland (4% each)—all members of the Chafee coalition and underrepresented in the Senate compared to the standard of one person, one vote.

nonlinear relationship between state population and the dependent variable (see also Lee 1998, 47). Because the populous states receive such large sums in absolute terms, relatively small cuts in their share can finance large increases for the smallest states. A cut of 1% in California's 1991 transportation share (compared to ISTEA) is equivalent to \$199 million, a sum that could, for example, fund an 86% increase in Wyoming's share. Because of the very different sums involved when senators adjust the shares of large and small states, the effect of state population on Senate outcomes is expected to follow the Phillips curve.³¹ Under this functional form, the effect of state population on Senate outcomes is greatest for the smallest states, but the slope of the line flattens out at the higher values of state population. In other words, the model predicts that the smallest states will experience dramatic funding percentage increases, medium-sized states small percentage increases, and large states very small increases or cuts.

Control variables are included to account for states' other political advantages in the process and for changes in their need for federal transportation dollars between the early and late 1990s. To address the possibility that party and committee representation affect outcomes, three variables are included. First, States' Partisan Representation controls for the effect majority party status has on distributive politics. The Democrats held a majority in the Senate during the 1991 transportation reauthorization; the Republicans were in the majority in 1997 and 1998. Did the change in party control benefit states represented by Republicans? The variable is coded 0 for states with no Republican senator, 1 for states with one Republican senator, and 2 for states with two Republican senators. Second, a dummy variable, State Represented by Ranking Member on Committee, accounts for the advantages of committee leaders, coded 1 if a state is represented by a senator who is a chair or a ranking member on either the committee or subcommittee, 0 otherwise. Third, a dummy variable, State Represented on Committee, accounts for any advantages that states gain from committee representation, coded 1 for all states represented by senators who are members of the Environment and Public Works Committee, 0 otherwise.

To address the effect of changes in state need for federal transportation funds, two control variables are included: Percentage Change in States' Federal-Aid Highway Mileage and Percentage Change in the Vehicle Miles Traveled in the State.³² Clearly, one would expect states that have experienced increases in the use or extent of their highway system to receive additional dollars.

Finally, to account for the incidence of majority rule cycling between 1991 and 1997, the model includes a

³¹ For a discussion and illustration of this functional form, see Brown 1991, 130.

²² Data are from the Office of Highway Information Management, Federal Highway Administration, for 1990 and 1995 (the last year for which data are available).

TABLE 2. States' Percentage Funding Increase or Decrease In the Senate's 1998 Surface Transportation Bill (S. 1173) Relative to 1991 Levels, Regressed on State Population and Control Variables

Varlable	В	Standard Error
Reciprocal of state population (in millions)	3.68**	1.15
State represented on committee	-0.345	4.16
State represented by ranking member on committee	6.61	8.18
State's partisan representation	5.45*	2.39
Per-capita percentage share under ISTEA	-23231.60*	10878.23
Percentage change in vehicle miles traveled	.17*	.09
Percentage change in federal ald highway mileage	.10	.12
Constant -	-10.62	7.33
F	3.60**	
Adj. R ²	.27	
Mean	2.6	
N	50	
p < .05, ** $p < .01$; one-tailed tests.	-	

variable identifying the states that were winners in the 1991 transportation battle, *Per-Capita Percentage Share under ISTEA*. In attempting to retain the ISTEA apportionments, for example, Chafee had to contend with many senators' perceptions that "the Northeast made out like bandits last time" (Ota 1998a, 656). Given this kind of concern, states that were winners in the formula battle of 1991 were likely to receive smaller increases or even cuts in 1997; thus, the sign on this variable is expected to be negative.

The results of the regression are shown in Table $2^{.33}$ The findings provide strong support for the hypothesis that small-state senators are advantaged in the coalition-building politics of funding formulas. The model reveals a strong relationship between state population and the state's funding increase or cut in the Senate bill. The coefficient on the reciprocal of state population is $3.68 \ (p < .01)$, which reveals that less populous states received substantially larger increases in transportation funding than did more populous states.

The control variables also seem to affect funding patterns in the Senate bill. Change in Senate party control had a statistically significant effect: States represented by Republicans tended to receive larger increases in transportation funding than states represented by Democrats. States with increased levels of highway use (measured by percentage change in the

vehicles miles traveled in the state) received additional funds (B=.17, p<.05). In addition, states that fared well under the 1991 transportation formula tended to receive smaller funding increases or, in some cases, cuts in share (p<.05). Although the signs on the other control variables are in the expected direction, their coefficients do not reach statistical significance.³⁴

Holding the control variables at their means, the model predicts that a state the size of Wyoming would receive an increase of 36% in the Senate's 1997 bill relative to its 1991 funding level, while a state the size of California would receive a funding share cut of 3.6%. Given the total amount appropriated for transportation in 1998, such a cut is equivalent to a loss of more than \$717 million. In sum, the data clearly reveal that the final 1997 Senate committee bill reflected the preferences of small-state senators more closely than those of other senators.

After trouble on the floor in 1997 and two weeks of debate in 1998 that focused on the distribution of funds to the states (Ota 1998b, 1998c; Weisman 1997a), the committee's formula was approved by the Senate on March 12, 1998, with a 96-4 vote. Before passage, some large-state senators-including Levin of Michigan and Specter of Pennsylvania-complained about the distribution of funds (Cong. Rec. 1998, S1848–50). No doubt, the large funding increases authorized which were 40% over the 1991-96 level, helped mollify the losers. Every state except Massachusetts received dollar increases over previous levels, even though 15 states had their funding shares reduced. It is clear that Warner was more pleased with the outcome in 1998 than in 1991. He had succeeded in raising the funding share for all the middle-sized southern states, particularly Virginia. In addition, he obtained small share hikes for the populous southern states, Florida and Texas. Warner could only secure these gains by increasing small states' shares above the 1991 formula level and by substantially cutting the shares for several populous states.

Warner's success suggests that he had learned some things since his defeat in 1991 about how to build successful Senate coalitions in funding formula politics. His 1997 strategy eventually mirrored that of Moynihan in 1991: Procure funding increases for one's own state by constructing an efficient Senate coalition. Doing so means securing small-state senators' votes using the funds gained by excluding some populous states from the coalition. By accepting smaller increases than he wanted for his own coalition members and by focusing share cuts on a few populous states, Warner was able to merge his group with Baucus's, thus building the supermajority coalition necessary to succeed in the Senate. As in 1991, the Senate's formula largely prevailed in conference with the House.³⁵

25 The only significant change between the Senate bill and the

³³ OLS regression statistics were calculated using SPSS 8.0 for Windows.

³⁴ In order to determine whether majority party members of the Environment and Public Works Committee received additional advantages, an interaction term (state's representation on committee × state's partisan representation) was added to the model. The coefficient was not statistically significant, and the variable did not add any explanatory power to the equation.

CONCLUSION

It goes back to the very fundamental principle, Mr. President, the very fundamental principle that when our forefathers laid down this Government and fashioned this Republic, the oldest surviving democratic form of republic on earth today, the United States with its Constitution and Bill of Rights, when they fashioned that, they recognized letting the states have equal representation in the Senate...so we strengthened the financial position of the western states.

—Senator John Warner (R-VA), Congressional Record, September 12, 1997, S9255

Equal representation of states in the Senate, "the very fundamental principle" Warner invokes, affects Senate politics in a crucial but long-ignored way. Senate apportionment creates a situation in which it is much less expensive to obtain some votes than others. In the context of funding formula controversies, it is clear that senators prefer the formula that will distribute the most funds to their state. Similarly, senators who build coalitions in support of formulas do so (at least in part) in order to increase funding to their own state. Because state need for federal funds corresponds closely with state population size, it is relatively inexpensive to provide funding increases for small states. As a result, coalition leaders who seek support for formulas that benefit their own state can both build winning coalitions and offer larger increases to their own states by including small-state senators and excluding their large-state counterparts. In sum, coalition leaders in the Senate have strong incentives to "strengthen the financial position" of less populous states.

The surface transportation battles of the 1990s clearly reveal these coalition-building dynamics. In both 1991 and 1997, the coalitions containing more small-state senators were less expensive to build, with the cost of the average vote being substantially lower in these coalitions than in those containing more largestate senators. Coalition-building efficiency gives senators two advantages. First, it enables them to provide more funds for their own constituents. In 1991 Moynihan both built a winning coalition and obtained a generous allocation for New York by devising a formula that all the small-state senators and a few largestate senators preferred to the alternative proposed. In 1997 Warner compromised his longstanding goal of increasing funding levels for all the large states and instead excluded a number of them from his coalition. using those funds to gain the support of small-state senators. In this way, he raised the funding share for his own state. Second, building coalitions efficiently enhances the chance of success. Efficiency means that more funds are available to spread across other states and thus garner additional votes. In 1991, the more efficient coalition won outright. In 1997, Warner merged with the highly efficient Baucus coalition to

conference report was a one percentage share increase for Pennsylvania, doubtless the result of the influence of conference committee chair and House Transportation and Infrastructure Committee chair Bud Shuster (R-PA). The increase was produced by tiny percentage share cuts spread across 35 states.

support a formula that could garner the supermajority needed to prevail.

Because they are so attractive to coalition builders, small-state senators have an advantage in funding formula disputes. They possess the votes needed to build efficient coalitions. For this reason, coalition leaders accommodate their demands to a greater extent than those of others. The bills passed by the Senate after both the 1991 and 1997 transportation formula fights more closely reflect the preferences of small-state senators than their large-state colleagues. In 1991, the formula preferred by small-state senators won outright. In 1998, the final bill gave larger funding increases to small states than to others.

These coalition-building dynamics have considerable consequences for understanding distributive policy in the United States. The overwhelming majority of all federal domestic assistance funds—in fiscal 1995. 90%—is allocated by congressionally mandated formulas (Advisory Commission 1995, 14). Despite this, political scientists have taken relatively little interest in funding formulas, focusing instead on the distribution of project grants and earmarked funds in appropriations legislation. Such a preference almost certainly derives from the dominant theoretical approach in the field, the study of how powerful members of Congress direct funds to their district and state. From this perspective, formula grants lack interest because they are stable for long periods and thus do not closely reflect the distribution of committee and party power in a particular Congress. The longevity of funding formulas, however, makes the small state's advantage in formula politics all the more significant. The distribution of committee and party power changes over time, but small states are always advantaged in the Senate. Regardless of when programs are created or reauthorized, small-state senators are always well positioned to benefit from their attractiveness to coalition builders.

The findings from this study have important implications for the theoretical and empirical study of congressional politics. Theoretical work on coalition building typically conceptualizes divide-the-dollars games as symmetric, meaning that the value of a coalition is contingent solely on its size. In the Senate, however, the game is not symmetric: Smaller coalitions are not necessarily able to provide higher payoffs than larger ones. Instead, coalitions are constructed efficiently when they include certain players rather than others. Coalition builders can obtain small-state senators' votes at a lower price than large-state senators' votes. As a consequence, coalitions containing smallstate senators provide greater payoffs to members and leaders than do coalitions containing large-state senators. This study points to the need for models of coalition building that take Senate apportionment into account.

Empirical work on distributive politics tends to neglect coalition building in favor of studying legislative outcomes. Indeed, nearly all the scholarship on coalition building in Congress has been in formal theory. Empirically oriented researchers need to pay more attention to the strategies, alternatives, and incentives of legislators as they construct coalitions. The study of coalition-building processes will, of course, enhance understanding of legislative outcomes. The findings presented here help explain the pervasive small-state advantage in the distribution of federal funds that has been documented elsewhere (Atlas et al. 1995; Lee 1998; Lee and Oppenheimer 1999). Going beyond outcomes will also make possible a fuller, more complete knowledge of how the structures of American political institutions—including Senate apportionment—shape policymaking.

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A Bargaining Model of Collective Choice

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e provide a general theory of collective decision making, one that relates social choices to the strategic incentives of individuals, by generalizing the Baron-Ferejohn (1989) model of bargaining to the multidimensional spatial model. We prove existence of stationary equilibria, upper hemicontinuity of equilibrium outcomes in structural and preference parameters, and equivalence of equilibrium outcomes and the core in certain environments, including the one-dimensional case. The model generates equilibrium predictions even when the core is empty, and it yields a "continuous" generalization of the core in some familiar environments in which the core is nonempty. As the description of institutional detail in the model is sparse, it applies to collective choice in relatively unstructured settings and provides a benchmark for the general analysis of legislative and parliamentary politics.

central finding in the spatial model of collective choice, known at least since the work of Plott (1967), is that in two or more dimensions the majority rule core, consisting of those alternatives unbeaten in pairwise majority comparisons, is empty for most configurations of individual preferences (Cox 1984; Davis, DeGroot, and Hinich 1972; Le Breton 1987; McKelvey and Schofield 1987; Plott 1967; Rubinstein 1979; Schofield 1983). In addition, McKelvey (1976, 1979) has shown that the majority preference relation is particularly badly behaved when the core is empty: Any one alternative can be reached from any other through a series of pairwise majority comparisons, which makes it difficult to place any bounds on the possible outcomes of collective decision making (see Cohen 1979; Cohen and Matthews 1980; and Schofield 1978 for related "chaos" results). Furthermore, these results are not peculiar to majority rule but extend to all voting rules (short of ones such as dictatorship and unanimity) in one form or another (Austen-Smith and Banks 1999; Banks 1995; Greenberg 1979; Saari 1997; Schofield 1984; Strnad 1985). Therefore, the simple application of majority (or some other) rule is often insufficient to generate useful predictions and, as a consequence, is lacking as the basis for a general theory of collective decision making.

These negative results concerning the core had a profound effect on subsequent research in political science. Indeed, Weingast (1989, 975) states that "the literature on the positive theory of legislatures remains under the spell of McKelvey and his colleagues." One of the more popular escape routes, beginning with the work of Kramer (1972) and Shepsle (1979), has been to identify legislative procedures and arrangements as

elements of more explicit decision-making processes, ones that work to constrain the chaotic tendencies of the majority preference relation. These "structure-induced" equilibrium approaches typically generate predictions when no "preference-induced" (i.e., core) outcomes are to be found, and they highlight the relevance of incorporating real-world features into models of political phenomena. Our goal, in contrast, is a model that incorporates less institutional detail. Such a model would serve as a benchmark for the general analysis of legislative and parliamentary politics but could be applied to describe collective decision making even in relatively unstructured settings.

With this in mind, we assume that the decision-

making process takes the form of bargaining. Specifically, we embed the noncooperative bargaining model of Binmore (1987) and Baron and Ferejohn (1989) in the general spatial model of politics in which two or more individuals must select an alternative from a compact, convex subset of multidimensional Euclidean space. Individuals' preferences are represented by continuous and concave utility functions but are otherwise quite arbitrary; thus, besides the classic spatial model of politics, we admit "divide-the-dollar" problems, exchange economies, and public goods economies (with or without private goods). In common with most of the bargaining literature, the decision process is one of discrete time and infinite horizon, and in each period a single proposal is made and is either accepted, stopping the process, or rejected, moving the process to the next period. Individuals may or may not discount future utility, and discount rates are allowed to vary among the individuals. In contrast to the models of Rubinstein (1982) and others, the identity of the proposer does not alternate in a fixed sequence; rather, the proposer is randomly selected, or "recognized," in each period. The recognition probabilities are time-invariant but otherwise unconstrained. Finally, passage of a proposal requires the assent of one from a collection of "decisive coalitions," which is time-invariant and is arbitrary save for the minimal conditions of nonemptiness and monotonicity.

We prove the existence of stationary "no delay" equilibria, in which proposals and voting decisions are independent of the time period and history of the game

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and in which the first proposal is always accepted. Furthermore, we show that when discount rates are all less than one, delay cannot occur in any stationary equilibrium. We show that the set of equilibrium outcomes varies upper hemicontinuously in the recognition probabilities and the individuals' discount factors, as well as in various utility-specific parameters (e.g., ideal points in the case of quadratic utilities). Thus, "small" variations in the parameters of the model cannot cause the set of equilibrium outcomes to "blow up." We also provide a two-dimensional, three-person, majority-rule example with impatient individuals in which multiple equilibrium payoffs arise, thereby precluding any universal uniqueness result.

Our existence result requires that individuals be allowed to mix over their proposals, but for a restricted class of environments we prove the existence of pure strategy stationary equilibria. Interestingly, the sufficient conditions for pure strategy existence turn out to be well-known conditions sufficient for nonemptiness of the core of the voting rule, as described by the set of decisive coalitions: We can assume either that the policy space is one dimensional or that there is a privileged group (an "oligarchy") with the power to pass a proposal, each member of which possesses a veto. As a special case of the latter assumption, we obtain pure strategy equilibria under unanimity rule.

We next turn to the relationship between the equilibrium outcomes in the bargaining game and the core outcomes (if any) of the underlying voting rule. We show that, when the individuals are perfectly patient and utility functions are strictly quasiconcave, equivalence of stationary equilibrium outcomes and core outcomes obtains if the policy space is one dimensional (in which case, under majority rule, the core consists of the median policy or policies) or if some individual possesses a veto, the latter condition weaker than the existence of an oligarchy but also known to be sufficient for nonemptiness of the core. In the one-dimensional case, therefore, our bargaining model provides a version of the Median Voter Theorem, as in Black's (1958) model of committee decision making and in Downs's (1957) model of competition between political parties. We also show that if individuals' utilities are quadratic, if the voting rule is "strong" (e.g., majority rule with an odd number of individuals), and if a core point exists, then core equivalence once again obtains. Using our upper hemicontinuity result, we find that if preferences are "close" to being quadratic and admitting a nonempty core, and if individuals are sufficiently patient, then the outcomes of the bargaining process will be "close" to core outcomes; and if there is a unique core outcome, then all equilibrium outcomes will be close to one another as well. Thus, we can view the model as providing a "continuous" generalization of the core: When the core is nonempty, the equilibrium outcomes of the process coincide with the core, while the small changes in preferences that may cause the core to become empty have only small consequences on the equilibrium outcomes.

In related work, Binmore (1987) analyzes a model of bargaining among two individuals, in which the alter-

natives are divisions of a "dollar," individuals have selfish preferences, and unanimous agreement is required to pass a proposal. Baron and Ferejohn (1989) analyze that model allowing for multiple individuals and assuming majority rule is used. Whereas the latter authors assume utility equal to the amount of the dollar received, Harrington (1989, 1990a, 1990b) extends the model to allow for risk aversion; Calvert and Dietz (1996) allow for externalities in consumption of the dollar. Baron (1991) examines the case of a twodimensional set of alternatives, three voters with quadratic preferences, and voting by majority rule. Winter (1996) analyzes the advantage of veto players over others in the divide-the-dollar problem, assuming at least one individual has a veto; McCarty (1998) takes up the same subject, allowing for variable veto power across individuals. Jackson and Moselle (1998) consider the case in which a one-dimensional public decision is selected along with a division of a dollar using majority rule; they investigate the nature of coalitions "formed" in equilibrium. Merlo and Wilson (1995) study the divide-the-dollar problem under unanimity rule and allow the size of the dollar to change stochastically over time, which generates (efficient) equilibrium delay as a consequence. Notably, they prove uniqueness of stationary equilibrium payoffs in their model.

Other work on bargaining and the core stems from Selten's (1981) model, in which a proposal to a coalition consists of a vector of payoffs "feasible" for the coalition. Chatterjee, Dutta, Ray, and Sengupta (1993) analyze a transferable utility game with discounting and a fixed "protocol" describing the order of proposers and respondents, with the first rejector becoming the new proposer. They show how delay is generally possible even in stationary equilibria, and they provide conditions under which delay cannot occur. Moldovanu and Winter (1995) examine a nontransferable utility game with no discounting and with a similar "rejector becomes proposer" feature, and they prove an equivalence between the core and stationary "order independent" equilibrium outcomes. Okada (1996) studies a transferable utility game with equal discount rates and a random recognition rule with equal recognition probabilities. He shows that delay does not occur in equilibrium and provides conditions sufficient for the existence of efficient equilibria as the common discount rate goes to one.

THE MODEL

Let $X \subseteq \mathbb{R}^d$ denote a nonempty, compact, convex set of alternatives, with $d \geq 1$, and let $N = \{1, \ldots, n\}$ denote the set of individuals, with $n \geq 2$. Individuals interact to select a single alternative from X, with the timing of interaction as follows: (1) At $t \in \{1, 2, \ldots\}$, individual $i \in N$ is recognized with probability ρ_i , where $\rho = (\rho_1, \ldots, \rho_n) \in \Delta$, the unit simplex in \Re^n ; (2) if recognized, i makes a proposal $p_i^t \in X$; (3) all $j \in N$ simultaneously vote either to accept or to reject the proposal. The recognition probabilities, ρ_1, \ldots, ρ_n , are exogenously fixed throughout the game. Let $\mathfrak{D} \subseteq \mathbb{R}$

 $2^{N}\setminus\{\emptyset\}$ denote a collection of coalitions, called *decisive*, also exogenously fixed throughout the game. If $\{j \in N | j \text{ accepts}\} \in \mathfrak{D}$, then the proposal p_i^t is the chosen alternative, and the game ends; otherwise, the process moves to period t+1 and is repeated.

In general, we impose on the voting rule \mathfrak{B} only the minimal conditions that it is nonempty and *monotonic*: $C \in \mathfrak{D}$ and $C \subseteq C'$ imply $C' \in \mathfrak{D}$. (Together, these two assumptions imply $N \in \mathfrak{D}$.) Thus, we allow for a variety of voting rules beyond such common examples as majority rule $(\mathfrak{D} = \{C \subseteq N | | C| > n/2\})$, unanimity rule $(\mathfrak{D} = \{N\})$, and dictatorship $(\mathfrak{D} = \{C \subseteq N | i \in C\})$ for some $i \in N$. For instance, it could be that the set of individuals is partitioned into two "houses," N_1 and N_2 , and that majorities from each house are required to pass a proposal:

$$\mathfrak{D} = \left\{ C \subseteq N \middle| \left| C \cap N_1 \right| > \frac{n_1}{2} \text{ and } \left| C \cap N_2 \right| > \frac{n_2}{2} \right\},\,$$

where $n_1 = |N_1|$ and $n_2 = |N_2|$. Adding an "executive" with a veto is straightforward. We also allow for more unusual voting rules, such as when \mathfrak{D} contains all nonempty coalitions; in this instance, a proposal passes if at least one individual votes to accept.

Some of our results, however, do place additional qualitative restrictions on the voting rule. Say that \mathfrak{D} is (1) proper if $C \in \mathfrak{D}$ implies $N \setminus C \notin \mathfrak{D}$; (2) collegial if $\bigcap_{C \in \mathfrak{D}} C \neq \emptyset$; (3) oligarchic if $\bigcap_{C \in \mathfrak{D}} C \in \mathfrak{D}$; and (4) strong if $C \notin \mathfrak{D}$ implies $N \setminus C \in \mathfrak{D}$. Thus, properness precludes the possibility of disjoint decisive coalitions, while collegial rules are those that give at least one individual (belonging to each decisive coalition) a veto; and a collegial rule is oligarchic if the vetoers are decisive by themselves. Prime examples of oligarchical voting rules are dictatorship and unanimity. An easy example of a strong rule is majority rule when n is odd. We next give a "one-parameter" illustration of all these concepts.

Example 1. Let n = 5, and consider a weighted q-rule with the following weights assigned to the individuals: $w_1 = w_2 = .35$, $w_3 = w_4 = w_5 = .1$. The set of decisive coalitions is then $\mathfrak{B} = \{C \subseteq N | \Sigma_{i \in C} w_i \ge q\}$, where q is fixed in (0, 1]. For $q \in (.45, 1]$, the rule is proper, since a decisive coalition must contain at least one "large" individual and either the other large one or two "small" ones, in which cases the complement cannot be decisive. For $q \in (.65, 1]$ the rule is collegial, as the membership of the two large individuals is necessary for a coalition to be decisive. For $q \in$ (.65, .70] the rule is in fact oligarchic, since a decisive coalition requires both large individuals to be members, and these individuals together constitute a decisive coalition. Similarly, for $q \in (.90, 1]$ the rule is oligarchic, since the only decisive coalition is N. Alternatively, for $q \in (.7, .9]$ the rule is collegial but not oligarchic, because the only individuals needed for a decisive coalition are the large ones, but they do not

make up a decisive coalition by themselves. Finally, the rule is strong when $q \in (0, .55]$, since a coalition or its complement must include either both large individuals or one large and two small individuals; whichever one does is a decisive coalition.

Each individual i's preferences over outcomes are described by a von Neumann-Morgenstern utility representation $u_i: X \to \Re$ and a discount rate $\delta_i \in [0, 1]$ as follows: If $x \in X$ is accepted in period $t \in \{1,$ 2, ...}, then i's payoff is $\delta_i^{t-1}u_i(x)$; if no alternative is ever accepted, then each individual receives a utility of zero. We assume throughout that each u_i is continuous and concave and that $u_i(x) \geq 0$ for all $x \in X$; moreover, we assume there is some $x \in X$ such that, for all $i \in N$, $u_i(x) > 0.2$ For some of our results, we also assume that each utility representation u, is strictly quasiconcave in this sense: For all $x, y \in X$ with $x \neq y$ and for all $\alpha \in (0, 1)$, $u_t(\alpha x + (1 - \alpha)y) > \min\{u_t(x),$ $u_i(y)$. Geometrically, this means that the set of alternatives weakly preferred to any x by individual i is "strictly" convex. For other results, we use a weaker condition, defined as follows. Denote i's weak and strict upper contour sets at x, respectively, by

$$R_{i}(x) = \{ y \in X | u_{i}(y) \ge u_{i}(x) \},$$

$$P_{i}(x) = \{ y \in X | u_{i}(y) > u_{i}(x) \},$$

and let

$$R_C(x) = \bigcap_{r \in C} R_r(x),$$

 $R(x) = \bigcup_{C \in S} R_C(x),$

with similar conventions used for $P_C(x)$ and P(x). That is, $R_C(x)$ consists of the alternatives that every member of C weakly prefers to x, and R(x) consists of the alternatives weakly preferred to x by all members of some decisive coalition, with similar interpretations of $P_C(x)$ and P(x). Given $Y \subseteq X$, let \overline{Y} denote the closure of Y. We say that the condition of *limited shared weak preference* (LSWP) holds if, for all $C \subseteq N$ and all $x \in X$,

$$|R_C(x)| > 1$$
 implies $R_C(x) \subseteq \overline{P_C(x)}$.

That is, if y (distinct from x) is weakly preferred to x by all members of a coalition C, then it can be approximated by alternatives that all members of C strictly prefer to x. Many familiar environments, including those below, satisfy LSWP.

• Classical spatial model/Pure public goods. Each u_i is strictly quasiconcave, as when there exists x^i such that $u_i(x) = k_i - ||x - x^i||$ or $u_i(x) = k_i - ||x - x^i||^2$. Or, if alternatives represent public goods, each u_i may be monotonic as well.

¹ In the terminology of cooperative game theory, ² describes a simple game, and the elements of ² are "winning coalitions."

 $^{^2}$ We implicitly assume here that the status quo (the outcome prevailing in the absence of agreement) has a utility of zero; by our normalization of utilities, it follows that the alternatives in X are unanimously weakly preferred to the status quo. In our concluding discussion, we explain that this restriction can be relaxed if the discount rates of the individuals are identical.

- Public decisions with transfers. $X = Z \times T$, $T \subseteq \Re^n$, and each u_i is quasilinear: $u_i(z, t) = \phi_i(z) + t_i$, ϕ_i strictly quasiconcave.
- Exchange economy. Alternatives are allocations of private goods, and each u_i is strictly quasiconcave and strictly monotonic in i's consumption.
- Divide the dollar. $X = \Delta$, and $u_i(x) = x_i$.

To see why LSWP holds in the classical spatial model, for example, suppose all members of coalition C weakly prefer some alternative y to $x \neq y$. By strict quasiconcavity, every convex combination of x and y (with positive weight on both) is strictly preferred to x by all members of C. Letting the weight on x go to one, we approximate x by alternatives in $P_C(x)$, as required. Banks and Duggan (1999) prove that LSWP holds in two general models, from which all the above examples can be obtained as special cases.

Complete information about preferences, the structure of the game form, and so on, is assumed throughout. A history of length l in the game describes all that has transpired in the first l periods (who the previous proposers were, what they proposed, how individuals voted) as well as whether in the current period we stand prior to the proposer being recognized, after such recognition but prior to the proposal being made, or after the proposal but prior to the vote. Therefore, in general a strategy for an individual would be a mapping that specifies an intended action (what to propose, how to vote) as a function of all histories of all lengths. Since our focus is only on equilibria in stationary strategies, we shed unneeded generality and provide a formal definition only of such strategies. A (pure) stationary strategy for $i \in N$ consists of a proposal $p_i \in X$ offered any time i is recognized and a measurable decision rule $r_i: X \to \{\text{accept, reject}\}\$, or equivalently an acceptance set $A_i = r_i^{-1}$ (accept).

It turns out that mixtures over proposals are required for our most general existence result, so let $\mathfrak{P}(X)$ denote the set of Borel probability measures on X, and endow $\mathfrak{P}(X)$ with the topology of weak convergence. Given measurable $Y \subseteq X$, let $\mathfrak{P}(Y)$ denote the subset of probability measures on X that place probability one on Y. Let $\pi_i \in \mathfrak{P}(X)$ denote a mixed stationary proposal for i, and let $\pi = (\pi_1, \ldots, \pi_n)$ denote a profile of mixed stationary proposals. A mixed stationary strategy for i is a pair $\sigma_i = (\pi_i, A_i)$, and we let $\sigma = (\sigma_1, \ldots, \sigma_n)$ denote a profile of mixed stationary strategies. It is important to note that randomization takes place before voting. The individuals know which alternative has been proposed at the time they cast their votes.

Given a profile σ of mixed stationary strategies (π_i, A_i) , $i = 1, \ldots, n$, and given $C \subseteq N$, define the set

$$A_C = \bigcap_{i \in C} A_i$$

of proposals acceptable to all members of C, and define the social acceptance set

$$A = \bigcup_{C \in \mathbf{A}} A_C$$

which consists of proposals passed in any and all periods. The profile is a no-delay profile if $\pi_i(A) = 1$ for all $i \in N$ (implying, of course, that $A \neq \emptyset$). Let $S(\pi_i)$ denote the support of π_i , and let $S(\pi) = \bigcup_{i=1}^n S(\pi_i)$. We say σ has finite support if $S(\pi)$ is finite.

Informally, a profile σ^* constitutes a stationary equilibrium if, for all $i \in N$, π_n^* is optimal given the acceptance sets (A_1^*, \ldots, A_n^*) of the other individuals and A_i^* is optimal given that σ^* describes what would happen if the current proposal were rejected. To formalize these conditions, note first that any strategy profile σ defines in an obvious (if notationally dense) manner a probability distribution over the outcome space $(X \times \{1, 2, \ldots\}) \cup \{\emptyset\}$, and with it an expected utility $v_i(\sigma)$ for each $i \in N$ as evaluated at the beginning of the game; by stationarity this is also i's continuation value throughout the game, that is, i's expected utility as evaluated next period if the current period's proposal is rejected. In a no-delay stationary equilibrium, we can write the individual i's continuation value as

$$v_i(\pi) = \sum_{j \in N} \rho_j \left[\int_{\mathcal{X}} u_i(x) \pi_j(dx) \right],$$

a continuous function of π . That is, $v_i(\pi)$ is i's expected utility from the "continuation lottery," $\sum_{j \in N} \rho_j \pi_j$, associated with the profile of mixed proposals. In contrast, let

$$x(\pi) = \sum_{i \in N} \rho_i \left[\int_X x \pi_i(dx) \right]$$

denote the expected value of the individuals' proposals under the continuation lottery. By the concavity of u_i , we know that $u_i(x(\pi)) \ge v_i(\pi)$ in every no-delay stationary equilibrium, a feature that will prove quite useful below.

Formally, we require that the individuals' proposals satisfy sequential rationality and that their acceptance sets satisfy weak dominance, with the latter eliminating nasty equilibria in which, for instance, under majority rule everyone accepts $x \in X$ independently of preferences.⁵ The equilibrium condition on the acceptance sets is that for all $i \in N$,

$$[u_i(x) > \delta_i v_i(\sigma^*) \text{ implies } x \in A_i^*] \text{ and}$$

 $[u_i(x) < \delta_i v_i(\sigma^*) \text{ implies } x \notin A_i^*].$

³ Although LSWP is satisfied in divide-the-dollar environments, it does not hold for general linear u_i . For example, let n=2, X the unit ball in \Re^2 , where one individual has gradient (1,0) and the other has gradient (-1,0). Then, all points along the vertical axis are weakly preferred to the origin by both individuals, but because their gradients are exactly opposed, there are no alternatives that both strictly prefer to the origin.

⁴ That is, $S(\pi_i)$ is the intersection of all closed subsets Y of X such that $\pi_i(Y) = 1$.

⁵ Baron and Kalai (1993) refer to such strategies as "stage-undominated."

As for proposals, if A is nonempty, then individual i, when recognized as proposer, chooses either utility-maximizing outcomes from within A or an outcome that will be rejected, thereby generating a payoff of $\delta_i \nu_i(\sigma)$. Thus, the equilibrium condition on proposals is that for all $i \in N$,

$$\pi_i^*(\arg\max\{u_i(y)|y\in A^*\})=1$$

when $\sup\{u_i(y)|y\in A^*\} > \delta_i v_i(\sigma^*)$; that $\pi_i^*(XA^*) = 1$ when the inequality is reversed; and that π_i^* place positive mass only on the union of these two sets when equality holds.

THREE EXAMPLES

We offer a trio of three-person, majority-rule examples to illustrate several important properties of equilibrium behavior. In each, we assume equal recognition probabilities and common discount rates: $\rho_1 = \rho_2 = \rho_3 = \frac{1}{3}$ and $\delta_1 = \delta_2 = \delta_3 = \delta$.

Example 2. Let X = [-1, 1], and assume $u_1(x) = 2 - [-1 - x]$, $u_2 = 1 - |x|$, and $u_3 = 2 - |1 - x|$. That is, utility functions are negative one times Euclidean distance from the voters' ideal points (-1, 0, and1), plus constants to guarantee nonnegative utilities. We look for a symmetric equilibrium given by some $\alpha \in [0, 1]$, in which individual 1 proposes $p_1 = -\alpha$ if recognized, individual 2 proposes $p_2 = 0$ if recognized, and individual 3 proposes $p_3 = \alpha$ if recognized. Acceptance sets will be given by the discounted continuation values: Here, 1's and 3's continuation values are equal to one, and 2's is equal to $1 - 2\alpha/3$; after discounting, 1 accepts any $x \le 1 - \delta$, 2 accepts any $x \in$ $[-1+\delta-2\alpha\delta/3, 1-\delta+2\alpha\delta/3]$, and 3 accepts any $x \ge \delta - 1$. It is clear that $1 - \delta + 2\alpha \delta/3 \ge 1 - \delta$, so individual 3's optimal proposal will be the highest alternative (the one closest to 3's ideal point) that individual 2 will accept: $p_3 = 1 - \delta + 2\alpha \delta/3$. Using $p_3 = \alpha$, we solve these equations to find

$$\alpha = \frac{1-8}{1-\frac{2}{3}8}.$$

Given the proposal strategies specified above, the individuals vote to accept any alternatives that yield at least their discounted continuation values; and given the voting strategies specified above, the individuals' proposal strategies are indeed optimal. Thus, we have a stationary equilibrium.

Note that, in example 2, we have found an equilibrium in *pure* strategies, one that is actually continuous in the discount rate δ . Furthermore, $\alpha = 0$ when $\delta = 1$, which means every individual proposes the median (i.e., core) ideal point.

Example 3. Let X contain the unit simplex in \Re^3 ; let utility functions be quadratic, that is, $u_i(x) = k_i - ||x - x^i||^2$ where k_i is a constant term to guarantee nonnegative utilities; let ideal points x^1 , x^2 , x^3 be at the

respective unit coordinate vectors; and let $\delta=1$. This is pictured in Figure 1. As in Baron (1991), one mixed strategy equilibrium is given by $\pi_1^*(a)=\pi_1^*(b)=\frac{1}{2}$, $\pi_2^*(c)=\pi_2^*(d)=\frac{1}{2}$, and $\pi_3^*(e)=\pi_3^*(f)=\frac{1}{2}$, with acceptance sets circumscribed by indifference curves representing the individuals' continuation values; for example, individual 1's proposal strategy is optimal because a and b are equidistant from 1's ideal point, and because neither 2 nor 3 will accept any alternative closer to e^1 . Other strategies involving these six alternatives also yield stationary equilibria: Indeed, one pure strategy equilibrium is $\pi_1^*(a)=\pi_2^*(c)=\pi_3^*(e)=1$; another is $\pi_1^*(b)=\pi_2^*(d)=\pi_3^*(f)=1$.

Example 3 shows that no-delay stationary equilibria are not generally unique, but each individual's ex ante payoffs from the equilibria demonstrated are the same, which raises the issue of uniqueness of stationary equilibrium payoffs. While this property may hold in special environments (Eraslan 1999; Merlo and Wilson 1995), it does not hold generally. In our analysis of core equivalence, below, we give an example in which equilibria are not payoff equivalent, assuming $\delta = 1$. Our next example shows that nonunique equilibrium payoffs may occur even when all individuals' discount rates are below one.

Example 4. Let X be the unit simplex in \mathfrak{R}^3 , and assume $\mathfrak{d} = .95$. Again, we let individual *i*'s ideal point be at e^i , the *i*th unit vector, but now we define u_i by a particular monotonic transformation of Euclidean distance from e^i . Specifically, define the piecewise linear function f by

$$f(0) = 14,900$$
 $f(.25) = 14,894$ $f(.2762) = 14,890$
 $f(1.138) = 12,100$ $f(1.1642) = 12,000$ $f(1.2983) = 11,220$
 $f(1.3072) = 11,000$ $f(1.4142) = 0$

as in Figure 2.6 and define $u_i(x) = f(||e^i - x||)$, which is concave, strictly quasiconcave, and continuous. Consider a strategy profile in which individual 1 proposes a = (.8232, .1768, 0), 2 proposes b = (0, .8232, .1768), and 3 proposes c = (.1768, 0, .8232). Note that

$$u_1(a) = f(.25) = 14,894$$

 $u_1(b) = f(1.3072) = 11,000$
 $u_1(c) = f(1.1642) = 12,000$

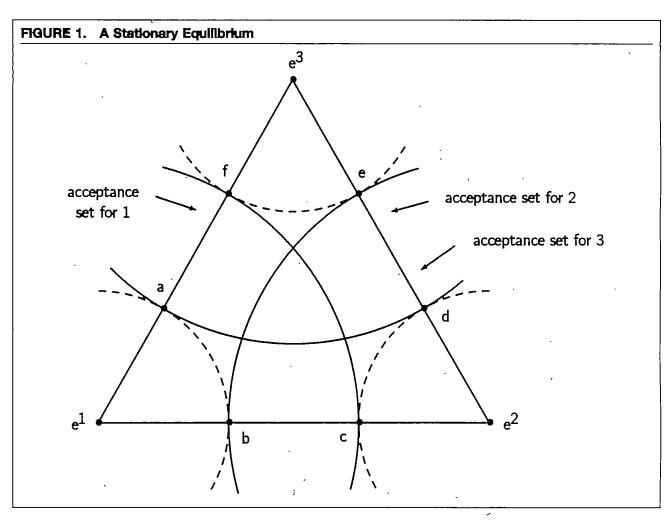
and that, by symmetry, the payoffs of 2 and 3 are merely permutations of those for 1. Individual 1's continuation value is

$$v_1 = \frac{1}{3} (14,894) + \frac{1}{3} (11,000) + \frac{1}{3} (12,000)$$
= 12,631,

and, by symmetry, $v_1=v_2=v_3$. Noting that $\delta_1v_1=12,000$, we define 1's acceptance set as

$$A_1 = \{x \in X | u_1(x) \ge 12,000\},\$$

⁶ The figure is meant to illustrate the definition of the function f. It is not drawn true to any linear scale.



with A_2 and A_3 defined symmetrically. Proposal a gives individual 2 a payoff of exactly 12,000, which is the minimum needed to acquire 2's approval. Thus, 1's strategy is a best response, as are those of 2 and 3, and we have a no-delay stationary equilibrium.

Now consider a strategy profile in which individual 1 proposes a' = (.8047, .1953, 0), 2 proposes b' = (0, .8047, .1953), and 3 proposes c' = (.1953, 0, .8047). Note that

$$u_1(a') = f(.2762) = 14,890$$

 $u_1(b') = f(1.2983) = 11,220$
 $u_1(c') = f(1.138) = 12,100$

and that, again, the payoffs of 2 and 3 are merely permutations of those for 1. Individual 1's continuation value is

$$v_1' = \frac{1}{3} (14,890) + \frac{1}{3} (11,220) + \frac{1}{3} (12,100)$$

= 12,737,

and $v_1' = v_2' = v_3'$. Noting that $\delta_1 v_1' = 12,100$, we define 1's acceptance set as

$$A_1' = \{x \in X | u_1(x) \ge 12,100\},\$$

with A'_2 and A'_3 defined symmetrically. Proposal a' gives 2 a payoff of exactly 12,100, which is the minimum

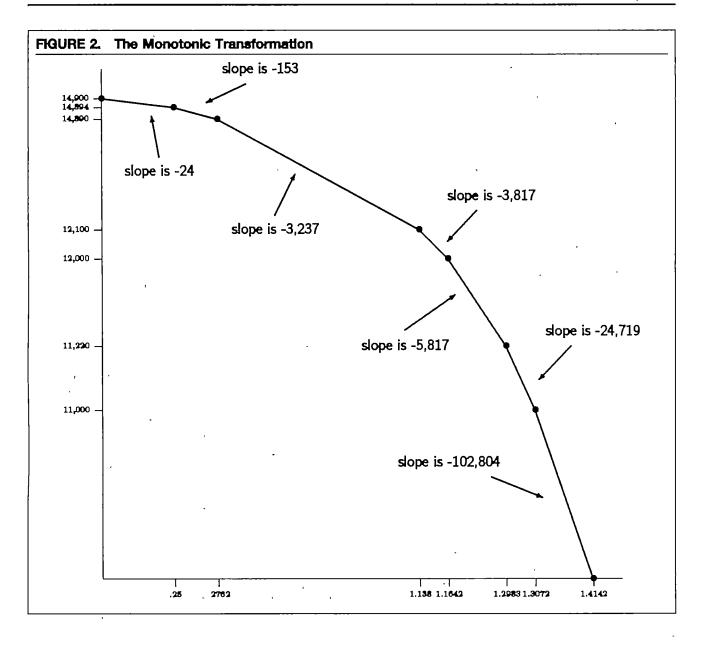
needed to acquire 2's approval. Thus, 1's strategy is a best response, as are those of 2 and 3, giving us a no-delay stationary equilibrium with continuation payoffs distinct from those just identified.

EXISTENCE AND CONTINUITY

In each of the examples in the previous section, a no-delay stationary equilibrium exists. Our first theorem establishes this as a general result.

THEOREM 1. (i) If $\delta_i < 1$ for all $i \in N$ or if LSWP holds, then there exists a no-delay stationary equilibrium. (ii) If $\delta_i < 1$ for all $i \in N$, then every stationary equilibrium is a no-delay equilibrium. (iii) If each u_i is strictly quasiconcave, then every no-delay stationary equilibrium has finite support.

Proof: For all $\pi \in [\mathfrak{P}(X)]^n$ and all $i \in N$, define $A_i(\pi) = \{x \in X | u_i(x) \geq \delta_i v_i(\pi)\}$. By concavity of u_i , by $u_i(x) \geq 0$ for all $x \in X$, and by $\delta_i \leq 1$, we have $x(\pi) \in A_i(\pi)$, and therefore $A_i(\pi)$ is nonempty. It is compact by the continuity of u_i , and it is convex by the concavity of u_i . For all $C \in \mathfrak{D}$, define $A_C(\pi) = \bigcap_{i \in C} A_i(\pi)$, also nonempty, compact, and convex; and define $A(\pi) = \bigcup_{C \in \mathfrak{D}} A_C(\pi)$, which is nonempty and compact but not necessarily convex. Theorem 7, in the Appendix, establishes that A is continuous as a corre-



spondence on $[\mathcal{P}(X)]^n$ if either $\delta_i < 1$ for all $i \in N$ or LSWP holds. To prove (i), define $M_i(\pi) = \arg M_i(\pi)$ $\max\{u_i(x)|x\in A(\pi)\}\$ for all $i\in N$. By the Theorem of the Maximum (Aliprantis and Border 1994, Theorem 14.30), $M_i: [\mathfrak{P}(X)]^n \longrightarrow X$ has nonempty and compact values and is upper hemicontinuous; but, it is not necessarily convex-valued, since $A(\pi)$ is not necessarily convex. Let $B_i(\pi) = \mathcal{P}(M_i(\pi))$ denote the set of mixtures of optimal proposals, which defines a nonempty, compact- and convex-valued, upper hemicontinuous correspondence (Aliprantis and Border 1994, Theorem 14.14). Define the correspondence B: $[\mathfrak{P}(X)]^n \longrightarrow [\mathfrak{P}(X)]^n$ by $B(\pi) = B(\pi) =$ $B_1(\pi) \times \ldots \times B_n(\pi)$; since $[\mathfrak{P}(X)]^n$ is compact and convex, by Glicksberg (1952) B has a fixed point, say, $\pi^* = (\pi_1^*, \ldots, \pi_n^*)$. Then π^* , together with acceptance sets $A_i^* = A_i(\pi^*)$, $i = 1, \ldots, n$, constitutes an equilibrium. To prove (ii), let σ^* be a stationary equilibrium. In step 7 of the proof of theorem 7, in the Appendix, we prove the existence of $x \in X$ such that,

for all $i \in N$, $u_i(\hat{x}) > \delta_i v_i(\pi^*)$. By weak dominance, $\hat{x} \in A^*$. Then

$$\sup\{u_i(y)|y\in A^*\}>\delta_i\nu_i(\pi^*),$$

and sequential rationality requires π_i^* put probability one on arg $\max\{u_i(y)|y\in A^*\}$ for all $i\in N$. In particular, $\pi_i^*(A^*)=1$ for all $i\in N$. To prove (iii), let σ^* be a no-delay stationary equilibrium. Each A_i^* is convex by strict quasiconcavity and weak dominance, and therefore each A_C^* is convex. Optimality of i's proposals requires that π_i^* put probability one on arg $\max\{u_i(y)|y\in A^*\}$, which can be rewritten

$$\arg\max\{\arg\max\{u_i(y)|y\in A_C^*\}|C\in\mathfrak{D}\}.$$

By convexity of A_C^* and strict quasiconcavity of u_i , arg $\max\{u_i(y)|y\in A^*\}$ is a singleton. Since $\mathfrak B$ is finite, the result is proved. Q.E.D.

A few remarks are in order. First, by definition, in no-delay equilibria the first proposal is always accepted, so there is no inefficiency due to delay. If some individuals make distinct proposals with positive probability, however, and if utility functions are strictly concave, then there will be ex ante inefficiency due to uncertainty.

Second, our presentation of the model and the proof of theorem 1 require only notational changes if we generalize the voting rule to a collection $\{\mathfrak{D}_i\}_{i\in N}$, where \mathfrak{D}_i is the nonempty, monotonic collection of coalition, that can pass a proposal from individual i. Let $A^i = \bigcup_{C \in \mathfrak{D}_i} A_C$ consist of the proposals i can pass. In the proof, we would define $A^i(\pi)$ accordingly, and we would define $M_i(\pi) = \arg\max\{u_i(x)|x \in A^i(\pi)\}$, restricting i's proposals to those the individual can pass; we again find a fixed point of the correspondence B, defined as in the proof of theorem 1, giving us a no-delay stationary equilibrium.

Third, the fixed points of B are all the no-delay stationary equilibrium proposals if each $\delta_i < 1$ or if LSWP holds. That is, in any such equilibrium, individual i's mixed proposal strategy must put probability one on the utility-maximizing alternatives in $A(\pi)$, defined in the proof of theorem 1. Indeed, consider a no-delay stationary equilibrium with proposals given by profile π° and acceptance sets $\{A_i^{\circ}\}$. Thus, $\pi_i^{\circ}(A^{\circ}) = 1$ for all $i \in N$. By weak dominance, $A_i^{\circ} \subseteq A_i(\pi^{\circ})$ for all $i \in N$, implying $A^{\circ} \subseteq A(\pi^{\circ})$. In equilibrium, individual j must put probability one on the utility-maximizing proposals in A° , but it is conceivable that these proposals are not utility maximizing in $A(\pi^{\circ})$. Let x' and C' satisfy $x' \in A_{C'}(\pi^{\circ})$, $C' \in \mathfrak{D}$, and $u_j(x') = \max\{u_j(x)|x \in A(\pi^{\circ})\}$.

In steps 8 and 9 of the proof of theorem 7, in the Appendix, we prove that one of two cases obtains. First, it may be that LSWP holds and $|A_{C'}(\pi^\circ)| = 1$. Second, it may be that $A_{C'}(\pi^\circ) \subseteq \overline{A_{C'}^I(\pi^\circ)}$, that is, for all $x \in A_{C'}(\pi^\circ)$, there is a sequence $\{x_k\}$ such that $x_k \to x$ and each x_k gives the members of C' strictly more than their discounted continuation value. In the second case, weak dominance implies $x_k \in A_{C'}^{\circ}$ for all k. By continuity of u_j , individual j can pass proposals with utility arbitrarily close to $u_i(x')$. Therefore, in equilibrium, π_i° must put probability one on arg max $\{u_i(x)|x\in$ $A^{\circ}(\pi^{\circ})$. In the first case, $|A_{C'}(\pi^{\circ})| = 1$, and it follows from concavity of the u_i that $A_{C'}(\pi^{\circ}) = \{x(\pi^{\circ})\}$ and that $x' = x(\pi^{\circ})$. If $|A_C(\pi^{\circ})| > 1$ for any $C \in \mathfrak{D}$, step 9 in the proof of theorem 7 establishes the existence of a sequence $\{x_k\}$ in $P_C(x(\pi^\circ))$ converging to x'. Since $P_C(x(\pi^\circ)) \subseteq A_C^*(\pi^\circ)$, weak dominance implies $x_k \in A_C^*$ for all k. Thus, individual j can again pass proposals with utility arbitrarily close to $u_i(x')$. If $|A_C(\pi^\circ)| = 1$ for all $C \in \mathfrak{D}$, then $A(\pi^{\circ}) = \{x(\pi^{\circ})\}$. Since A° is a nonempty subset of $A(\pi^{\circ})$, it follows that $A^{\circ} = \{x'\}$ and that individual j can pass x'.

Last, if $\delta_l = 1$ for some individuals, then there can exist stationary equilibria with delay. Let n = 2 and $\mathfrak{D} = \{N\}$; let $x \in X$ be Pareto optimal for individuals 1 and 2; let $y \in X$ be such that $u_2(x) > u_2(y)$; and let $\rho_1 = 1$. In each period, individual 1 proposes x with probability $\frac{1}{2}$ and y with probability $\frac{1}{2}$; and individual

2 rejects any $z \in X$ such that $u_2(z) < u_2(x)$. Then, since x is Pareto optimal, it is the best 1 can do given 2's acceptance set; and with probability one x is proposed in finite time, so i receives utility of $u_i(x)$.

Because the set A of socially acceptable proposals need not be convex, even in equilibrium (as in Figure 1), mixed strategies are required in the proof of theorem 1 to convexify the individuals' best responses. This is not a problem if we impose certain restrictions, on either the voting rule or the dimension of the set of alternatives, that deliver convexity of A. An implication of the next result is that the existence of pure strategy no-delay stationary equilibria, as in example 2, holds generally when X is one dimensional.

THEOREM 2. Assume either \mathfrak{D} is oligarchic or d=1. (i) If $\delta_i < 1$ for all $i \in N$ or if LSWP holds, then there exists a pure strategy no-delay stationary equilibrium. (ii) If each u_i is strictly quasiconcave, then every no-delay stationary equilibrium is in pure strategies.

Proof: If \mathfrak{D} is oligarchic, let $C' = \bigcap_{\mathfrak{D}} C$. Then $A(\pi)$, defined in the proof of theorem 1, is just $\bigcap_{i \in C'} A_i(\pi)$, a convex set. We claim $A(\pi)$ is also convex if d = 1. Let $\bar{x} = \max A(\pi)$ and $\bar{x} = \min A(\pi)$, which exist by compactness of X and continuity of the u_i , and let Cand \underline{C} satisfy $\bar{x} \in A_{\underline{C}}(\pi)$ and $\underline{x} \in A_{\underline{C}}(\pi)$. Clearly, $x(\pi) \in [\underline{x}, \overline{x}] = \operatorname{conv} A(\pi)$ and, by concavity, $x(\pi) \in$ $A_{\mathcal{C}}(\pi) \cap A_{\mathcal{C}}(\pi)$. Take any $y \in \operatorname{conv} A(\pi)$. Suppose $y \in [x, x(\pi)]$. Since $A_{\mathcal{L}}(\pi)$ is convex, by concavity, we have $y \in A_{\mathcal{C}}(\pi) \subseteq \overline{A}(\pi)$. A symmetric argument addresses the case $y \in [x(\pi), \bar{x}']$. To prove (i), we can simply revise the argument in the proof of theorem 1 (i) using only pure strategies. Since $A(\pi)$ is convex, it follows that $M_i(\pi)$, defined there, is convex-valued for all $i \in N$. Therefore, there exists a pure strategy no-delay stationary equilibrium. To prove (ii), recall the third remark after the proof of theorem 1: Under our assumptions, every no-delay stationary equilibrium σ^* is such that each π_i^* puts probability one on arg $\max\{u_i(x)|x\in A(\pi^*)\}$. Since $A(\pi^*)$ is convex and u_i is strictly quasiconcave, arg max $\{u_i(x)|x\in A(\pi^*)\}$ is a singleton, so π_i^* is a pure strategy.

The equilibrium identified in example 2 is clearly continuous in the common discount rate of the individuals. We now take up the continuity issue in more depth. The stationary equilibria of our model are parameterized by the individuals' recognition probabilities, $\rho = (\rho_1, \ldots, \rho_n) \in \Delta$, and their discount factors, $\delta = (\delta_1, \ldots, \delta_n) \in [0, 1]^n$. To these we add information about their utility functions. Let $\Lambda \subset \Re^k$ be a set parameterizing profiles of utility functions, so the individuals' preferences can be represented as $u_i(x) = u_i(x, \lambda)$ for some $\lambda \in \Lambda$. Assume that each u_i is jointly continuous in (x, λ) ; that, for all $\lambda \in \Lambda$, each $u_i(\cdot, \lambda)$ is concave and nonnegative; and that, for all $\lambda \in \Lambda$, there exists $x \in X$ such that, for all $i \in N$, $u_i(x, x)$ λ) > 0. As examples of such parameterizations, we could have $\Lambda \subseteq \Re^{nd}$, $\lambda = (\lambda_1, \dots, \lambda_n) \in \Lambda$, with each λ_i representing the ideal point of a quadratic utility function for individual i; or, more generally, λ_i

might be the matrix-defining weighted Euclidean distance utilities (Hinich and Munger 1997); alternatively, preferences could be represented in a general separable form (Caplin and Nalebuff 1991),

$$u_i(x, \lambda) = \sum_{h=1}^m \lambda_i^h t^h(x) + t^{m+1}(x),$$

where $t^h: X \to \Re$ is continuous and strictly concave for h = 1, 2, ..., m + 1.

For parameters ρ , δ , and λ , let $E(\rho, \delta, \lambda)$ denote the set of no-delay stationary equilibrium mixed proposal profiles given by the fixed points of the correspondence B, defined in the proof of theorem 1. As discussed above, if discount rates are all less than one or if LSWP holds, then $E(\rho, \delta, \lambda)$ consists of all no-delay stationary equilibrium profiles of proposals. We say that E is upper hemicontinuous at (ρ, δ, λ) if, for every open set $Y \subseteq [\mathcal{P}(X)]^n$ with $E(\rho, \delta, \lambda) \subseteq Y$, there exists an open set $Z \subseteq \Delta \times [0, 1]^n \times \Lambda$ with $(\rho, \delta, \lambda) \in Z$ such that, for all $(\rho', \delta', \lambda') \in Z$, $E(\rho', \delta', \lambda') \subseteq Y$. In words, "small" variations in (ρ, δ, λ) cannot lead the set of equilibrium outcomes to enlarge discontinuously.

THEOREM 3. If either $\delta_i < 1$ for all $i \in N$ or LSWP holds at λ , then E is upper hemicontinuous at (ρ, δ, λ) .

Proof: Given parameters ρ , δ , and λ satisfying the assumptions of the theorem and given a profile π of mixed stationary proposals, let θ denote the vector $(\rho, \delta, \lambda, \pi)$. Define

$$A_{\iota}(\theta) = \{x \in X | u_{\iota}(x, \lambda) \geq \delta_{\iota} v_{\iota}(\pi, \rho, \lambda)\},\$$

where $v_i(\pi, \rho, \lambda)$ is i's continuation value, defined as above but using $u_i(\cdot, \lambda)$. Theorem 7, in the Appendix, establishes that A is continuous as a correspondence at θ . Hence, by the Theorem of the Maximum,

$$M_i(\theta) = \arg\max\{u_i(x, \lambda)|x \in A(\theta)\}$$

is upper hemicontinuous at θ , and therefore so is $B_i(\theta) = \mathcal{P}(M_i(\theta))$. Since B_i has closed values and regular range as well, it has closed graph (Aliprantis and Border 1994, Theorem 14.11). Now, let $(\rho^m, \delta^m, \lambda^m) \to (\rho^o, \delta^o, \lambda^o)$, and take any sequence $\{\pi^m\}$ such that $\pi^m \in E(\rho^m, \delta^m, \lambda^m)$ for all m. Suppose $\pi^m \to \pi^o$. Since $\pi_i^m \in B_i(\theta^m)$ for all m and since B_i has closed graph, we see that $\pi_i^o \in B_i(\theta^o)$ for all $i \in N$. Therefore, $\pi^o \in E(\rho^o, \delta^o, \lambda^o)$, and we conclude that E has closed graph at θ . Since it has compact Hausdorff range space as well, it is upper hemicontinuous at θ (Aliprantis and Border 1994, Theorem 14.12). Q.E.D.

As applications, note that when $\delta_i = 0$ for all $i \in N$, the individuals will propose their ideal points when recognized; by theorem 3, therefore, when discount rates are all close to zero, each proposal will be close to the proposer's ideal point. Similarly, when $\rho_i = 1$, as in the model of Romer and Rosenthal (1978a, 1978b), we can easily solve for the equilibrium proposals for i; and when ρ_i is close to one, the individual's proposals will be close to that when ρ_i equals one, and hence the

FIGURE 3. Ideal Points in a Pentagon

contract curve for 1 and 2

expected outcome will be close to the original. Finally, suppose all equilibrium proposals actually coincide at the point x^* for some λ^* ; then, for all values of λ close to λ^* , all equilibrium proposals will be close to x^* .

CORE EQUIVALENCE

We now examine conditions under which the stationary equilibrium outcomes of our model coincide with the unbeaten, or "core," alternatives, as they do in example 2. We assume throughout that each u_i is strictly quasiconcave, so LSWP holds, and, from theorem 1 (iii), every stationary equilibrium has finite support; that each $\rho_i > 0$, which implies all individuals have some chance of being recognized; and that each $\delta_i = 1$, which implies all individuals are perfectly patient. In addition, we assume that \mathfrak{D} is proper. Define the *core* of the voting rule \mathfrak{D} , denoted \mathfrak{K} , as

$$\mathfrak{K} = \{x \in X | y \in P_C(x) \text{ for no } (C, y) \in \mathfrak{D} \times X\}.$$

The next example illustrates the concept of the core for the voting rule from example 1 for different values of the quota q.

Example 5. Let n = 5 and $X = [-1, 1]^2$, and assume each u_i is quadratic, with ideal points arranged in a pentagon, as in Figure 3. We suppose that D is a weighted q-rule with weights from example 1, so the ideal points of the large individuals, 1 and 2, are at nonadjacent corners of the pentagon. When $q \in (0,$.55], the core is empty: Since {1, 4, 5} is decisive, any core point would be in the triangle given by the three individuals' ideal points; similarly, it would be in the {2, 3, 4}-triangle, and it would be on the contract curve between 1's and 2's ideal points, but these sets have empty intersection. When $q \in (.55, .65]$, the core is now nonempty; because these points are Pareto optimal for 3, 4, and 5, the core is the thick portion of the contract curve for 1 and 2. When $q \in (.65, .8]$, the core increases to the entire contract curve for 1 and 2: For $q \in (.65, .7]$, 1 and 2 are oligarchs, and the claim is clear; for $q \in (.7, .8]$, any core point has to be in the $\{1, 2, 3\}$ -triangle and the $\{1, 2, 4\}$ -triangle, which

leaves only the contract curve. When $q \in (.8, .9]$, the core increases to the shaded triangle: These points are no longer upset by the coalition $\{1, 2, 3\}$, as this coalition is no longer decisive. Finally, when $q \in (.9, 1]$, the rule is equivalent to unanimity rule, and the core consists of the entire pentagon.

Our first lemma establishes one inclusion of the desired equivalence, namely, that every core alternative is the unique outcome of some stationary equilibrium. Furthermore, it establishes that a stationary equilibrium has a unique outcome only if that outcome is in the core. Indeed, it is clear from the proof of the lemma that the latter implication does not rely on strict quasiconcavity. In what follows we will denote pure strategy profiles of proposals by $p = (p_1, \ldots, p_n)$.

Lemma 1. $p_1^* = \cdots = p_n^* = x^*$ are no-delay stationary equilibrium proposals if and only if $x^* \in \mathcal{K}$.

Proof: Assume $p_1^* = \cdots = p_n^* = x^*$ are no-delay stationary equilibrium proposals. If $x^* \notin \mathcal{H}$, then there exists $C \in \mathfrak{D}$ and $y \in P_C(x^*)$, which implies $u_i(y) > u_i(x^*) = \delta_i v_i(\pi^*)$ for all $i \in C$. By weak dominance, $y \in A_i^*$ for all $i \in C$, and hence $y \in A^*$. Thus, when selected to propose, individual $i \in C$ gets a strictly higher payoff from proposing y than from proposing x^* , a contradiction. Now assume $x^* \in \mathcal{H}$. Setting $p_i^* = x^*$ and $A_i^* = R_i(x^*)$ for all $i \in N$, we claim that $((A_1^*, p_1^*), \ldots, (A_n^*, p_n^*))$ is a no-delay stationary equilibrium. The acceptance sets clearly satisfy weak dominance, so if this is not an equilibrium, then there must exist an individual with a better acceptable proposal. But if there exists $z \neq x^* \in X$ such that $z \in A_C^*$ for some $C \in \mathfrak{D}$, then $\frac{1}{2}x^* + \frac{1}{2}z \in P_C(x^*)$ by strict quasiconcavity, which contradicts $x^* \in \mathcal{H}$.

The next lemma reveals a connection between the structure of the set of no-delay stationary equilibrium proposals and the structure of the voting rule. Define the *Nakamura number* of \mathfrak{D} , denoted \mathcal{N} , as

$$\mathcal{N} = \min\{|\mathcal{G}||\mathcal{G}\subseteq\mathfrak{D} \text{ and } \bigcap_{c\in\mathcal{G}} C=\emptyset\}$$

if \mathfrak{D} is not collegial, and define $\mathcal{N}=\infty$ if it is. In words, when \mathfrak{D} is not collegial, the Nakamura number of \mathfrak{D} is the size of the smallest collection of decisive coalitions having empty intersection; and when \mathfrak{D} is collegial, it suffices here to assign it any infinite cardinality. If \mathfrak{D} is proper, as we now assume, then $\mathcal{N} \geq 3$; and if \mathfrak{D} is noncollegial, then $\mathcal{N} \leq n$. Given a subset $Y \subseteq X$ and $x \in Y$, say that x is an extreme point of Y if there do not exist distinct $y, z \in Y$ and $\alpha \in (0, 1)$ such that $x = \alpha y + (1 - \alpha)z$, that is, if it cannot be written as the convex combination of distinct elements of Y.

LEMMA 2. Let $\sigma^* = ((A_1^*, \pi_1^*), \ldots, (A_n^*, \pi_n^*))$ be a no-delay stationary equilibrium. If $S(\pi^*)$ has more than one extreme point, then the set of extreme points of $S(\pi^*)$ has cardinality at least \mathcal{N} .

Proof. Suppose that $S(\pi^*)$ has more than one extreme point, yet the cardinality of the set of extreme points is less than \mathcal{S} . From theorem 1, strict quasiconcavity implies that $S(\pi^*)$ is finite and, therefore, has a

finite number of extreme points. Denumerate the set of extreme points of $S(\pi^*)$ as x_1, x_2, \ldots, x_m , and define $C_h = \{i \in N | x_h \in A_i^*\}$, $h = 1, \ldots, m$. Since σ^* is a no-delay equilibrium, we have $C_h \in \mathfrak{D}$ for all h. Then, by definition of the Nakamura number, there is some $j \in \bigcap_{h=1}^m C_h$. That is, there is some j such that, for all $h, x_h \in A_j^*$. We claim that $u_j(x) = u_j(y)$ for all $x, y \in S(\pi^*)$. Otherwise,

$$\bar{u} = \max\{u_i(x)|x \in S(\pi^*)\} > \min\{u_i(x)|x \in S(\pi^*)\} = u,$$

and furthermore, by the Bauer Maximum Principle (Aliprantis and Border 1994, Theorem 4.104), μ is achieved at an extreme point of $S(\pi^*)$, say, x_k . But, since $\delta_j = 1$, this means that j's acceptance set violates weak dominance: $\rho_i > 0$ for all $i \in N$ implies $\mu < \infty$ $v_j(\pi^*)$, so in equilibrium we must have $x_k \notin A_i^*$. This contradiction establishes the claim. Next, note that, by concavity of the u_i , $u_i(x(\pi^*)) \ge v_i(\pi^*)$ for all $i \in N$. Since $x_1 \in A_{C_1}^*$, strict quasiconcavity and the weak dominance condition on $A_{C_1}^*$ imply that the convex hull of $\{x_1, x(\pi^*)\}$ is included in $A_{C_1}^*$. Then, because π_i^* is a best-response proposal strategy for j, continuity of u_i implies that there exists $x^* \in A^*$ such that $u_i(x^*) \ge$ $u_i(x(\pi^*))$. Now, by the Krein-Milman Theorem (Aliprantis and Border 1994, Theorem 4.103), we know that $x(\pi^*)$, since it is an element of the convex hull of $S(\pi^*)$, can be written as a convex combination of the extreme points of $S(\pi^*)$, which, we may suppose, places positive weight on the first h extreme points. Since $S(\pi^*)$ contains more than one element and $\rho_i >$ 0 for all $i \in N$, $x(\pi^*)$ cannot itself be an extreme point. Thus, h > 1. Then strict quasiconcavity and $u_j(x_1) = \cdots = u_j(x_k)$ yield $u_j(x(\pi^*)) > u_j(x_1)$. In equilibrium, j must propose alternatives with utility of at least $u_j(x^*) \ge u_j(x(\pi^*)) > u_j(x_1)$. But $\rho_j > 0$, so j's proposals are elements of $S(\pi^*)$, which contradicts the claim that j's utility is constant on $S(\pi^*)$.

Nakamura (1979) introduced the concept of the Nakamura number in his analysis of core nonemptiness in the absence of convexity restrictions, while Schofield (1984) and Strnad (1985) assume convexity of X and impose a weak convexity condition on preferences. They prove that the core is nonempty if $d \leq \mathcal{N} - 2$ and that, otherwise, there exist profiles of preferences for which the core is empty. As special cases, the core is nonempty if d=1 (since $\mathcal{N} \geq 3$) or if \mathfrak{D} is collegial (since $\mathcal{N} = \infty$). These two cases play analogous roles in the analysis of this section, as each provides sufficient structure for the existence of pure strategy stationary equilibria and equivalence with core outcomes.

If \mathfrak{B} is collegial, so that \mathcal{N} is infinite, then it is clear that the cardinality of the set of extreme points of $S(\pi^*)$, finite from theorem 1 (iii), cannot exceed \mathcal{N} . From lemma 2, therefore, $S(\pi^*)$ has at most one extreme point. Since it is finite and nonempty, it has exactly one, say, x^* . From lemma 1, therefore, we have $x^* \in \mathcal{H}$, which proves the following result.

THEOREM 4. If \mathfrak{D} is collegial, then all no-delay stationary equilibria are in pure strategies and are of the form $p_1^* = \cdots = p_n^* = x^*$ for some $x^* \in \mathfrak{K}$.

Thus, using lemma 1, we have full equivalence between the core of the underlying voting rule and the no-delay stationary equilibrium bargaining outcomes when individuals are perfectly patient and the voting rule $\mathfrak D$ is collegial. As an application, this yields an exact characterization of the no-delay stationary equilibria in example 5 for $q \in (.65, 1]$ when discount rates all equal one; for example, for $q \in (.8, .9]$ they coincide with the shaded triangle in Figure 3. Using theorem 3, we have a partial characterization as discount rates converge to one. Adding theorem 2 when $\mathfrak D$ is oligarchical, all no-delay stationary equilibria are in pure strategies for all discount rates, and pure strategy equilibrium proposals converge to the core as discount rates converge to one.

If d=1, then the set of extreme points of $S(\pi^*)$ has at most two elements. Since \mathfrak{D} is proper, however, $\mathcal{N} \geq 3$, and from lemma 2 we then know that $S(\pi^*)$ has at most one extreme point. Again, because $S(\pi^*)$ is finite and nonempty, we conclude that it has exactly one, say, x^* . From lemma 1, therefore, we have $x^* \in \mathcal{H}$, which proves the following result and generalizes the core equivalence seen in example 2.

THEOREM 5. If d=1, then all no-delay stationary equilibria are in pure strategies and are of the form $p_1^* = \cdots = p_n^* = x^*$ for some $x^* \in \mathcal{K}$.

Thus, when the alternative space has but a single dimension and individuals are perfectly patient, we again have full equivalence between the core of the underlying voting rule and the no-delay stationary equilibrium bargaining outcomes. When applied to majority-rule bargaining, a consequence of theorem 5 is that our model replicates the predictions of the Median Voter Theorem (Black 1958; Downs 1957) when individuals are perfectly patient. Using theorems 2 and 3, all no-delay stationary equilibria are in pure strategies for all discount rates, and pure strategy equilibrium proposals converge to the core (the medians under majority rule) as discount rates converge to one.

Now, suppose in addition that the core of D is actually a singleton. Then we also can conclude that, as the discount rates converge to one, all the equilibrium bargaining proposals collapse down to a single point in the space. Therefore, even if there are multiple equilibria when discount rates are less than one, the equilibrium proposals will be close to one another when those discount rates are close to one. A sufficient condition for the core (when nonempty) to be a singleton, given our assumption of strictly quasiconcave utilities, is that 20 is strong. Majority rule is an example of a strong rule when n is odd, and in that case the core consists of the (uniquely defined) median ideal point. Thus, in that case, the stationary equilibrium outcomes of our model will lie within arbitrarily small neighborhoods of the median as discount rates go to one.7

Given theorems 4 and 5 and the results of Schofield

(1984) and Strnad (1985), one might conjecture that core equivalence holds whenever $d \leq \mathcal{N} - 2$. To see that conjecture is false, consider the following example.

Example 6. Let n = 4, $X = [-1, 1]^2$, and $\rho_i = \frac{1}{4}$ for all $i \in N$, and assume each u_i is quadratic (ignoring the constant term). Let the ideal points of individuals 1 through 4 be given by (1, 0), (0, 1), (-1, 0), and (0, -1), respectively. Under majority rule there is a unique core point at (0, 0); however, consider the proposals $p_1 =$ $(\alpha, 0), p_2 = (0, \alpha), p_3 = (-\alpha, 0), \text{ and } p_4 = (0, -\alpha),$ where $\alpha \in (0, 1]$. Quadratic utility implies meanvariance analysis; given that the mean of these four (equally weighted) proposals is (0, 0) and the variance is α^2 , individual i's continuation value is equal to -1 α^2 . Consider the utility to individuals 2 and 4 from the proposal p_1 ; by the Pythagorean Theorem this is equal to $-1 - \alpha^2$, and so these two individuals are indifferent between accepting and rejecting p_1 , and thus accepting is a best response. A similar logic holds for proposals p_2 , p_3 , and p_4 , which thereby guarantees that each proposal will pass. Furthermore, given the continuation values, it is clear that these are utility-maximizing choices for the proposers. Thus, we have a stationary equilibrium away from the core (in fact, a continuum of them, parameterized by α).

The "problem" in the preceeding example is that the core is not associated with any one individual's ideal point and so is not offered as a proposal. To illustrate, suppose there is now a fifth individual with quadratic preferences and ideal point at (0, 0). The majority rule core remains at (0, 0), but now, as long as ρ_5 is strictly positive, this core point can have some influence on the individuals' behavior. In particular, the above proposals for individuals 1 through 4, along with $p_5 = (0, 0)$, do not constitute an equilibrium, even with ρ₅ arbitrarily small (and the other recognition probabilities equal to $(1 - \rho_5)/4$). To see this, note that the mean of the proposals has not changed; the variance has strictly decreased below α^2 , however, and by offering $p_1 = (\alpha,$ 0) individual 1 will acquire the votes of no other individual. The only equilibrium in example 6, so augmented, is at the core point, as in theorems 4 and 5.8 The next theorem subsumes this observation as a special case of a general result on quadratic preferences and strong voting rules.

THEOREM 6. If \mathfrak{D} is strong, if u_i is quadratic for all $i \in N$, and if $\mathfrak{H} = \{x^*\}$ with $x^* \in \text{int}X$, then all no-delay stationary equilibria are in pure strategies and are of the form $p_1^* = \cdots = p_n^* = x^*$.

Proof: Let $u_i(x) = -\|x - x^i\|^2$ for all $i \in N$, and let σ^* be a no-delay stationary equilibrium. The proof proceeds in a number of steps.

⁷ Analogous results employing theorem 4 are less interesting, as the only strong collegial rule turns out to be dictatorship.

⁸ Note that this example demonstrates a violation of lower hemicontinuity of E in ρ : When $\rho_5 = 0$, example 6 demonstrates a continuum of equilibrium outcomes, but the core is the only one that can be reached as the limit of equilibrium outcomes when all recognition probabilities are positive.

(1) \mathfrak{B} strong, u_i quadratic, and $\{x^*\} = \mathfrak{K}$ with $x \in \operatorname{int} X$ imply $x^* = x^i$ for some $i \in N$. If not, $\nabla u_i(x^*) \neq 0$ for all $i \in N$, so let H be a hyperplane, with normal p, through zero containing no gradient vector. Since \mathfrak{B} is strong, either

$$\{i \in N | \nabla u_i(x^*) \cdot p > 0\}$$
 or $\{i \in N | \nabla u_i(x^*) \cdot p < 0\}$

is decisive. Suppose the former, without loss of generality. Since $x^* \in \text{int} X$, there exists $\varepsilon > 0$ such that $x^* + \varepsilon p \in X$ and for all members i, $u_i(x^* + \varepsilon p) > u_i(x^*)$. Thus, $x^* \notin \mathcal{H}$, a contradiction.

(2) $\{i \in N | u_i(x^*) \ge u_i(x(\pi^*))\} \in \mathfrak{D}$. If not, since \mathfrak{D} is strong, its complement is decisive, implying $x^* \notin \mathfrak{K}$, a contradiction.

(3) $x^* \in A^*$. If not, $C = \{i \in N | x^* \in A_i^*\} \notin \mathfrak{D}$, and by \mathfrak{D} strong $N \setminus C \in \mathfrak{D}$. By weak dominance, $u_i(x^*) \leq v_i(\pi^*)$ for all $i \in N \setminus C$; then concavity yields $u_i(x^*) \leq u_i(x(\pi^*))$ for all $i \in N \setminus C$. If $|S(\pi)| > 1$, these inequalities hold strictly, but then $x \notin \mathcal{H}$, a contradiction. The remaining case is $|S(\pi)| = \{y\}$ for some $y \in X \setminus \{x\}$, but then $\frac{1}{2}x^* + \frac{1}{2}y \in P_{N \setminus C}(x^*)$ and $x \notin \mathcal{H}$, a contradiction.

(4) Letting individual 1 be such that $x^* = x^1$, $S(\pi_1^*) = \{x^*\}$. This follows directly from step 3 and the definition of stationary equilibrium

the definition of stationary equilibrium.

Let $\underline{x} \in \arg\min\{u_1(x)|x \in S(\pi^*)\}$, which is well-defined by theorem 1 (iii). Suppose, in order to deduce a contradiction, that $\underline{x} \neq x^*$, that is, $|S(\pi^*)| > 1$

- (5) $u_1(\underline{x}) < v_1(\pi^*)$. This follows from $p_1^* = x^*$ and $o_1 > 0$.
 - (6) x^* is a total median. In particular, the coalition

$$D = \{i \in N | (x^{i} - x^{*}) \cdot (x(\pi^{*}) - \underline{x}) \ge 0\}$$

is decisive. Otherwise, by \mathfrak{D} strong, $ND \in \mathfrak{D}$; but because $x^* \in \text{int} X$ and the u_i are quadratic, we could then take $\varepsilon > 0$ small enough that

$$x^* + \varepsilon(x - x(\pi^*)) \in P_{ND}(x^*)$$

and $x^* \notin \mathcal{H}$, a contradiction.

(7) For all $i \in D$,

$$||x^{t} - x(\pi^{*})||^{2} - ||x^{t} - \underline{x}||^{2}$$

$$= -2x^{t} \cdot (x(\pi^{*}) - \underline{x}) + x(\pi^{*}) \cdot x(\pi^{*}) - \underline{x} \cdot \underline{x}$$

$$\leq -2x^{*} \cdot (x(\pi^{*}) - \underline{x}) + x(\pi^{*}) \cdot x(\pi^{*}) - \underline{x} \cdot \underline{x}$$

$$= ||x^{*} - x(\pi^{*})||^{2} - ||x^{*} - \underline{x}||^{2}.$$

(8) For all $i \in D$, $u_i(\underline{x}) < v_i(\pi^*)$. Quadratic utility implies mean-variance analysis. Letting v^* denote the variance of π^* , we have

$$v_{i}(\pi^{*}) - u_{i}(\underline{x}) = u_{i}(x(\pi^{*})) - v^{*} - u_{i}(\underline{x})$$

$$= -\|x^{i} - x(\pi^{*})\|^{2} + \|x^{i} - \underline{x}\|^{2} - v^{*}$$

$$\geq -\|x^{*} - x(\pi^{*})\|^{2} + \|x^{*} - \underline{x}\|^{2} - v^{*}$$

$$= u_{1}(x(\pi^{*})) - v^{*} - u_{1}(\underline{x})$$

$$= v_{1}(\pi^{*}) - u_{1}(\underline{x}) > 0,$$

where the first inequality follows from step 7 and the second from step 5.

(9) $\underline{x} \notin A^*$. Given step 8, weak dominance implies that $\underline{x} \notin A_i^*$ for all $i \in D$. Since $D \in \mathfrak{D}$ by step 6, and since \mathfrak{D} is proper and monotonic, we have $\{i \in N | \underline{x} \in A_i^*\} \notin \mathfrak{D}$.

Because \underline{x} is, by definition, proposed with positive probability, step 9 contradicts our assumption that σ^* is a no-delay equilibrium. Thus, we conclude that $\underline{x} = x^*$. (10) $S(\pi^*) = \{x^*\}$. This follows from $\underline{x} = x^*$ and uniqueness of individual 1's ideal point. Q.E.D.

Thus, under majority rule with n odd, quadratic utilities, and perfect patience, if the individuals' ideal points satisfy the "Plott conditions" (Plott 1967), then the majority rule core consists of a unique point, which coincides with the unique no-delay stationary equilibrium outcome of bargaining. Furthermore, by theorem 3, if we perturb the individuals' ideal points, then the equilibrium proposals stay near to one another as well as to their original position. Just this sort of continuity has been observed experimentally. Fiorina and Plott (1978) ran five-person, two-dimensional experiments in which utilities were such that the Plott conditions were sometimes satisfied and sometimes violated (and the core empty) but close to being satisfied. The experimental outcomes tended to cluster around the core in the former cases and did not stray very far in the latter.

The role of quadratic utility functions in theorem 6 is to allow us to use mean-variance analysis in the proof. To see that the theorem does not generalize to all utility functions based on Euclidean distance, consider one last example.

Example 7. Let n = 5, $X = [-1, 1]^2$, and $\rho_t = \frac{1}{5}$ for all $i \in N$, and assume ideal points x^i are at (1, 0), (0, 1), (-1, 0), (0, -1), and (0, 0). Rather than quadratic utilities, assume $u_t(x) = -\|x^t - x\|^4$ (ignoring the constant term), so (0, 0) is still the unique core point. Given the profile of proposal strategies in which all individuals propose their ideal points, the continuation values of the first four individuals are -5, and the continuation value of individuals 2 and 4 from the proposal $p_1 = (1, 0)$ is -4, which is greater than their continuation values, and accepting is a best response, so 1's proposal passes. A similar logic holds for the remaining proposals. Thus, we have a stationary equilibrium in which the core occurs only with probability one-fifth.

DISCUSSION

We have analyzed a noncooperative model of multiperson bargaining over a multidimensional alternative space. We have proved the existence of stationary equilibria and have related equilibrium outcomes to the core outcomes of the underlying voting rule. Our bargaining model replicates the predictions of the core under certain conditions but avoids the principle "negative" result of the social choice approach, namely, that the core is typically empty in multidimensional collective choice problems. Small perturbations of parameters that can lead the core suddenly to become empty will not affect the existence of stationary equilibria in our model. Furthermore, we establish a robustness property of equilibrium predictions that ensures the equilibrium outcomes of our model will respond to such perturbations in a continuous way, namely, that the set of outcomes does not "blow up" upon perturbation of parameters.

The model implicitly assumes that each individual's utility from the (unseen) "status quo" alternative is zero, and alternatives in X give the individuals nonnegative utility; equivalently, everyone weakly prefers all the alternatives in X to the status quo. If we drop this assumption, the proof of theorem 1 (i) no longer goes through, but we regain existence of equilibrium if we (1) explicitly specify the status quo, q, as an alternative in X; (2) impose the requirement that the discount rates of the individuals are identical, that is, there exists some $\delta \in [0, 1]$ such that, for all $i \in N$, $\delta_i = \delta$; and (3) define individual *i*'s payoff from outcome (x, t) as $(1 - \delta^{t-1})u_i(q) + \delta^{t-1}u_i(x)$. If we then assume $u_i(q) = 0$ for all $i \in N$ (now just a normalization), then individual i's payoff from outcome (x, t) would be defined just as before. As a special case, we obtain the model of Romer and Rosenthal (1978a, 1978b), where d = 1, majority rule, $\rho_i = 1$ for some $i \in N$ (the "agenda setter"), and $\delta_1 = \cdots = \delta_n = 0$. The core equivalence results are unaffected by the introduction of the status quo.

Once the status quo is explicitly brought into the model, an obvious but difficult extension would be to allow for bargaining to continue after passage of a proposal in period t, that proposal being the new status quo for period t + 1, and so on. Baron (1996) characterizes the stationary equilibria of such a model when the set of alternatives is one dimensional, and Baron and Herron (1998) investigate a finite-horizon version of the model with three individuals, twodimensional policy space, and quadratic utility functions. With the addition of this state variable, stationary strategies must be conditional on the status quo: Proposal strategies would be mappings from X (possible status quo outcomes) to probability measures on X, and acceptance strategies would be correspondences from X to X. It may be possible to modify the techniques of this article to the analysis of this complex but realistic setting, but that is a matter for future research.

We have taken recognition probabilities as exogenously given, without offering an explanation of their possible origins. One interpretation, appropriate when bargaining takes place within a parliamentary or legislative body, is that the individuals represent parties and that the recognition probability of a party represents the number of seats it holds, proxying for the party's influence in lawmaking. An interesting extension would

be to model explicitly the voters who elect the members of parliament, providing an alternative to Austen-Smith and Banks (1988), Coate (1997), Schofield (1998), Schofield and Sened (1998), and Baron and Diermeier (1998).

While we have investigated a model of bargaining, other types of institutional structures have been modeled noncooperatively to obtain equilibrium existence in the absence of core outcomes. These approaches include sophisticated voting under various types of agendas (Banks 1985; Farquharson 1969; McKelvey and Niemi 1978; Miller 1977, 1980; Shepsle and Weingast 1984); mixed strategy equilibria in two-party spatial competition games (Banks, Duggan, and Le Breton 1998; Kramer 1978; Laffond, Laslier, and Le Breton 1993; Laslier and Picard 1998; McKelvey 1986); and the "structure-induced equilibrium" model of committees (Denzau and Mackay 1981; Shepsle 1979), in which each committee is assigned to a dimension of the alternative space and is given sole jurisdiction over the location of the policy along that dimension. In contrast to the latter approach, our bargaining model may be viewed as a model of legislatures in the absence of a formal committee system, as one that provides a useful benchmark against which a general model of institutional choice can be constructed, and as one that offers a prediction in situations in which no such institutional arrangements are to be found. An important question is then the extent to which our model gives different predictions for the same underlying preferences and how well these predictions square with empirical observations.

An alternative within the cooperative paradigm is to consider solutions other than the core. Numerous alternative solution concepts have been suggested, such as the von Neumann-Morgenstern solution (von Neumann and Morgenstern 1944), the Nash bargaining solution (Nash 1950), the bargaining set (Aumann and Maschler 1964), the competitive solution (McKelvey, Ordeshook, and Winer 1978), the uncovered set (Banks, Duggan, and Le Breton 1999; Cox 1987; McKelvey 1986; Miller 1980), and the heart (Schofield 1996, 1998). A question we have not addressed here is the relationship between the equilibrium outcomes of our noncooperative bargaining model and the above solutions, but our core equivalence results do inform us that assuming unanimity rule and setting discount rates equal to one in our model does not yield the Nash bargaining solution (or any other selection from the Pareto optimals): By lemma 1, every Pareto-optimal alternative could be supported as a stationary equilibrium outcome for this specification of the model. It may be that, as in Binmore's (1987) model of two-person bargaining, equilibrium outcomes do converge to the Nash solution as discount rates increase to one, which would give us a robust selection from the limit equilibrium outcomes.

APPENDIX

As discussed preceeding theorem 3, let $\rho=(\rho_1,\ldots,\rho_n)\in\Delta$ and $\delta=(\delta_1,\ldots,\delta_n)\in[0,1]^n$ be the profiles of

It need no longer be the case that $u_i(x(\pi)) \geq \delta_i v_i(\pi)$, so that $x(\pi)$ need not be an element of $A_i(\pi)$, which invalidates our argument that $A_C(\pi)$ is nonempty. When all individuals have discount rate δ_i , however, concavity of u_i yields $(1-\delta)q + \delta x(\pi) \in A_i(\pi)$ for all $i \in N$.

individuals' recognition probabilities and discount rates. Index profiles of utility functions by $\lambda \in \Lambda \subseteq \Re^k$, and assume each $u_i(\cdot, \lambda)$ is concave and nonnegative. Moreover, assume there is some $x \in X$ such that, for all $j \in N$, $u_j(x, \lambda) > 0$. Also assume each u_i is jointly continuous. Let $\Theta = \Delta \times [0, 1]^n \times \Lambda \times [\mathcal{P}(X)]^n$, let $\theta = (\rho, \delta, \lambda, \pi)$ as in the proof of theorem 3, and define

$$r_i(\theta) = \delta_i v_i(\pi, \rho, \lambda),$$

where v_i is defined as above but using $u_i(\cdot, \lambda)$. Define the correspondence $A_i:\Theta\longrightarrow X$ by

$$A_i(\theta) = \{x \in X | u_i(x, \lambda) \ge r_i(\theta)\},\$$

and let $A_C(\theta) = \bigcap_{i \in C} A_i(\theta)$ and $A(\theta) = \bigcup_{C \in \mathfrak{D}} A_C(\theta)$. We can now state the main result of this section.

THEOREM 7. A has nonempty, compact values. If $\delta_i < 1$ for all $i \in N$ at θ , or if LSWP holds at θ , then A is continuous at θ.

Proof: The proof proceeds in a series of steps.

(1) A_C has nonempty values. By concavity and nonnegativity of $u_i(\cdot, \lambda)$ and $\delta_i \leq 1$, it follows that $u_i(x(\pi), \lambda) \geq$ $r_i(\theta)$; therefore, $x(\pi) \in A_C(\theta)$.

(2) A_C is compact-valued. This follows from the continuity of $u_1(\cdot, \lambda)$, the compactness of X, and the fact that compactness of the $A_i(\theta)$ sets is preserved by intersections.

- (3) A_i is upper hemicontinuous. Take any θ and any open $V \subset X$ such that $A_{i}(\theta) \subseteq V$. Suppose there is a sequence $\{\theta_m\}$ converging to θ such that, for all $m, A_i(\theta_m) \setminus V \neq \emptyset$. For all m, let $x_m \in A_i(\theta_m) \setminus V$; then $\{x_m\}$ lies in $X \cap V$, which is compact since V is open and X is compact. Thus, without loss of generality we can assume $\{x_m\}$ converges to some $x \in$ $X \cap V^{c}$. Since $u_{i} - r_{i}$ is jointly continuous (see Billingsley 1968, Theorem 5.5), $(\theta_m, x_m) \to (\theta, x)$ implies $[u_i(x_m, \lambda_m) - r_i(\theta_m)] \to [u_i(x, \lambda) - r_i(\theta)]$. Then $x_m \in A_i(\theta_m)$ implies $u_i(x_m, \lambda_m) - r_i(\theta_m) \ge 0$ for all m, so $u_i(x, \lambda) - r_i(\theta_m) \ge 0$. $r_i(\theta) \geq 0$. But then $x \in A_i(\theta) \subseteq V$, which contradicts the assumption that $x \in \mathcal{V}^c$.
- (4) A_C is upper hemicontinuous. Upper hemicontinuity follows from theorem 14.24 in Aliprantis and Border (1994), which states that the intersection of compact-valued, upper hemicontinuous correspondences is upper hemicontinuous.

Now define the correspondence $A_i^x:\Theta\longrightarrow X$ by

$$A_i^s(\theta) = \{x \in X | u_i(x, \lambda) - r_i(\theta) > 0\},$$

- and let $A_{c}^{r}(\theta) = \bigcap_{i \in C} A_{i}^{r}(\theta)$.

 (5) A_{i}^{r} has open graph. Take $(\theta, y) \in GrA_{i}^{r}$, that is, $u_{i}(y, y) \in GrA_{i}^{r}$, that is, $u_{i}(y, y) \in GrA_{i}^{r}$, that is, $u_{i}(y, y) \in GrA_{i}^{r}$. $(\lambda) - r_i(\theta) > 0$. Since $u - r_i$ is continuous on $\Theta \times X$, there exists an open set $V \subseteq \Theta \times X$ such that $(\theta, y) \in V$ and $u_i(y', y') \in V$ λ') $-r_i(\theta') > 0$ for all $(\theta', y') \in V$, which implies GrA_i^* is open.
- (6) A_C^s has open graph. This follows from step 5, since the finite intersection of open sets is open.
- (7) If $\delta_i < 1$ for all $i \in N$, then $A_C^*(\theta) \neq \emptyset$. Let $x \in X$ be such that $u_i(x, \lambda) > 0$ for all $i \in N$. If $r_i(\theta) = 0$, then, since $u_i(\cdot, \lambda)$ is concave and nonnegative, $u_i(\alpha x + (1 - \alpha)x(\pi))$, $\lambda > 0 = r_i(\theta)$ for all $\alpha \in (0, 1)$. If $r_i(\theta) > 0$, then, since $u_i(\theta) > 0$, λ) is concave and nonnegative and since $\delta_i < 1$, $u_i(x(\pi))$, $(\lambda) > \delta_i \nu_i(\pi, \lambda) = r_i(\theta)$. Thus, by continuity, $u_i(\alpha x + (1 - \epsilon))$ $\alpha(x(\pi), \lambda) > r(\theta)$ for $\alpha > 0$ low enough. Taking $\alpha > 0$ low enough, therefore, $\alpha(x) + (1 - \alpha)x(\pi) \in A_C^{\bullet}(\theta)$.
- (8) If $\delta_i < 1$ for all $i \in N$, then A_C is lower hemicontinuous at θ . Take any $x \in A_C(\theta)$; from step 7 there exists $y \in A_C(\theta)$; by concavity of the $u_1(\cdot, \lambda)$, $1/m y + (1 - 1/m)x \in$ $A_C(\theta)$ for all nonnegative integers m. Letting m go to infinity, we have $x \in A_C(\theta)$. With step 6 it follows that, at θ , A_C differs from an open subcorrespondence only at points of

closure. Therefore, following lemma 14.21 in Aliprantis and Border (1994), A_C is lower hemicontinuous at θ .

(9) If LSWP holds at θ , then either $|A_C(\theta)| = 1$ or $A_C(\theta)$ $\subseteq A_C^*(\theta)$. Suppose $|A_C(\theta)| > 1$, take any $x \in A_C(\theta)$, and note that, as in step 1, $x(\pi) \in A_i(\theta)$ for all $i \in C$. Partition C into two sets:

$$I = \{i \in C | u_i(x(\pi), \lambda) = r_i(\theta)\},\$$

$$J = \{i \in C | u_i(x(\pi), \lambda) > r_i(\theta)\}.$$

Note that, for all $i \in J$, $x(\pi) \in A_i^*(\theta)$, an open set. Thus, we can find an open set V such that $x(\pi) \in V \subseteq A_I^{\mathfrak{p}}(\theta)$. Note also that

$$|R_I(x(\pi))| = |A_I(\theta)| \ge |A_C(\theta)| > 1,$$

so, by LSWP, there is a sequence $\{x_k\}$ in $P_I(x(\pi))$ converging to $x(\pi)$. Picking k high enough, therefore, we have

$$x_k \in V \cap P_I(x(\pi)) \subseteq P_C(x(\pi)) \subseteq A_C^s(\theta).$$

By concavity, $1/m x_k + (1 - 1/m)x \in A_C^{\sigma}(\theta)$ for all nonnegative integers m. Letting m go to infinity, we have $x \in$ $A_C(\theta)$.

(10) If LSWP holds at θ , then A_C is lower hemicontinuous at θ . From step 9 there are two cases to consider. If $|A_C(\theta)|$ $= \{x'\}$ for some $x' \in X$, then, for every open set $V \subseteq X$, V $\cap \{x'\} \neq \emptyset$ implies $\{x'\} \subseteq V$, in which case lower hemicontinuity at $\hat{\theta}$ follows from steps 1 and 4. If $A_C(\theta) \subseteq$

 $A_C^{\bullet}(\theta)$, then, as in step 8, A_C is lower hemicontinuous at θ . We can now complete the proof. That A has nonempty, compact values follows from steps 1 and 2 and the arbitrary choice of C. If $\delta_i < 1$ for all $i \in N$, then continuity of A_C at θ follows from steps 4 and 8; continuity of A at θ then follows from theorem 14.26 in Aliprantis and Border (1994), which states that the finite union of continuous correspondences is continuous. If LSWP holds at θ , continuity of A_C at θ follows from steps 4 and 10, and continuity of A at θ again follows.

Q.E.D.

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The Supreme Court and Local Public Opinion

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ost research suggests that the mass public knows very little about the Supreme Court and, consequently, that decisions do not affect attitudes toward the Court. I argue that where there is sufficient access to information about Court cases and when the issues are perceived as important, people pay attention and use this information in their evaluation of the Court. The research is based on a series of two-wave panel studies that examine the effect of Supreme Court cases in the local communities where the controversies began. The results show that a substantial number of residents heard about the Court's decision and subsequently changed their evaluation of the Supreme Court, especially those who live in the immediate community. The results suggest that we need to consider other circumstances in which people hear about and care about Supreme Court decisions.

Research on the relationship between specific Supreme Court decisions and public support for the Court has been frustrated by the apparent public ignorance of all but the most controversial and visible cases (see Caldeira 1991). In the standard account, citizens are portrayed as quite willing to offer an opinion about the institution, but they do so without knowledge of many individual decisions. Thus, many scholars conclude that support for the Court rests upon more enduring attitudes about the legitimacy of the Court in the system of government rather than on agreement or disagreement with specific decisions.

Although most research suggests that the majority of Court decisions go unnoticed, the possibility that these decisions influence attitudes toward the institution is not without some support in the literature. The connection has been established in experimental research (Mondak 1991, 1992; Segal 1995) but has not been very well documented outside the laboratory. The reason is straightforward: If Court decisions are not common knowledge, by definition they can have no effect. One major obstacle is that most national surveys do a poor job of identifying conditions in which people are motivated to learn about specific Court decisions and in which they have sufficient access to information about them (but see Franklin and Kosaki 1995; Franklin, Kosaki, and Kritzer 1993; Hoekstra and Segal 1996). Consequently, we may be underestimating the importance of citizens' reactions as an element of support for the Court. One instance in which interest and access to information are likely to be high is the local communities where a controversy began. People should be more interested in cases that involve members of their own community than in cases that involve individuals or groups from somewhere else (Boninger, Berent, and

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Krosnick 1995; Krosnick, Boninger, and Chuang 1993). Also, since the local media tend to report on issues of local concern (Graber 1997), residents of the community should have the opportunity to hear about these decisions.

I describe the results of a series of two-wave panel studies designed to assess individual awareness of Court decisions, individual interest in those decisions, and the effect of those decisions on individual attitudes toward the Court. I focus on four Supreme Court cases and the attitudes of citizens in the communities where the controversies began. The results should be generalizable to other communities in which there is an interest in an issue and access to information about a relevant case. Religious groups, for example, are likely to follow cases that concern religious freedom or abortion (see Franklin and Kosaki 1989), as are police officers regarding criminal procedure cases (see Berkson 1978). The ability to generalize is also justified by recent research that shows national levels of awareness are higher than previously recognized (Franklin and Kosaki 1995; Franklin, Kosaki, and Kritzer 1993).

SUPPORT FOR THE SUPREME COURT

Most accounts of public support for the Supreme Court refer to the Court's legitimacy as an institution of government. Explanations for the source of this legitimacy are diverse. Early research emphasized that the Court benefits from socialization that leads citizens to perceive it is a wise and benevolent institution and as the last bastion of constitutional freedoms (Adamany and Grossman 1973; Casey 1974; Easton 1965; Easton and Dennis 1969; Jaros and Roper 1980). More recent research focuses on whether support comes from relatively enduring attitudes (i.e., diffuse support) or stems from evaluations of actions taken by the Court (i.e., specific support) (Caldeira 1986; Caldeira and Gibson 1992; see also Jaros and Roper 1980; Murphy and Tanenhaus 1968; Tanenhaus and Murphy 1981). Sim-

¹ Caldeira and Gibson (1992) have a focus different from mine. Primarily, their aim was to disentangle two different types of support for the Court (specific and diffuse) that have been poorly measured in previous research. Nevertheless, their finding of the relative absence of specific information in long-term evaluations of the Court

ilar to the notion of the issue voter in electoral research, specific support refers to a "set of attitudes toward an object based upon the fulfillment of demands for particular policies or actions" (Caldeira 1986, 322). In contrast, similar to the voter who relies on party cues, diffuse support refers to "generalized and firm attachments" or a "reservoir of favorable attitudes" (p. 322). Diffuse support is more enduring and lacks much connection between outcomes and attitudes.

The prevailing consensus is that support for the Court is more diffuse than specific, especially among the mass public (Caldeira and Gibson 1992; Mondak 1992). Yet, the research does not entirely preclude the effect of decisions. Rather, there is assumed to be some dynamic component to the process, such that especially notable or activist decisions may factor into the equation. According to Caldeira and Gibson (1992, 652), "to the extent that the Supreme Court openly embraces judicial activism, the citizenry may judge the institution in the same light as other political institutions: policy agreement and disagreement will significantly affect support for the institution." Mondak and Smithey (1997) echo this idea. They present a primarily theoretical dynamic model of support for the Court and suggest that individual attitudes generally tend to be positive, but they may change as a result of controversial and unpopular decisions.

Finally, others argue that Court procedures—such as the secrecy of deliberations, infrequent media attention, and perceptions of being removed from partisan political battles both within the Court and between the Court and other branches of government—lend it greater support in the public eye (Hibbing and Theiss-Morse 1995). Indeed, aggregate levels of support for the Court are consistently higher than for Congress and the executive branch, and they appear to be relatively more stable as well (Hibbing and Theiss-Morse 1995; Marshall 1989; Mondak and Smithey 1997). Although the data on aggregate public support for the Court are consistent with this process argument, they also may very well be consistent with citizens hearing about certain cases and changing their perception of the Court in response. As Mondak and Smithey (1997, 1139) note, "individual level change does not necessarily preclude aggregate level stability." The evidence in support of the process argument is based largely on aggregate measures of support for the Court, with little attention paid to variation at the individual level.

KNOWLEDGE OF SUPREME COURT DECISIONS

The conclusion that the public is uninformed and uninterested in Court activities is usually drawn from

among the mass public is relevant to this research. The way they measure support for the Court also differs from my method. I am interested in how specific decisions affect individuals' attitudes, whereas Caldeira and Gibson are more interested in measuring commitment to the very institution, which theoretically should be completely separate from its decisions.

national public opinion surveys, typically of the voting behavior of the American electorate. These surveys may be well suited to the examination of voting behavior, but they usually lack data appropriate for the study of the Supreme Court and its activities. Even when good measures are found, they are rarely repeated, the questions often do not adequately reflect the subtlety of the issues involved in the cases, the time lags between surveys and decisions vary considerably, and they are usually conducted around the timing of elections rather than the Court's calendar (Caldeira 1991). Finally, these studies rarely do very much to identify cases about which we can reasonably expect citizens to hear. Recent research by Franklin and his colleagues demonstrates this point. Their data, collected with the Court's calendar in mind, reveal more widespread awareness of Court decisions than commonly thought (Franklin and Kosaki 1995; Franklin, Kosaki, Kritzer 1993).

In short, experimental research holds the door open on the question, but there has been little evidence to document the Court's effect on public opinion outside the laboratory environment (Mondak 1991, 1992; Segal 1995). The main obstacle appears to be the lack of appropriate data to test this relationship and the inability to identify conditions in which individuals have much knowledge of specific Court decisions. With these ideas in mind, I turn to my hypotheses about the effect of Supreme Court decisions in and around the local communities in which cases originated.

HYPOTHESES

Awareness and Perceptions of Importance

Local media are likely to report on cases ultimately decided by the Supreme Court. These cases are newsworthy because they involve local officials, groups, individuals, and issues that may have been in dispute for some time. The consensus of psychological and political science research on political attitudes is that the more important an issue is to an individual, the greater is the motivation to pay attention to and spend time thinking about the issue and its political implications (Fiske and Taylor 1992; Krosnick, Boninger, Chuang 1993; Petty and Cacioppo 1986). The perceived importance need not come from some actual material interest being jeopardized. In fact, social psychologists emphasize the subjective sense of importance (see Boninger, Krosnick, and Berent 1995). Other sources of importance can include such considerations as identifying with the people involved (e.g., members or groups in the local community) (Boninger, Krosnick and Berent 1995). Although people in both the immediate and surrounding communities in which controversies originate should have roughly equal access to information about the Court, those from the immediate vicinity should find the case especially compelling because they identify with and recognize the groups involved. This leads to my first hypotheses:

Awareness

Hypothesis 1a. Residents in and around the immediate community in which a case originates should exhibit a higher level of awareness of the Court's decision than is usually found in national samples.

HYPOTHESIS 1b. Those within the immediate community should show a higher level of awareness than those who reside in the surrounding areas.

Importance

HYPOTHESIS 2. Among those who hear of the Court's decision, those from the immediate community should find the case more important than those from the surrounding areas.

Evaluation of the Court

If Court activities do not affect how people feel about the Court, evaluations should change according to how the individual initially feels about an issue. This leads to my third hypothesis.

HYPOTHESIS 3. Awareness of the Court's decision should affect evaluations of the Supreme Court. Those who initially approve/disapprove of the policy position articulated in the decision should show increased/decreased support for the Court.

Finally, since those in the immediate community are expected to rate the issue as more important, I expect that they will attach greater weight to the information than those from the surrounding areas. This leads to my final hypothesis.

HYPOTHESIS 4. The effect of hearing about the Court's decision should be greatest among those who reside in the immediate community. They will show greater change in evaluation of the Court following its decision, above and beyond that predicted for those who reside outside the community.

CASES

To test these hypotheses, I use four recent Supreme Court decisions: Two cases involve civil liberties claims, and two are economic/regulation cases. Lamb's Chapel v. Center Moriches Union Free School District (1993) (hereafter the Center Moriches case), involves First Amendment free speech, free exercise of religion, and religious establishment issues. The New York state legislature provided school districts with a list of the types of organizations that could use school facilities after hours. The list included social, civic, recreational,

and political groups but specifically prohibited religious groups from using school facilities for religious purposes. The school district in Center Moriches, New York, allowed many local groups access to facilities after hours, but in 1988 a nondenominational evangelical church, Lamb's Chapel, asked to use the high school auditorium to show a six-part film, "Turn Your Heart Toward Home." It featured a psychologist discussing parenting and family issues from a religious point of view. The school district twice denied the church's request. After two lower courts rejected the church's claims, Lamb's Chapel appealed to the Supreme Court, which agreed to hear the case. Oral arguments were presented in February 1993, and the Court unanimously decided in favor of the church on June 7, 1993. The decision asserted that the law, and the school district's refusal, violated the free exercise and free speech rights of the church. Any establishment of religion claims put forth by the school district were not strong enough to justify infringement of the rights of the church.

The second case also revolves around religion and the First Amendment (hereafter the Monroe case, the city where the controversy began). At issue in Board of Education of Kiryas Joel v. Grumet (1994) was whether the creation of a special school district for a Hasidic Jewish community by the New York legislature violated the religious establishment clause of the First Amendment. The Hasidic Jews in Monroe educate the majority of their children in private schools that they pay for and run, but a number of the children need special education. The Hasidim could not provide that service but opposed using the public school programs, fearing that their children would be traumatized by interactions with outsiders. In 1989 the legislature created a special school district for the community, paid for by the state. The boundaries were drawn to include the entire Hasidic community and excluded all others. Before the district opened, a suit was filed in state court arguing that creation of the district impermissibly advanced a particular religious interest. The trial court issued a summary judgment against the operation of the school district (601 NYS 2061). The Supreme Court heard the case and issued its decision in 1994. It ruled that the creation of the school district violated the establishment clause of the First Amendment. Since no other religious group in New York could expect similar treatment, the state was impermissibly favoring the Hasidic community.

The third case, Oklahoma Tax Commission v. Chickasaw Nation (1995) (hereafter the Oklahoma case) involved state taxes on gasoline sold by Native Americans on tribal land.³ The Supreme Court, in its decision on June 14, 1995, ruled against the taxation because the U.S. government has exclusive control over relations with Native Americans. Since Congress has not enacted such taxation, Oklahoma had overstepped its authority, according to the Court.

² I present the results for four of five studies that were done. The fifth was conducted in the Columbus, Ohio, area and is not presented because the Court decision was handed down the day of the Oklahoma City bombing. The event swamped news coverage, and only about 5% of the respondents in the Ohio samples had heard of the Supreme Court decision. Among the very few individuals who had heard about the decision (nine people), however, the results are completely consistent with the other samples. There are simply too few observations to place much confidence in them.

Reservations per se do not exist in Oklahoma. Instead, Native Americans own land privately, but that land is subject to the same relationship with state and federal government as are reservations.

The fourth case, Babbitt, Secretary of the Interior, et al. v. Sweet Home Chapter of Communities for a Greater Oregon (1995) (hereafter the Oregon case), involved the 1973 Endangered Species Act. At issue was whether the act, which protects endangered species from harm, should similarly apply to the habitat of endangered species. That was Babbitt's position, but business interests in Oregon challenged his interpretation. They argued that the broad interpretation of the legislation placed an unfair burden on private property and businesses by regulating what owners could and could not do with their own property. The Supreme Court agreed with Babbitt, stating that it was logical to protect the habitat as well.

These four cases are rather typical of those the Court routinely decides. None are of the sort to garner the controversy or publicity of cases such as *Roe* v. *Wade* (1973). Although these cases were not chosen randomly, they are fairly representative of the Court's docket each year.⁴

DATA AND METHODS

The data for this project come from four quasi-experimental studies that examine attitudes toward the Court before and after the announcement of the decisions. For each survey, I generated two random samples: one from the immediate community, and one from the surrounding area. The boundaries for the latter were drawn to reflect the coverage of the dominant local newspaper (see Appendix A). I expect the former to perceive the case as more important than the latter. In addition, I drew two additional samples for the Oklahoma and Oregon cases to participate only in the second wave. These were similarly drawn according to town of residence. I used these additional samples to assess whether participation in the first wave had an effect on interest in the cases. The first wave was conducted following oral arguments and before the Court handed down its decision. The second wave took place during the two weeks after the decision.

RESULTS

Hypothesis 1a predicts a relatively high level of local awareness of Court decisions compared to levels typically reported in national samples. To determine awareness, I used an uncued recall question (see Appendix B). Table 1 shows that in two studies awareness was quite high for both the immediate and the surrounding communities. In Center Moriches, 85.1% of the immediate sample and 78.3% of the surrounding sample knew about the decision. In Monroe, the respective figures were 74.1% and 68.6%. In the Oklahoma and Oregon studies, levels in the immediate community were more moderate (39.0% and 47.3%) and in the surrounding areas were somewhat lower, especially in Oklahoma (10.7% and 33.3%, respectively). I also include a pooled estimate of awareness level

TABLE 1. Aggregate Level of Awareness of Supreme Court Decisions

	Immediate Community	Surrounding Area	
Center Moriches N	85.1% (67)	78.3% (46)	
Monroe	74.1%	68.6%	
N	(54)	(70)	
Oklahoma ^a	39.0%	10.7%	
N	(41)	(75)	
Oklahoma (second-wave only) N	, 35.0% (20)	13.6% (22)	
Oregon ^a	47.3%	33.3%	
N	(74)	(72)	
Oregon (second-wave only) N	56.3% (16)	42.9% (14)	
Pooled ^a	62.7%	44.1%	
N	(236)	(263)	

Note: In parentheses is the number of respondents successfully contacted who agreed to perticipate in the second wave. The difference between the immediate communities and the surrounding area is significant at $\rho<0.6$

across the four studies.⁵ Not surprisingly, it shows that those in the immediate community were more likely to hear about the decision than those in the surrounding areas.⁶

How do these results compare with national levels of awareness? I do not have national data for these four cases, but I can compare my results with those from similar cases for which national surveys exist. The best comparable data were collected by Franklin, Kosaki, and Kritzer (1993), who assess national awareness of six cases from the Court's 1992 term. This information is presented in Table 2, along with relevant media information for all cases. Franklin, Kosaki, and Kritzer (1993) examine awareness for cases involving sexual harassment (Franklin v. Gwinnett County Public Schools), sales tax (Quill Corp. v. North Dakota), hate speech (R.A.V. v. City of St. Paul), tobacco company liability (Cipollone v. Liggett Group), school prayer (Lee v. Weisman), and abortion (Planned Parenthood of Southeastern Pa. v. Casey). As Table 2 shows, national

⁴ I discuss this issue of the representativeness of these cases in greater detail below, in the section on media coverage.

⁵ I also conducted subsequent analysis pooling the data, and the results lend support to the hypothesis. I do not present those results for two reasons. First, the expectations are the same for each study, but analysis revealed some differences across the studies. Second, these samples are drawn from different populations, which makes pooling the data inappropriate. I present the pooled analysis on awareness in Table 1 simply to summarize the findings.

⁶ In Table 1 I also include reports from those who participated only in the second wave for the Oklahoma and Oregon studies. The results show they were about as aware, or even more aware, of Court decisions as participants in first-wave studies. Therefore, it is unlikely that participation in the first wave artificially increased attention to the Court's activities.

	Minutes on		New York			Aware	Locally
	Network News*	Lead Story	<i>Tim</i> es Articles ^b		Local Newspaperb	Immediate Community	Surrounding Area
Center Morlches	210	1	2	6	Newsday	85.1%	78.3%
Monroe	340	_	7	25°	Times Herald Record	74.1%	68.6%
Oklahoma	0	_	0	2 6	Dally Oklahoman Ada Evening News	39.0%	10.7%
Oregon	150	34	4	6 2	Oregonian Albany Democratic Herald	47.3%	33.3%
Franklin, Kosaki, and Kritzer (1993) Cases						Aware N	lationally*
Sexual harassment	220	1	1	10	Atlanta Journal Constitution .	0	%
Sales tax	380	1	1	na		15	%
Hate speech	420	2	7	17	St. Paul Ploneer Press Dispatch	24	%
Tobacco ^f	355	1	6	7	The Record	• 30	%
School prayer	395	2	5	3	The Providence Journal Bulletin	28	%
Abortion	1,530	3	17	51	The Harrisburg Patriot	40	%

Note: Data on newspaper coverage for Franklin, Kosak, and Kritzer cases obtained from Online Resources (http://www.onliners.com), a fee-based newspaper archive. Data for local media coverage of my case come from the research/archive office of the newspaper and/or Academic Universe (http://exde-nexts.com/universe). Data on television news coverage come from Vanderbitt Television News archives (http://tvnews.vanderbitt.edu/).
*On the evening news the day of the decision only.

^olinoludes one story in the Sunday magazine.

The Oregon decision was handed down the same day as a congressional redistricting case and a religion case. All three were reported together on all three networks as the lead story. On all three networks the Oregon case was reported following the other two.

"The percentage aware probably overestimates the level ence it is the high point of awareness in the Franklin, Kosaki, and Kritzer study. The percentage that I present is total awareness during the entire two-week period. In sum, the companion between my study and that of Franklin, Kosaki, and Kritzer is conservative.

The tobacco and school prayer decisions were handed down the same day. The Vanderbitt transcripts of the coverage of these two cases on CBS listed them under one subheading. That listing stated that the combined coverage lasted four minutes, ten seconds. I divided that in half, allotting to each two minutes, five seconds (125 seconds each).

awareness ranges from a low of virtually none (the sexual harassment case) to a high of 40% (the abortion case).⁷

In both the immediate and surrounding communities, awareness in Center Moriches and Monroe exceeded the national level in the most similar cases in the Franklin, Kosaki, and Kritzer study. In fact, in those two cases, local awareness even surpassed national awareness of the abortion decision. The same is true for the immediate community in the Oregon case, although awareness of the abortion decision was higher in the surrounding areas. Even in the Oklahoma case, which received virtually no attention in the national media, immediate community awareness was similar to the Franklin, Kosaki, and Kritzer figures for abortion,

although national abortion awareness was markedly higher than awareness in the surrounding areas. The reasons for generally high awareness of the local issue is simple: The local papers provide access to information that is not widely available elsewhere in the nation. This is supported by the data on local media coverage of the cases studied by Franklin, Kosaki, and Kritzer (1993). In four of the five cases for which local media coverage was available, greater attention was paid to those cases than in the national media.⁸

As hypothesis 1b suggests, however, exposure to information is only part of the picture. Those who reside in the immediate community should show

^bincludes editorials.

⁷ I report the highest level of swareness in their national samples. Their study also reports awareness for each day following the Court's decision. There is a definite peak at the time of the decision and then gradual decline. The measure I report for my studies is total awareness over the entire sampling period, regardless of the amount of time lapsed between the decision and the survey. This provides for a conservative comparison between their study and mine. The abortion decision is abit of an outlier in terms of media attention. It appears in 17 New York Times articles, and the three networks made it the lead story on the evening news broadcast, devoting a combined 1,530 seconds to it. The sexual harassment decision is also somewhat of an outlier. It received moderate attention in the national media, yet it did not register with the national public at all.

Since my cases and two of the Franklin, Kosaki, and Kritzer (1993) cases specifically mention the city or school board in the name of the case, I made one other media comparison for the sake of caution. I took a random sample from the Court's 1996–97 term and examined local and national media attention for those cases (see Appendix C). Although I do not have data on awareness level for those cases, the findings for media coverage of them support my findings regarding local media coverage. Approximately half the cases received greater local than national media attention. For those cases with local media coverage equal to or less extensive than national attention (i.e., New York Times), there was even less, if any, coverage elsewhere in the nation. In other words, cases were covered by the local media, and often the New York Times, but not the local media m other parts of the country. This supports the claim that the local media are important in disseminating information about the Supreme Court.

greater interest in and motivation to seek out information than those in the surrounding area, even though the information available is roughly equal.9 In other words, there should be differences within each of the studies. The aggregate measures in Table 1 support this possibility, but it is impossible to determine whether there are significant differences at the individual level between the geographic samples without controlling for factors that tend to increase general political knowledge, such as attention to media, the frequency of engaging in political discussions, level of education, and gender. Each has been shown to increase attention to politics in general and to Supreme Court decisions in particular (Franklin and Kosaki 1995). In addition, Franklin and Kosaki suggest that case-specific factors such as religion and race may positively affect awareness of cases that deal with those issues. For example, Catholics should pay more attention to abortion decisions, and African Americans should pay more attention to discrimination cases. Similarly, those from the immediate community in which the controversy began should be more inclined to seek out information about that case, regardless of the substantive topic.

To test for the effect of geographic location on attention to Court cases, I looked at the effect of town of residence while controlling for factors previously shown to influence awareness of Court decisions. The dependent variable is simply whether the respondent knew how the Court had decided (following the filter question that asked whether they had heard anything about the issue recently). It is a dichotomous variable; 1 indicates that they knew the decision, 0 otherwise. Because the dependent variable is dichotomous, I analyzed the relationship using logistic regression. The independent variables were attention to media and politics, education, gender, and the variable most important to this research, town of residence (see Appendix B). Attention to politics and media was measured by the number of days in the last week that the respondent had (1) watched the news on television (Television Viewing), (2) read the news in a daily paper Newspaper Reading), and/or (3) discussed politics (Political Discussion). Education was measured on a fivepoint scale that ranged from 1 (less than high school) to 5 (postgraduate education). Gender was measured as male (1) or female (0). Town of Residence was coded 1 for those who reside in the immediate community, 0 for those who reside in the surrounding areas. Based on previous research, I expect each variable to be positively related to the probability of having heard about

The results are presented in Table 3. In two of the

four studies, living in the immediate community significantly increased the probability of hearing about the Court's decision. Only in Center Moriches and Monroe, where there were very high levels of awareness and little aggregate variation between the local community and surrounding areas, did town of residence prove statistically insignificant. In both cases it is in the correct direction, however. As expected, gender, education, and the frequency of newspaper reading and political discussion are also important explanations of individual levels of awareness of the decision.¹⁰

To understand better the substantive effect of town of residence along with the other independent variables, I calculated a series of predicted probabilities and present the results in Table 4. The probabilities are arranged for various levels of education, attention to media (newspaper and television), and the frequency of engaging in political discussions. The analysis was conducted separately for those from the immediate and surrounding communities. Overall, the probability of hearing about the Court's decision increases with education and attention to politics. More important for the purposes of this study, in each survey those from the immediate community are predicted to be more likely to hear about the decision than those from the surrounding areas.

The data in tables 3 and 4 are consistent with what we know about the effect of education, gender, and engagement in politics on levels of political knowledge. What is more important, the data reveal how one measure of interest, geographic proximity to the origins of a controversy, can increase attention to and interest in a specific Supreme Court case even after controlling for these other variables.

Perceived Importance of the Issue

Hypothesis 2 predicts that those who hear about the decision and reside in the immediate community will find the issue more important than those who reside in the surrounding areas. I used four measures of importance in both waves of the survey: (1) how strongly the individual felt about the issue (Strength of Opinion), (2) how much time s/he spent thinking about the issue (Time Thinking), (3) how important the issue was personally (Personally Important), and (4) how important it was to the community (Important to Community). Each variable was measured on a three-point scale (1 = not very strongly/often/important; 3 = verystrongly/often/important). The Center Moriches study included only a single measure of importance (strength of opinion). I also report a summary measure (Summary), which simply adds the responses to four items. It ranges from 0 to 12. Since I am most concerned with respondents' perceptions of the importance of the issue after hearing the Court's decision, I present the importance ratings from the second wave, shown in Table 5.

⁹ My samples were designed to reflect the coverage of the dominant newspaper in the region, but smaller newspapers operate in some of the towns. For example, in the Oklahoma study, some of the residents in Ada (the immediate community) reported reading about the decision in the Ada Evening News, but most of the respondents there and in the surrounding areas reported the source as the local television news, followed closely by the radio The main newspaper for both groups in the Oregon sample is the Oregonian, but some respondents in both groups read about the decision in the Albany Democrat Herald; the most common source for both groups was television news.

¹⁰ Although I do not present the pooled model for reasons described earlier, the results from pooled analysis show that each variable, except the frequency of television viewing, is positively and significantly related to the probability of hearing about the decision.

TABLE 3. Probability of Hearing about Court Decision (Logistic Regression) Center								
	Morlches	Monroe	Oklahoma	Oregon				
Town of residence	.43	.65	1.98**	.98**				
	(.55)	(.46)	(.59)	(.41)				
Gender	56	1.08**	1.62**	.68 *				
	(.5 9)	(.47)	(.62)	(.38)				
Newspaper reading ⁴	.19*	.13	.26**	.05				
	(.10)	(.09)	(.12)	(.06)				
Television viewing:	.13	- <i>.22</i> *	.04	.00				
	(.10)	(.10)	(.12)	(.08)				
Political discussions	_	.15 (.11)	.19* (.11)	.25** (.08)				
Education	.63**	.39 *	.21	.18				
	(.28)	(.21)	(.25)	(.18)				
Constant	-1.63	84	-5.8 6**	-2.37**				
	(1.04)	(.86)	(1.33)	(.77)				
N	112	122	115	145				
-2 × Log-likelihood ratio % correctly predicted Dependent variable	88.41 83.93	128.65 76.23	85.08 82.61	174.59 68.28				
1 = 0 =	83.04	70.49	20.87	40.69				
	16.96	29.51	79.13	59.31				

Note: The measure of the frequency of political discussions with friends or neighbors was not included in the Center Moriches study. *p < .05, **p < .01, one-tailed hypothesis test. Standard errors are in parentheses.

"The frequency of newspaper reading, television viewing, and political discussions were obtained in the second wave.

In each study, there is some evidence that the members of the geographic communities rate the importance of the issues differently. In the Center Moriches study, which measured only strength of opinion, the differences between the communities are not what hypothesis 2 predicts. Residents in surrounding areas felt more strongly about the issue than did residents of

the immediate community. This may be due to the fact that only a single measure was used, and it fared poorly in the Monroe and Oregon studies, too. In the remaining three studies the evidence is generally as expected. Those from the immediate community rated the issue more important than did those in the surrounding areas. The item assessing perception of the importance

TABLE 4. Predi				
	Male (Education = 1	Male (Education ≈ 3	Male (Education = 3 1 Newspaper = 7 2 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1	
	· Newspaper = 1	Newspaper = 4		
	TV News = 1	ews = 1 TV News = 4		
	Talk Politics = 1)	Talk Politics = 4)	Talk Politics $= 7$)	
Center Morlches				
Town	.31	.80	.91	
County	.22	.72	.87	
Monroe		•		
Town	.83	.92	.94	
County	.72	.86	88	
Oklahoma	•			
Town '	.17	.58	.86	
County '	.03	.16	.46	
Oregon			, 1991 s	
Town .	.44	.74	.87	
County	.23	.51	.72	

Note: The variable Education is coded 1 = less than a high school education, 2 = high school, 3 = some college, 4 = four years of college, 5 = postgraduate education. Newspaper, TV News, and Talk Politics are all measured as the number of days in the last week that the respondent engaged in these activities.

TABLE 5. Ratings of the Perceived Importance of the Issues at First and Second Interview, by Geographic Community

	Center Morlches		Monroe		Okl	ahoma	Or	egon
	Immediate	Surrounding	Immediate	Surrounding	Immediate	Surrounding	Immediate	Surrounding
Strength of oplnion p <	2.24 (.10)	2.53 (.12) .03	2.48 (.10)	2.39 (.11)	2.50 (.16)	1.87 (.23)	2.66 (.10)	2.50 (.11)
N	53	.03	40	.29 46	16	.02 8	32	.15 22
Time thinking p <		_	1.87 (.13)	1.79 (.09) .31	1.81 (.16)	1.37 (.18)	2.29 (.12)	2.29 (.17)
N			39	.31	16	.06 84	34	.50 24
Personally Important p <	_	_	2.10 (.11)	2.00 (.11) .26	2.25 (.17)	1.50 (.19) .01	2.35 (.13)	2.25 (.12) .29
, N			4	48	16	.01	34	24
Important to community p <	_	_		2.31 (.11) .00	2.75 (.14)	2.25 (.25) .04	2.85 (.09)	2.38 (.15)
N			39 ′	45	16	8	34	24
Summary	_	_	9.26 (.27)	8.63 (.32)	9.31 (.47)	7.00 (.42)	10.13 (.26)	9.50 (.45)
p < _ N	53	34	39	.07 43	16	.00 8	32	.10 22

Note: Analysis is restricted to those who were aware of the decision. Cell entries are means. Standard errors are in perentheses. Probabilities are based on one-tailed difference in means test. The summery measure is a combination of all four importance ratings.

to the community and the summary measure provide the strongest and most consistent support for hypothesis 2.11

In short, there is moderately strong evidence to support hypothesis 2. This is important for two reasons. First, it suggests that members of the public perceive some Court decisions as more important than others, and this is due to more than mere exposure to the information. Respondents from both groups in each study had roughly equal access to the information, but those from the immediate community perceived the issue as more important. Second, this finding is important as a basis for the underlying assumption of the final hypothesis, that Court decisions have an effect on evaluations of the Court, especially among those from the immediate community.

Evaluation of the Court

Hypothesis 3 predicts that initial support for (opposition to) what the Court ultimately decides should increase (decrease) support for the Court. Furthermore, and as hypothesis 4 predicts, this effect should be greater among those from the immediate community than from surrounding areas. People in the immediate community who support (oppose) the ultimate decision

should show a greater increase (decrease) in support for the Court than do people in the surrounding areas.

To create the dependent variable for measuring change in support, I simply subtracted the evaluation of the Court the respondent offered in the first wave from the evaluation offered in the second wave. The measure consisted of asking people whether they approved or disapproved of the Court, followed by a question asking how strongly. Responses were combined into a seven-point scale. The differenced dependent variable was calculated such that positive values indicate greater support for the Court following the decision, whereas negative values indicate a less favorable evaluation.

Four independent variables were included in the analysis. Initial Evaluation of the Court was measured from 1 (disapprove very strongly) to 7 (approve very strongly). This was used for both statistical and substantive reasons. Statistically, it controls for any regression effects, which is the tendency for extreme values to become less extreme on subsequent measurement (Finkel 1995; Markus 1990). Since regression to the mean is movement in the opposite direction, initial values may be negatively related to subsequent values, and the coefficient should be negative. Substantively, a simple transformation of the coefficient $(1 + \beta)$ provides information relevant to interpretation. The number that results from this transformation is the same as would result if the level of support for the Court at the second wave (rather than change in support) was regressed on the level of support at the first wave (see

¹¹ I do not present the data, but pooling across the samples yields virtually the same picture. Members of the immediate community rated the issue more important across all four measures, but the differences are statistically significant for personal importance, community importance, and the summary measure.

TABLE 6. Change in Evaluation of the Court among Those Who Knew about Decision

	Monroe	Oklahoma	Oregon
Initial evaluation of	23**	25	47**
Court	(.09)	(.16)	(.12)
initial opinion on Issue	.07	.51*	03
	(.09)	(.26)	(.13)
Town of residence	-1.08*	2.17*	−.90
	(.65)	(1.23)	(.82)
Initial opinion \times town of residence	.20*	54 *	.37*
	(.12)	(.28)	(.20)
Constant	.74	59	2.05**
	(.69)	(1.46)	(.87)
F	3.89	2.18	4.72
Probability of F	.01	.11	.00
Adjusted R ²	.12	.17	.20
N	88	24	59

Note: The measure of support for the Court was not included in the second wave of the Center Morlohes study. *p < .05, **p < .01; one-talled hypothesis test. Standard errors are in parentheses.

Finkel 1995). That is, it is equivalent to the effect of prior evaluation of the Court on subsequent evaluation. For example, if the effect of prior evaluation of the Court on change in evaluation were -.35, this would mean a portion of the change is due to stability (1 + [-.35] = .65), or about 65% of the current support of the Court is explained by prior support. Since the conventional wisdom is that the Court is insulated from the effect of its decisions, the more cautious approach is to include prior evaluation.

The second independent variable is the respondent's initial opinion on the issue in the case (Initial Opinion on the Issue). This is measured on a seven-point scale; 1 indicates initially very strong disagreement (with the position the Court ultimately adopted), and 7 indicates initially very strong agreement. The coefficient on this variable should be positive. The third variable is town of residence, coded 1 for residents of the immediate community, 0 otherwise. Finally, an interaction term (initial opinion × town of residence) tests hypotheses 3 and 4. Positive and significant values indicate that the effect of agreement or disagreement is greater among those from the immediate community than those from the surrounding areas.

The results are presented in Table 6. Note that prior evaluation of the Court proves significant in two of the three studies. Thus, some of the change is due to regression effects. Prior attitudes do spill over into current evaluations, as previous research would lead us to expect. In Monroe, the coefficient of -.23 suggests that about 77% (1 + [-.23]) of previous opinion explains current evaluation. In Oregon, the figure is about 53%. In Oklahoma, the coefficient is not statistically significant.

Yet, prior attitudes do not explain all the change. Decisions indeed alter feelings about the Court, as suggested by the interaction between town of residence and (dis)agreement with the decision rendered. The coefficient is significant in all three studies. It is in the predicted direction for Monroe and Oregon but in the opposite direction for Oklahoma.

Looking first at Monroe, the interaction (.20) suggests that those who reside in the immediate community and approve of the Court's decision became more supportive than those who reside in the surrounding areas and approve of the decision. Conversely, those from the immediate community who disagree with the decision became less supportive, and to a greater extent, than those from the surrounding areas who disagree. The results are virtually the same in the Oregon study. In the Monroe study, however, the statistically significant and negative coefficient on town of residence (-1.08) suggests that among those from the immediate community, the effect of disagreement may be stronger than agreement. This may be similar to negativity effects that have been found in other areas of political behavior (e.g., Lau 1985).12 The town of residence variable in the Oregon study is also negative but not significantly so.

In the Oklahoma study, the interaction shows that the effect is in the opposite direction hypothesized. This suggests that members of both communities changed their opinion in accordance with how the Court decided (hypothesis 3), but the effect was greater in surrounding areas than in the immediate community. Monroe and Oregon are statistically significant at p < .05. In the Oklahoma study, the overall model does not quite reach conventional standards of statistical significance (p = .11). 13

CONCLUSION

Court decisions matter. When the issue is perceived as important and is covered by the media, the public exhibits rather surprisingly high levels of awareness. In addition to geographic proximity, awareness is affected by education, gender, attention to the media, and the frequency of political discussions. At the very least, we ought to be skeptical about research suggesting that Court decisions go unnoticed by the public. Most people may not know about most, or even many, of the rulings, but they do hear about those that have some relevance to their community. Furthermore, there is evidence that satisfaction with those decisions influences subsequent evaluations of the Court. This is not to say that satisfaction is the sole factor, only that decisions may be an ingredient in individual levels of

¹² Another possible explanation for the statistically significant coefficient may be that, on average, those from the immediate community felt less support for the Court's decision than did those from the surrounding communities. If so, the town of residence variable might pick up some of that effect. Among those who heard about the decision, however, there was no statistically significant difference in support for it between those in the immediate and surrounding communities.

¹³ I do not present the pooled results, but they lend mixed support to the hypotheses. Initial evaluation proved statistically significant and in the correct direction, but no other variable proved significant. The reason should be fairly clear. Expectations were similar across the samples, but the results were not.

support. The effect of unpopular decisions is mitigated to some extent by the perception that the Court operates in a less political, partisan, or ideological fashion than other branches, as the process argument maintains (Hibbing and Theiss-Morse 1995). The dynamic argument advanced by other scholars is also supported by the data (Caldeira and Gibson 1992; Mondak and Smithey 1997). Prior evaluations of the Court carry over into subsequent assessments after hearing about specific rulings.

The findings also suggest a more generalizable theory of the influence of Supreme Court decisions. I examined specific communities, but so long as people identify with the parties involved and there is some way to hear about cases, we could expect analogous results. This is consistent with research that found Catholics are likely to hear about the Court's abortion decisions in church (Franklin and Kosaki 1989) or that people hear about decisions related to their work (Berkson 1978). It is also consistent with the material on national levels of awareness given in Table 2. Although I presented the Franklin, Kosaki, and Kritzer data (1993) to show that local awareness exceeds national awareness, the latter was substantial in many cases, too. It is not a great leap to suggest that people across the nation who heard about those cases may have changed how they feel about the Court. Geography is one place to look for such an effect, and the media results suggest that it is probably common. Yet, there are many types of communities other than geographical where this may occur. Future research should examine other audiences for Supreme Court decisions to arrive at a more thorough description of the bases of individual support for the Court.

The four cases I studied originated in various parts of the nation, but large metropolitan areas such as New York City or Los Angeles were not included, so it is impossible to generalize to such a sizable and diverse community. The effect in very populous areas may be smaller, since any single news story must compete with so many others in a given day. Nevertheless, when people hear about a case, and at some point most people do, it has the potential to affect how they feel about the Court.

The implications for theories of support are important. Although the Court tends to be more popular than other institutions of government, we have not been able to pinpoint the reasons. As with many other aggregate measures, individual patterns may be hidden (see also Mondak and Smithey 1997). In each study presented here, neither uniform support nor opposition emerged. Some people were pleased with the ruling and became more favorable to the Court, others were displeased and felt less favorable. As with public opinion in general, on these issues it was divided. Therefore, aggregate data can mask individual behavior.

The fact that decisions have an effect on support for the Court presents an unusual problem. Because the public is assumed to be ignorant of rulings, the Court is typically thought to be relatively isolated from public opinion. My research suggests we should be less pessimistic about public knowledge of Supreme Court decisions. My focus was local geographic areas, but we should carefully and systematically think about and look for similar effects in other types of communities.

APPENDIX A. SAMPLING

The sampling frame for the Center Moriches study was 326 working telephone numbers. Of these, 212 were contacted and agreed to participate in the first wave, for a response rate of 65%. For the second wave, 112 were successfully recontacted and agreed to participate, for a retention rate of 52.8%.

For the Monroe study, the sampling frame was 300 working numbers, and 167 agreed to participate in the first wave, for a response rate of 55.67%. Of these, 122 were successfully recontacted and agreed to take part in the second wave, for a retention rate of 73.1%.

In the Oklahoma study, 206 people participated in the first wave, which from a sampling frame of 395 means a response rate of 52.15%. The second wave consisted of 116 of those respondents, for a retention rate of 56.3%.

The Oregon study had a sampling frame of 413 numbers. The 243 people who took part yielded a response rate of 58.8%. For the second wave, 145 of them participated, for a retention rate of 59.7%.

Overall, among the 836 individuals in the first wave, 495 were contacted after the Court's decision and agreed to be interviewed again.

APPENDIX B. SURVEY ITEMS ANALYZED

How many days in the past week did you watch the news on TV?

How many days in the last week did you read a daily newspaper?

How many days in the last week did you discuss politics with friends or family members?

Next, I'd like to ask your opinion on a few issues being discussed in the news lately. We understand that most people may not have the time or interest to follow every issue in the news. However, we are interested in knowing how you feel about a few of these issues, how much time you spend thinking about these issues, and how important they are to you.

Center Moriches. Do you feel that a religious group ought to be allowed to use public school facilities after school hours if other community groups are allowed to use the facilities, or do you believe that a church should not be allowed to use the school facilities?

Monroe. Do you believe that it is all right for school districts to be specially created for a community sharing a common religious faith, or do you believe that there should not be special consideration of religious beliefs when creating school districts?

Oklahoma. Do you think Oklahoma's gasoline tax should be collected on gasoline sold by Native Americans on tribal land, or do you think the government should not be involved in tribal business?

Oregon. Do you think the government should protect the habitat of endangered species with strict building restrictions? Or do you think the government goes too far in protecting endangered species?

Do you hold this opinion very strongly, strongly, or not so strongly?

Compared to other issues you might think about, how much time do you spend thinking about this sort of issue? Would you say you think about it a lot of the time, some of the time, or not much time?

Compared to other issues you might think about, how important do you think this issue is to you personally? Would you say it is very important, somewhat important, or not very important?

Compared to other issues you might think about, how important do you think this issue is to your community? Would you say it is very important, somewhat important, or not very important?

Now let's consider the way the U.S. Supreme Court makes decisions. I'm going to read you a series of statements. After I read each of these statements please tell me whether you strongly agree, agree, disagree, or strongly disagree with the following statements.

In general do you approve or disapprove of the way the Supreme Court is handling its job?

Do you approve/disapprove very strongly, strongly, or not strongly?

What is the highest grade of school or year of college you have completed?

What town do you live in?

We understand that most people may not have the time or the interest to follow everything that happens in the news. However, we are interested in knowing whether or not you have read or heard anything in the news recently about the issue of (issue in case)? (If yes, asked to explain.) (If they mention the Supreme Court:) And how did the Supreme Court decide?

APPENDIX C. LOCAL AND NATIONAL MEDIA ATTENTION TO A SAMPLE OF SUPREME COURT CASES (1996-97 TERM)

Case		Local Media Stories	New York Times
1. Arkansas'v. Farm Credit Services of			•
Central Arkansas	1	Arkansas Democrat Gazette	·0
2. Atherton v. FDIC	1	Star Ledger	0
3. Bennett v. Spear	2	Bulletin	2
4. Board of the County Commissioners of	1 (1)	Dally Oklahoman	
Bryan County, Oklahoma v. Brown	1	Tulsa World	2
5. California Division of Labor Standards	2 (1)	Los Angeles Times	0
Enforcement v. Dillingham Construction	1 `	Fresno Bee	
	1	San Francisco Examiner	,
	2 (1)	Sacramento Bee	
6. Chandler v. Miller	5 (1)	Atlanta Journal Constitution	2
of Grandor Vivinae	1 (1)	Augusta Chronicle	
7. General Motors Corp. v. Tracy,	1 ` ′	Columbus Dispatch	0
Tax Comm'r of Ohlo	1	Dayton Dally News	
TEX COMMIT OF CITE	1	Plain Dealer	
	1	Cincinnati Enquirer	
8. Idaho v. Coeur D'Alene Tribe	i	Spokesman Review (Idaho edition)	1
9. Glickmen v. Wilemen Brothers & Ellott, Inc.	1	Daily News of Los Angeles	1
9. Carchinari V. Wildright Dictroits & Elicity mo.	2	Los Angeles Times	•
	2 (1)	Fresno Bee	
	1	Sacramento Bee	
	i	Lewiston Morning Tribune	
	i	San Francisco Chronicle	
10. Inter Modal Rail Employee's Assoc. v. Atchison,	1	Los Angeles Times	1 '
Topeka & Santa Fe Railway Co.	•	LOS 74 Igaios Timos	
1000KB & SBITE TO HEIWAY CO.	na		1
11. Joseph P. Mazurek, Attorney General of Montana v. James H. Armstrong	1 MCI		•
	1	Star-Tribune	0
12. Kleur Et Ux. v. A. O. Smlth Corp.	2	Milwaukee Journal Sentinel	Ö
13. Lindh v. Murphy, Warden	1	Capital Times	J
	1	Wisconsin State Journal	•
	-	Beltimore Sun	1
13. Maryland v. Wilson	3 (1)	 	<u>'</u> ,
	1 (1)	Dally Record	5
14. O'Dell v. Netherland, Warden	2	Roanoke Times and World Report	3
	3 (1)	Virginian Pilot	
	3 (1)	Richmond Times Dispatch	1 4 (7)
15. Printz, Shertf/Coroner, Ravalli County,	na	,	4 (2)
Montana v. United States		T 0/	4
16. Reno v. Bossler Parish School Board	1 (1)	Times-Picayune	1 3
17. Richards v. Wisconsin	3 (2)	Milwaukee Journal Sentinel	3
	1 (1)	Capitol Times	
	2	Wisconsin State Journal	= (4)
18. Schenck v. Pro Cholce Network of	7 (2)	Buffalo News	5 (1)
	0 (4)	T1mes Union	
Western New York 19. Strate v. A-1 Contractors	3 (1) 0	TITIOS CINCIT	0

APPENDIX C (Continued)

Case		Local Media Storles	New York Times
20. United States v. Alaska 21. United States v. Brockamp, Admin'r of the	2 (1)	Anchorage Dally News	2
Estate of McGIII, Deceased	3 3 (1) 3	Los Angeles Times Tennessean Chattanooga Free Press	1
22. United States v. Lanier 23. United States v. O'Hegan	1 (1) 1 (1)	Commercial Appeal Star Tribune	1

Note: Information obtained from a search of state newspapers on Academic Universe, a subset of Lexis-Nexos (http://exos-nexis com/universe). Numbers in perentheses indicate the number of front-page stones. No media information was available for Montana.

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Strategic Auditing in a Political Hierarchy: An Informational Model of the Supreme Court's Certiorari Decisions

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e examine how the Supreme Court uses signals and indices from lower courts to determine which cases to review. In our game theoretic model, a higher court cues from publicly observable case facts, the known preferences of a lower court, and its decision. The lower court attempts to enforce its own preferences, exploiting ambiguity in cases' fact patterns. In equilibrium, a conservative higher court declines to review conservative decisions from lower courts regardless of the facts of the case or the relative ideology of the judges. But a conservative higher court probabilistically reviews liberal decisions, with the "audit rate" tied to observable facts and the ideology of the lower court judge. We derive comparative static results and test them with a random sample of search-and-seizure cases appealed to the Burger Court between 1972 and 1986. The evidence broadly supports the model.

rierarchical control of organizations is problematic throughout the realm of politics. Congress and presidents attempt to control agencies, upper levels of bureaucracies attempt to control lower levels, and higher courts strive to control lower courts. With incomplete information about their subordinates' decisions and knowledge, superiors in rule-based hierarchies often employ some form of auditing. In this article we study how the Supreme Court uses signals and indices from lower courts to pluck a relative handful of cases from a plethora of potential candidates for review. Our point of departure is the role of review in enforcing the doctrinal preferences of the Supreme Court within the judicial hierarchy.

We begin by presenting a game-theoretic model of the Court's certiorari process. The model goes much farther than earlier efforts to incorporate concepts commonly employed by judicial scholars. For example, we include the legal concepts of case facts, doctrine, and holding; formalize the attitudinal model; and include an explicit role for judicial culture. Most important, the model puts asymmetric information at its analytic center, following the hints in recent empirical

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studies based on interviews with justices of the Supreme Court and their clerks (Perry 1991).

The model explains several well-established patterns in certiorari, such as the importance of amicus briefs by the Solicitor General. It also generates several entirely new and nonobvious predictions. We test the predictions on a random sample of 274 decisions in the area of search-and-seizure law heard in the U.S. Courts of Appeals and appealed to the Supreme Court during the Burger years. The data strongly display the patterns predicted by the model. Thus, in addition to contributing to the certiorari literature, we hope to advance the burgeoning literature on the strategic behavior of courts (e.g., Caldeira and Wright 1988; Epstein and Knight 1998; Hall 1992).

The article is organized as follows. We first discuss the role of information in the certiorari process and the place of certiorari decisions in the incentive system of the judicial hierarchy. We then present the formal model, followed by the empirical analysis. We next discuss the implications of the findings and offer conclusions in the final section. All proofs are gathered in the Appendix.

INFORMATION AND INCENTIVES IN THE **CERTIORARI PROCESS**

The Supreme Court's decision to hear a case—in legal parlance, to grant certiorari—is perhaps the best studied area of the high court's decision making. A full review of the literature is outside the scope of this article and is unnecessary here, but it is helpful to review several major themes relevant to these decisions.1

¹ The Supreme Court uses the Rule of Four to grant certiorari, but a majority is necessary to prevail on the ments. Presumably, forwardthinking justices take the latter into account when voting to grant certiorarl. Absent a careful analysis of the strategic properties of the Rule of Four (to the best of our knowledge, no formal model of this procedure has appeared in print), we treat the Court as a unitary actor with respect to the lower courts.

Certiorari and incentives in the Judicial Hierarchy

Certiorari and Interactive Incentives. The certiorari system creates incentives for upper and lower courts alike. One cannot get very far with a deductive theory of certiorari without thinking about those incentives, but a strange feature of the contemporary field of judicial politics is that the literatures on certiorari decisions and judicial compliance have developed largely independently. Most of the empirical studies of certiorari decisions ignore doctrinal deviations by lower courts (we note some exceptions below). In turn, empirical studies of the responsiveness of lower courts to Supreme Court precedent typically ignore review probabilities. In other words, there has been scant appreciation of the fact that the certiorari process creates an interactive incentive system between higher and lower courts, with the behavior of appellate courts affecting the behavior of the Supreme Court and vice

A partial exception is the empirical work of Ulmer (1984), who used conflict with Supreme Court precedent as a predictor of certiorari, although he did not recognize that the probability of certiorari is apt to affect conflict with precedent. Ulmer noted that counsel for appellants seem to believe conflict with precedent galvanizes the Court into granting certiorari because such conflict is claimed in more than half of all petitions (six or more conflicts were claimed in more than 10% of the petitions). Attorneys may try to manipulate this cue by padding their claims about conflicts, so Ulmer tried to classify his sample into cases that do and do not contain "real" conflict with precedent. Using this distinction, he found that actual conflict is the single most important variable for understanding grants of certiorari; it explains a higher proportion of the variance than any of the other cues noted by Tanenhaus et al. (1963) and other scholars.

Ulmer's subjective coding of conflict is not amenable to replication, however. Moreover, his analysis puts dated precedents and more recent ones on a par although lower courts almost certainly defer more conscientiously to the preferences of the contemporaneous Court. Finally, Ulmer's dichotomization of real conflict leaves little room for nuance. Many scholars recognize that conflict involves subtle gradations of noncompliance (Feeney 1975). Nonetheless, one cannot read Ulmer's analysis without becoming aware of certiorari as a tool for policing the doctrinal decisions of lower courts.

Even if one accepts that certiorari creates an interactive incentive system, formalizing a model of it requires answers to two more questions. What does doctrinal compliance really mean? What sanctions and rewards are available to the Supreme Court if it detects noncompliance?

Doctrinal Compliance. The concept of a legal doctrine is central to the process of legal reasoning. What is a legal doctrine, and what does it mean to comply with a doctrine? We take as fundamental the conception implicit in classic descriptions of legal reasoning (e.g.,

Levi 1948): A legal doctrine indicates which fact situations are to be grouped together and treated similarly. In other words, it creates a set of equivalence classes in a fact or case space (Kornhauser 1992). When a lower court conforms to a legal doctrine, it accepts the equivalence classes defined by the higher court and acts in accordance with them. Nonconformity means that the lower court treats the case inappropriately for the class to which the case belongs. This concept of doctrine can be seen as an example of the broader notion of rule-governed behavior, a phenomenon general to bureaucracies. We formalize (part of) these conceptions of doctrine and doctrinal compliance below and employ them in the empirical analysis.

Sticks, Carrots, and Culture. What sanctions or rewards can a higher court bring to bear on lower courts that do not comply with its doctrine? In the American system of jurisprudence, the formal answer is "very few." Higher courts cannot promote, demote, or fire; they cannot cut salaries, give bonuses, or offer stock options. Thus, the Supreme Court possesses none of the motivational tools typically employed by hierarchical superiors. This is one of the most striking features of the federal judiciary considered as a hierarchical organization.

A focus on traditional motivational tools is too constricted a view of judges, however. In fact, two sources of control are available to higher courts. First, lower courts care about the disposition of cases: They wish to see justice done, at least as they conceive it. If a higher court reverses the decision of a lower court, the latter may well view the ultimate disposition of the case as much less attractive than if its judgment had stood. Hence, reversal itself can be a kind of sanction, at least for judges who care about the disposition of cases. Second, informal sanctions supplement the formal rules. "Judicial culture" famously includes a desire to avoid reversals. Frequent reversals bring the derision of colleagues and a decline in professional status. Higher courts are well aware of this sanctioning power. For example, Perry (1991, 267) notes that Supreme Court clerks "frequently talked about the need to 'slap the wrist' of a judge below." The importance of judicial culture should not be surprising. Federal judges belong to a very special and relatively close-knit society, and their informal culture is apt to affect their decisions.

Enforcing Doctrine Versus Creating Doctrine. Our model stresses the role of certiorari in enforcing doctrine, but this is a very partial view of the process. Perhaps equally important is the selection of cases as vehicles for creating new doctrine, for example, in novel fact situations. These often occur in the context of intercircuit conflict, when different circuits take different positions on new issues, which the Court must then answer. Indeed, the incremental, fact-soaked creation of new rules is one of the most interesting and distinctive elements of judicial politics. The justices' emphasis on finding "good" cases and "well-percolated" cases underscores this important part of the certiorari process (Perry 1991, chap. 8). In addition, the model ignores aggressive grants, that is, situations in

which the Supreme Court grants certiorari in order to affirm a lower court decision and make it binding on the entire nation.²

We defend the approach we take by noting that law creation is an important part of certiorari, but modeling it involves even more complex theoretical issues than the enforcement of doctrine, which is our focus. The development and testing of a rigorous theory of doctrinal enforcement is an important step toward a more general theory of the judicial hierarchy.

Structure of the Hierarchy. Our analysis simplifies the judicial structure into upper and lower courts. In reality, the federal judicial hierarchy has three levels, and courts at the first level play an important role in fact-finding. Our simplified structure enables us to explore the essential principal-agent dilemma. Extensions to this model could examine additional features of the judicial hierarchy, such as the special role of trial courts.

information and Certiorari

From "Cue Theory" to Strategic Manipulation. Political scientists have long recognized the central importance of information in the Supreme Court's certiorari process. Tanenhaus et al. (1963) argue that the Court must economize in its search for information, since each year it is confronted with a blizzard of cases and tens of thousands of pages of documents. In particular, they argue, the Court is apt to rely heavily on easily distinguishable but highly informative "cues," such as splits in the lower court, to winnow out the potential cases of interest.

Tanenhaus's cue theory, as it came to be known, provoked a flurry of empirical work that continues to this day. Studies tried to determine which cues the Court uses (if any), whether cues are dispositive or merely suggestive, and whether the Court's use of cues changes over time (Armstrong and Johnson 1982; Teger and Kosinski 1980). Development of the underlying theory of information acquisition received little attention, however. Consequently, scholars largely ignored the strategic manipulation of cues, although they soon recognized the importance of reputation in the behavior of the Solicitor General (Caldeira and Wright 1988; Tanenhaus et al. 1963; Ulmer 1984).

The neglect of the strategic dimension of cue theory began to change when Caldeira and Wright (1988) demonstrated the effect of amicus briefs on the probability of granting certiorari. When coupled with new findings about the litigation strategies of interest groups, Caldeira and Wright's finding strongly suggests that at least one cue, the presence of an amicus brief,

is deliberately manipulated by interested parties in order to affect the behavior of the Court (Epstein 1991). This move to greater theoretical sophistication continued in Perry's (1991) landmark study of the certiorari process. Perry adopted concepts from Jervis's (1970) classic work on signaling in international relations, including the distinction between *signals*, which are manipulable, and *indices*, which are not. Nonetheless, Perry did not address many of the issues that occupy center stage in game-theoretic accounts of signaling.

Meaning and credibility in certiorari signaling. Contemporary signaling theory revolves around two issues: meaning and credibility (Banks 1991; Farrell 1993). With respect to meaning, the key issues are the identity of the signal sender, the nature of his or her private information, and the interpretation of the message by the signal receiver. In the context of certiorari, the obvious questions are: Who is the signaler? What does the signaler know that the Supreme Court does not? What signals might reflect this private information? How can the Court understand or interpret the signal? With respect to credibility, the key issue is sometimes called "the Jervis paradox": If sending a particular message benefits a signaler with private information, then why do not others without the information do the same thing? If they do, then why does this not destroy the credibility of the message? In the context of certiorari, if a lower court that follows Supreme Court doctrine can signal its conformance by sending a particular message, then why do not courts who flout doctrine send the same signal? If they do, how can the signal from the first court credibly convey the intended message: "There is no need to review me since I am conforming to your doctrine"? The contemporary theory of signaling games provides powerful tools for answering these questions.

A JUDICIAL SIGNALING GAME

The Model

Our model shares some similarities with other auditing models in the social sciences, such as the importance of mixed strategy equilibria, but its structure differs from most others in three ways: It includes a publicly observable index, the message space for the signaler is restricted to two actions, and both actors agree about the disposition of some cases even when they differ strongly about others. Consequently, the equilibria in our model also differ in some regards from most others (for a review of those models and their characteristics, see Andreoni, Erard, and Feinstein 1998).

The players are higher court H and lower court L. Play of the game determines the legal disposition of a search-and-seizure case. A "case" is a set of facts (i.e., a point in a fact space). Each point in the fact space corresponds to a degree of intrusiveness of the search. That is, points in the fact space map into the real line X, where each point x indicates a degree of intrusiveness. Each case thus corresponds to a point x in X.

² Overall, 23.3% of the search-and-seizure cases were affirmed by the Burger Court (U.S. Supreme Court database, using orally argued citation and split vote as the unit of analysis). Some of these affirmations may be consistent with our model. That is, because of differences between the observed and actual intrusiveness of the search, the Court may take a case that seems suspect only to find on further review that it was correctly decided.

³ Cue theory aside, strategic analyses of certiorari date to Schubert 1959; also see Brenner 1979.

Some of the facts in a case are publicly observable, others are not. For example, it is easily determined whether a search took place in a car or home, or whether a warrant was issued; whether the police had "probable cause" for the search is a more difficult determination that can only be reached after careful review of evidence and testimony.

Let $x = \hat{x} + t$, where \hat{x} is the intrusiveness of the case based on the publicly observed facts, and t is an additional increment or decrement of intrusiveness based on the nonpublicly observable facts. We assume $t \in T = X$ and is distributed according to distribution F, with common knowledge density f assumed everywhere continuous, differentiable, and nonzero. We assume throughout that F is log concave (displays the monotone hazard rate property). This condition is met by most common probability distributions, including the normal, logistic, chi-squared, exponential, and Laplace (Bagnoli and Bergstrom 1989). We further assume the density f(t) is independent of the realization of \hat{x} .

Sequence of Play, Information Structure, and Strategies. The lower court hears the case, learns all the facts in the matter (i.e., it learns \hat{x} and t and hence x), and either admits or excludes the evidence. The higher court sees only the publicly observable facts (i.e., it learns \hat{x} but not t) and the decision of the lower court. H may deny certiorari (i.e., decline to review the case), and L's decision stands. Or, at cost k, H may review the case, learn the private information t, and affirm or reverse L.

Given this information structure, the action of L becomes a "signal" about its private information, exactly as noted by Songer (1979), while the publicly observable degree of intrusiveness becomes an "index" in Perry's terminology.

A strategy for L is a function

$$s: T \times X \to \Delta(M)$$
,

where $\Delta(\cdot)$ denotes the set of probability distributions over a finite set, and $M = \{m_1, m_2\}$, with $m_1 = \text{exclude}$ and $m_2 = \text{admit}$. Thus, $s(t; \hat{x})$ gives the probability of excluding the evidence given the private information t and the public information \hat{x} .

If H hears the case it will affirm or reverse L in such a way that the evidence is admitted or excluded according to its own preferred doctrine. Hence, we can take H's strategy as simply a reviewing or auditing strategy:

$$r: M \times X \to \Delta(A)$$
,

where $A = \{a_1, a_2\}$, with $a_1 =$ grant certiorari and $a_2 =$ deny certiorari. Thus, $r(m_i; \hat{x})$ is the probability of granting certiorari given action m_i by L and the observable degree of intrusiveness \hat{x} .

Preferences. Both courts have preferences defined partly in relation to the ultimate disposition of the case. Each wishes the case to be adjudicated correctly, given its own notions of justice. More specifically, each has a preferred legal rule, which uses a cut-point x_i in X, i = 1

H, L. Court i believes the evidence should be excluded in cases that are more intrusive than x_i and admitted in cases less intrusive than x_i . This captures the notion of a legal rule or doctrine and the "correct" versus "incorrect" dispositions of cases, concepts of critical importance in the judicial setting. Each court wishes to decide cases correctly in view of its preferred legal rule. Note, however, that the two courts differ in the value of the ideal doctrinal cut-point. The reason courts prefer different legal rules is not modeled here, but it could result, for example, from different perceptions of the social costs and benefits of curbing the police.

The unidimensional space X is exactly the "attitude" space of the "attitudinal model" assumed in many empirical studies of judicial decision making (Segal and Spaeth 1993). The ideal cut-point partitions the space in precisely the fashion noted in attitudinal studies. This partitioning, combined with the mapping from the *n*-dimensional fact space into the unidimensional attitude space, in turn partitions the fact space in exactly the way required by the concept of a judicial doctrine. Conveniently, however, most of the action of the model takes place in the simple attitude space.

To complete the specification of the doctrinal component of preferences, we normalize each player's payoffs. If a case is decided correctly from its perspective, then the court receives a payoff of 0; if incorrectly, the payoff is -1. This stylization reflects the different value of a correct versus an incorrect decision to the courts while simplifying the development and exposition of the model. In the interest of brevity and clarity in presentation, we take $x_L < x_H$ so that H is more conservative than L; results concerning the other, completely symmetrical, case are noted where appropriate.

In addition to preferences about outcomes, L prefers not to be reversed. If reversed, it suffers an $\epsilon > 0$ utility loss; this loss reflects the influence of judicial culture. We do not try to account for the origin or maintenance of this loss, although one can view it as sustained through repeated play within the legal community. Finally, the higher court loses $k \in (0,1)$ if it hears the case. This auditing cost reflects the time and effort of hearing the case, which involves a cost in other cases unheard and in leisure and other activities forgone. Given the normalization of the payoffs, ϵ and k can be seen as the utility losses from reversal and auditing relative to the utility loss of an incorrectly decided case.

$$= \begin{cases} 0 \text{ if } & \begin{cases} x < x_H \text{ and } m = \text{ admit and } a = \text{ deny} \\ x \ge x_H \text{ and } m = \text{ exclude and } a = \text{ deny} \end{cases}$$

$$= \begin{cases} x < x_H \text{ and } m = \text{ exclude and } a = \text{ deny} \\ x \ge x_H \text{ and } m = \text{ admit and } a = \text{ deny} \end{cases}$$

$$-k \text{ if } a = \text{ grant}$$

⁴ We bound the auditing cost by 1; otherwise, the higher court would never find it profitable to review cases (since the most it can gain as a result, exclusive of the auditing cost, is 1).

and for the lower court:

 $\begin{cases} x < x_L \text{ and } m = \text{admit} \\ x_L \le x < x_H \text{ and } m = \text{exclude and } a = \text{deny} \\ x \ge x_H \text{ and } m = \text{exclude} \end{cases}$ $\begin{cases} x < x_L \text{ and } m = \text{exclude and } a = \text{deny} \\ x_L \le x < x_H \text{ and } m = \text{admit} \\ x \ge x_H \text{ and } m = \text{admit and } a = \text{deny} \end{cases}$ $-1 - \epsilon \text{ if } x_L \le x < x_H \text{ and } m = \text{exclude and } a = \text{grant}$ $-\epsilon \text{ if } \begin{cases} x < x_L \text{ and } m = \text{exclude and } a = \text{grant} \\ x \ge x_H \text{ and } m = \text{admit and } a = \text{grant} \end{cases}$

Expected Utilities and Best Responses. The expected utility for the lower court from strategy $s(t; \hat{x})$ follows straightforwardly:

$$u_{L}(s(t; \hat{x}))$$

$$= \begin{cases} s(t; \hat{x})[r(m_{1}; \hat{x})(1 - \varepsilon) - 1] & \text{if } x < x_{L} \\ s(t; \hat{x})[1 - r(m_{1}; \hat{x})(1 + \varepsilon)] - 1 & \text{if } x_{L} \le x < x_{H} \\ (1 - s(t; \hat{x}))[r(m_{2}; \hat{x})(1 - \varepsilon) - 1] & \text{if } x \ge x_{H}. \end{cases}$$

L's best-response correspondence is immediate:

$$BR_L(r(m; \hat{x}), t; \hat{x})$$

$$s(t; \hat{x}) = 0 \quad \text{if} \begin{cases} x < x_L \\ x_L \le x < x_H \text{ and } r(m_1; \hat{x}) > \rho \end{cases}$$

$$= \begin{cases} s(t; \hat{x}) \in [0, 1] & \text{if } x_L \le x < x_H \text{ and } r(m_1; \hat{x}) = \rho \end{cases}$$

$$s(t; \hat{x}) = 1 \quad \text{if} \begin{cases} x \ge x_H \\ x_L \le x < x_H \text{ and } r(m_1; \hat{x}) < \rho, \end{cases}$$

where $\rho = 1/(1 + \epsilon)$.

L's best response correspondence indicates there are three strategically distinct regions in X: the region below x_L , the region between x_L (inclusive) and x_H , and the region above x_H (inclusive). If x falls into the first region, then L always admits the evidence; if x falls into the third region, then L always excludes the evidence; if x falls into the second region, the "conflict region," then L admits or excludes the evidence depending on whether the probability of certiorari (given exclusion and the observable facts) falls above or below a critical value ρ . If x falls into the conflict region and the probability of certiorari equals ρ , then L is indifferent between admitting and excluding the evidence and may randomize between the two pure strategies in any fashion.

This best-response correspondence is quite intuitive. The interests of the two players correspond when x is very high or very low (i.e., at or above H's ideal cut-point or below L's). Therefore, when x falls into those regions L can pursue her ideal policy without fear of reversal, yielding L a best response that is invariant to H's strategy. When x falls into the conflict region, the interests of the players are opposed. In this

case, a sufficiently imposing chance of reversal will keep L from pursuing her ideal policy. For cases that fall into the conflict region, the role of reversal costs is critical; as the cost of reversal falls to zero, the probability of an audit must go to one if the lower court is to be dissuaded from implementing its own preferred doctrine.

Finally, note that for any set of observable facts \hat{x} it is possible for L to have the best response "admit" or "exclude" (since by assumption $F(t_a) > 0$ and $1 - F(t_b) > 0$, where $t_a = x_L - \hat{x}$ and $t_b = x_H - \hat{x}$). This has an important implication: All information sets can be reached in equilibrium, so the need to determine beliefs at unreached information sets does not arise.

Let $\mu(x; m_i, \hat{x})$ denote H's beliefs about x given the observable facts \hat{x} , L's action m_i , and L's strategy. For example, $\mu(x < x_H; admit, \hat{x}')$ indicates the probability H puts on x lying below x_H after observing "admit" in a case with observable obtrusiveness \hat{x}' . The following fact is important: Given L's best-response correspondence, H's beliefs about x must be concentrated on the region below x_H after having observed "admit," regardless of the value of \hat{x} . Therefore, after observing "admit," the expected utility to H of review strategy $r(admit, \hat{x})$ must be $-r(\cdot)k$. This expression is maximized at $r(admit, \hat{x}) = 0$ for all \hat{x} . Accordingly, in any equilibrium, $r(admit, \hat{x}) = 0$ for all \hat{x} .

In addition, given L's best-response correspondence, H's belief about x must be concentrated above x_L following the observation of "exclude," regardless of the value of \hat{x} . Recall that the payoff to H from denying certiorari given exclusion is zero if $x \ge x_H$ but -1 if $x < x_H$. Therefore, following the observation of "exclude," the expected utility to H of review strategy $r(exclude, \hat{x})$ is for all \hat{x}

$$-r(exclude, \hat{x})k - (1 - r(exclude, \hat{x}))\mu(x_L \le x$$

$$< x_H, exclude, \hat{x}), \tag{1}$$

where, from Bayes's rule,

$$\mu(x_L \le x < x_H, exclude, \hat{x})$$

$$= \frac{\int_{t_a}^{t_b} s(\hat{x} + t) f(t) dt}{1 - F(t_b) + \int_{t_a}^{t_b} s(\hat{x} + t) f(t) dt}$$

(recall that $x_L = \hat{x} + t_a$ and $x_H = \hat{x} + t_b$). The solution to maximizing expression 1 with respect to $r(exclude, \hat{x})$ depends on the relative magnitudes of k and $\mu(x_L \le x < x_H, exclude, \hat{x})$. Thus, in any equilibrium, for all \hat{x}

$$r(exclude; \hat{x}) = \begin{cases} 1 \text{ if } \mu(x_L \le x < x_H) > k \\ \alpha \in [0, 1] \text{ if } \mu(x_L \le x < x_H) = k \\ 0 \text{ if } \mu(x_L \le x < x_H) < k. \end{cases}$$
(2)

Equilibria

We seek perfect Bayesian equilibria (PBE) to the judicial signaling game. In doing so it is convenient to consider a level of \hat{x} , \hat{x}^* , such that H's posterior belief about the true location of the case, given observed \hat{x} and "exclude" by L, leave him just indifferent between granting and denying certiorari, even if L definitely excludes the evidence for all cases truly lying in the conflict region (i.e., even if $s(t,\hat{x}) = 1 \ \forall \ x \in [x_L, x_H)$). Using the middle part of equation 2 and Bayes's rule, \hat{x}^* is implicitly defined as the level of \hat{x} such that

$$\frac{F(t_b) - F(t_a)}{1 - F(t_a)} = k. \tag{3}$$

Note that \hat{x}^* may not exist if k is sufficiently large, that is, if the left-hand quantity in equation 3 is less than k for all values of \hat{x} . If \hat{x}^* does exist, then the assumption of log concavity of $F(\cdot)$ assures that it will be unique.

Proposition 1.

1. If x* does not exist, then the following is a unique PRE:

$$\mathbf{s}(\mathbf{t}, \mathbf{\hat{x}}) = \begin{cases} 0 & \text{if } \mathbf{x} < \mathbf{x}_{L} \\ 1 & \text{otherwise} \end{cases}$$
$$\mathbf{r}(\mathbf{m}, \mathbf{\hat{x}}) = 0 \quad \forall \mathbf{m} \text{ and } \forall \mathbf{\hat{x}}$$

and beliefs are determined everywhere by Bayes's rule.

2. If \(\frac{1}{2} \)* exists, then the PBE are characterized by:

$$s^*(t, \hat{x})$$

$$= \begin{cases} 1 & \text{if } \begin{cases} x \geq x_H \\ x \geq x_L & \text{and } \hat{x} \geq \hat{x}^* \end{cases}$$

$$= \begin{cases} \beta(t, \hat{x}) \in [0, 1] \text{ such that } \mu(x_L \leq x < x_H, \text{ exclude,}) \\ \hat{x}) = k & \text{if } x_L \leq x < x_H \text{ and } \hat{x} < \hat{x}^* \end{cases}$$

$$0 & \text{if } x < x_L \end{cases}$$

$$\mathbf{r}^*(\mathbf{m}_{\upsilon}, \mathbf{\hat{x}}) = \begin{cases} \frac{1}{1+\varepsilon} & \text{if } \mathbf{\hat{x}} < \mathbf{\hat{x}}^* \text{ and } \mathbf{m}_{\iota} = \text{exclude} \\ 0 & \text{if } \begin{cases} \mathbf{\hat{x}} \ge \mathbf{\hat{x}}^* \text{ and } \mathbf{m}_{\iota} = \text{exclude} \\ \mathbf{m}_{\iota} = \text{admit} \end{cases}$$

and beliefs are determined everywhere by Bayes's rule.5

Part 2 of the proposition allows a great deal of latitude in admissable strategies for L. The latitude arises for cases whose observable intrusiveness is below \mathfrak{X}^* and whose true location lies within the conflict region. Hence, part 2 describes many equilibria. In all these equilibria, however, H's posterior belief that x lies in the conflict region, after observing "exclude" and \mathfrak{X} , must exactly equal k. In this sense, all of L's strategies characterized in part 2 can be said to be

"belief equivalent." All would be observationally equivalent to an outside observer not privy to t.

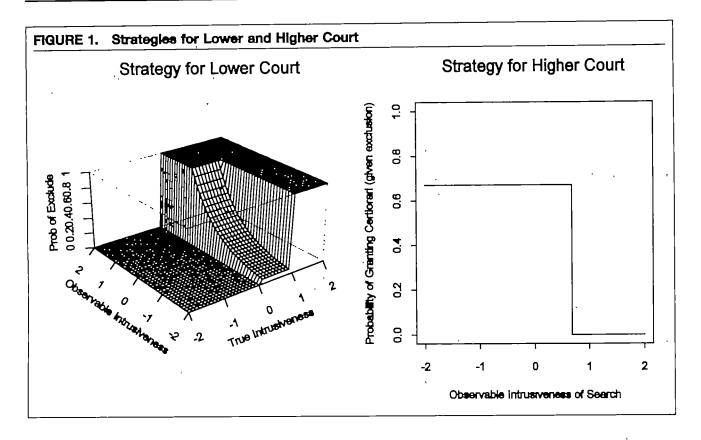
Example. Perhaps the best way to illustrate the review equilibria in part 2 of the proposition is through an example. Suppose $x_L = 0$, $x_H = 1$, $k = \frac{1}{2}$, $\epsilon = \frac{1}{2}$, and f is a normal distribution with mean zero and standard deviation 1. In this example, if $\hat{x} < \hat{x}^*$, then $s(t, \hat{x}) = s(t', \hat{x}) \forall x \in [x_L, x_H)$. In other words, if a case lies in the conflict region and $\hat{x} \leq \hat{x}^*$, then L keys the probability of excluding the evidence only on the case's observed intrusiveness.

Consider the strategy of the lower court, shown in the left-hand panel of Figure 1. If $x < x_L$ or $x \ge x_H$, then the lower court has obvious actions regardless of the value of \hat{x} . When $x < x_L$, the lower court admits the evidence; when $x \ge x_H$, the lower court definitely excludes the evidence. It can do so because even if audited it will not be reversed. If x falls into the conflict region, then L's strategy depends on the value of \hat{x} , the publicly observable level of intrusiveness. When £ is sufficiently high, at or above the critical level \hat{x}^* (which can be calculated to be about .69 in this example), the high court will not review the case even given exclusion of the evidence, for reasons explained below. Given this, the lower court can implement its preferred doctrine and exclude the evidence. When x is lower than \hat{x}^* , the lower court uses a probabilistic strategy keyed to the observable facts. For instance, when $\hat{x} =$.3, the probability of excluding the evidence is about

Now consider the strategy of the higher court. If it observes the admission of the evidence, then it accepts the lower court's judgment. This follows from the fact that L is more liberal than H, so any evidence it finds admissible will surely be found admissible by H as well. The higher court has a more difficult decision if it observes the exclusion of the evidence (the right-hand panel of Figure 1). If the true situation is $x \ge x_H$, then H would like L's judgment to stand and would prefer not to review the case. But if $x < x_H$, then H would like to review the case and reverses L, even though granting certiorari entails cost k. Unfortunately for the higher court, it can only observe \hat{x} and the fact of exclusion. So, given the observable degree of intrusiveness, the two ideal doctrinal cut-points, the cost of auditing, and the probability distribution F(t), the higher court must decide whether it is worthwhile to check the lower court after observing "exclude." When \hat{x} is large enough (equal to or greater than \hat{x}^*), it will not be worthwhile; that is, the probability that x truly falls into the conflict region between the two doctrinal cut-points will not be large enough to warrant the audit. When $\hat{x} < \hat{x}^*$, the higher court's calculation goes the other way. When H observes "exclude" from a liberal lower court, it employs a probabilistic auditing strategy keyed to the value of ε . In this case, the probability of granting certiorari after observing "exclude" is 3/3.

These strategies of the two players together determine outcomes. For example, if the liberal lower court admits the evidence, then the higher court does not

⁵ Recall that the proposition assumes the higher court is more conservative than the lower court. There is a "mirror" proposition when the higher court is more liberal than the lower court.



grant certiorari, and the lower court's decision stands. If the lower court excludes the evidence and $\hat{x} < .69$, then the higher court grants certiorari with a 3/3 chance. In the event of certiorari, if x actually falls in the conflict region, then H reverses L; if it does not fall in that region, then H affirms. (Note that the model predicts some grants and affirms as well as grants and reversals.) For instance, when x falls into the conflict region and $\hat{x} = .2$, L admits the evidence with a 42% chance and excludes it with a 58% chance; if it excludes, then H denies certiorari with a 33% chance, so the judgment stands, and grants certiorari with a 67% chance; if it so grants, then H reverses L. Finally, when $\hat{x} \geq .69$, L decides the case according to its own preferred doctrine, and H always denies certiorari, even if L excluded the evidence.

Meaning and Credibility. How do meaning and credibility play out in the model? The signals "admit" or "exclude" are inherently ambiguous, since x lies on a continuum, but the messages are dichotomous (i.e., the most informative equilibrium can only be a partial pooling equilibrium). Because L is liberal, however, the meaning of the signal "admit" (i.e., $x < x_H$) is sufficiently clear to make H's decision easy, provided the message is credible; indeed, the message is perfectly credible when L is more liberal than H. The signal "exclude" from L is also ambiguous, with more serious ramifications for H. A liberal lower court always (weakly) prefers the higher court to interpret "exclude" as meaning $x \ge x_H$, but this interpretation cannot be inherently credible because of the potential tension between the two courts. Instead, H uses an index, the observable degree of intrusiveness, to judge

the message credibility. If the observable facts indicate intrusiveness above the critical threshold \hat{x}^* , then the interpretation $x > x_H$ is sufficiently credible to warrant accepting L's decision. If not, then H will occasionally audit L. The audit rate will be keyed to the lower court's sensitivity to reversals, and sensitive courts will be audited less frequently than insensitive ones.

Hypotheses about Review

Because the model treats certiorari as part of an interactive incentive system, it generates hypotheses not only about the probability of review but also about the exclusion of evidence in the lower court. Both types of hypotheses can be used to test the model; because our primary interest is the certiorari decision, we focus on the former.

The first two hypotheses follow straightforwardly from the propositions but make explicit the consequences of a lower court that is more conservative than the higher court.

Hypothesis 1 (liberal admission or conservative exclusion). The probability the higher court grants certiorari is zero if $\mathbf{x}_L < \mathbf{x}_H$ and the lower court admits the evidence, or $\mathbf{x}_L > \mathbf{x}_H$ and the lower court excludes the evidence.

HYPOTHESIS 2 (liberal exclusion or conservative admission). (a) If $\mathbf{x}_L < \mathbf{x}_H$ and the lower court excludes the evidence, then the probability the higher court grants certiorari is zero only if the search is sufficiently intrusive $(\hat{\mathbf{x}} \geq \hat{\mathbf{x}}^*)$; (b) if $\mathbf{x}_L > \mathbf{x}_H$ and the lower court admits the evidence, then the probability the higher court

grants certiorari is zero only if the search is sufficiently unintrusive $(\hat{x} < \hat{x}^*)$.

A striking feature of these hypotheses, which are new to the literature, is the way the higher court's behavior depends on the lower court preferences relative to the higher court. The asymmetry of H's response to, say, "admit" arises because the meaning of the signal depends on the nature of the sender (a liberal or conservative lower court). These propositions—particularly the first, which might be called the Nixon goes to China proposition—are strongly reminiscent of Calvert's (1985) work on the value of information from a biased source ("if a cold warrior like Nixon goes to China it must be time for a change in American policy").

Now consider two circuit courts, both more liberal than the Supreme Court but one less liberal than the other. How does the critical value of the index \mathfrak{X}^* , the level of intrusiveness above which the Supreme Court does not review even following "exclude," differ for the two courts? This question is answered by the third hypothesis.

HYPOTHESIS 3 (proximity of lower court). Consider two lower courts. (a) If $\mathbf{x_L} < \mathbf{x_H}$ for both lower courts, then (i) the critical value $\mathbf{\hat{x}^*}$ is lower for the less liberal lower court, and (ii) the probability of review is (weakly) lower for that court given the same observable facts and actions. (b) If $\mathbf{x_L} > \mathbf{x_H}$ for both lower courts, then (i) the critical value $\mathbf{\hat{x}^*}$ is lower for the more conservative lower court, and (ii) the probability of review is (weakly) greater for that court given the same observable facts and actions.

Hypothesis 3 indicates that moving a lower court closer to the higher court expands the region in which cases escape review, a sensible result. Hypothesis 3 also indicates that if one estimates the probability of review as a function of observable facts and the ideological distance between the higher and lower court, then the coefficient on the distance between the two courts' ideologies should be positive. This hypothesis is new to the literature.

Hypothesis 4 (change in high court's preferences). (a) As the court becomes more conservative or more liberal, \hat{x}* moves in the same direction (e.g., if \hat{x}_H increases, then \hat{x}* increases). Consequently, (b) as the higher court becomes more conservative, cases from liberal lower courts become (weakly) more likely to be reviewed (controlling for observable facts), and cases from conservative lower courts become (weakly) less likely to be reviewed.

Hypothesis 4 is closely related to hypothesis 3 and also seems quite sensible, although it is new to the literature.

One of the most established facts about certiorari is that amicus briefs and the involvement of the Solicitor General greatly increase the probability the Supreme Court will accept a case. A complete analysis of those two factors would require modeling third-party signaling and is beyond our scope here. In a somewhat similar model, however, Banks and Weingast (1992) note that third-party signals can be seen as reducing auditing costs. The following hypothesis considers the effects of changes in the cost of review.

Hypothesis 5 (change in the cost of review). A decrease in the cost of review (a) increases x^* if $x_L < x_H$ and decreases x^* if $x_L > x_H$. Consequently, (b) the probability of review (weakly) increases, controlling for observable levels of intrusiveness.

This hypothesis offers one reason amicus briefs or involvement of the Solicitor General may increase the probability of certiorari: They reduce information costs for the Supreme Court.

Hypothesis 5 and the no-review equilibrium in part 1 of proposition 1 have an interesting implication. When the Supreme Court has little interest in a technically demanding area (such as admiralty or patent law), the value of k will be large, and the Court will largely or entirely abandon that area. For instance, during the last 50 years the Court has only rarely heard admiralty cases, once a substantial portion of its caseload. Conversely, when the Court has a substantial interest in a less technically demanding area (e.g., civil liberties), hypothesis 5 suggests it is likely to hear many such cases. The topic seems to act as a "cue" for the Court, though in fact the causal mechanism is not so much signaling as the magnitude of information costs relative to the possible gains from review. The importance of this type of cue is a recurrent finding in the literature.

EMPIRICAL ANALYSIS

Data

The data for this analysis were assembled as follows. As part of a larger project on circuit court decision making, we drew a random sample of U.S. Courts of Appeals search-and-seizure opinions (including per curiams) published in the Federal Reporter from 1961 through 1990. We determined the universe of these cases by searching Westlaw (www.westlaw.com) for all "searches and seizures," all Fourth Amendment cases, and criminal law topics 219, 226, 364, 394, and 207, plus relevant combinations of keywords. From this universe we then drew a random sample stratified by year, consisting of 40 cases per year. Any that were not in fact search-and-seizure cases were discarded and replaced with the next case from the universe. For years with less than 40 cases, all published cases were included in the sample.

The original sample of more than 1,100 published appeals court decisions yielded only 18 cases reviewed

⁶ Referring to Figure 1, it may seem plausible to strengthen hypothesis 2 so that the probability of review, if nonzero, is a constant with respect to observed facts. Yet, such a strengthening requires the additional assumption that ε is not only uncorrelated with the distance between the courts but also identical across all lower courts. Whether the probability of review, when positive, varies with respect to observable facts can be seen as a test of this rather strong assumption.

by the Supreme Court. This is not surprising, given the rarity of certiorari. Therefore, the next step was to add all other appeals court search-and-seizure cases reviewed by the high court. Such a sample, known as a choice-based sample, is commonly used when the event of interest is rare. Special statistical techniques are required for such samples, to which we return shortly.

Each case in the final sample was coded in various ways: whether the appeals court upheld or struck down the search, whether the case was appealed to the Supreme Court, whether certiorari was granted, and so on. We also coded fact patterns for each case using the procedures employed by Segal (1984). The identity and votes of each judge on the appellate court panel were also recorded. Using the identity information, appeals court judges were matched with data on their personal background and characteristics (see Songer, Segal, and Cameron 1994). Characteristics were unavailable for some, since often district court judges and retired judges sit as the third member of appellate court panels. Personal characteristics were available for the lead judge in most of the cases, however.

Our model indicates five key variables. The dependent variable is whether certiorari was granted (Grant). The independent variables are the signal, that is, whether the lower court admitted the evidence (Admit); an index, the publicly observable intrusiveness of the search (Intrude); another index, the ideology of the lower court (II); and the ideology of the Supreme Court.

Using the case facts, we constructed the measure of the publicly observable intrusiveness of the search in the following way. Segal (1984) estimates the likelihood the Supreme Court will uphold a search, based on the fact pattern in cases and a proxy for the ideology of the Court. In particular, Segal estimates weights on each variable using a logit regression. Since the Court's decision to uphold or strike down search-and-seizure cases depends partly on the intrusiveness involved, the logit weights on case facts provide a convenient way to measure intrusiveness. More specifically, we construct the variable intrude as follows:

$$Int = -3.256 \times Incident - 1.049 \times Afterlaw + .06$$

$$\times Unlawful - 1.928 \times Warrant + 3.25 \times Home$$

$$+ 2.054 \times Person + 2.733 \times Business + 2.243$$

$$\times Car - 1.411 \times Except + 1.766 \times Extent.$$

The definitions for the case fact variables are identical to those of Segal (1984), but we reverse the sign on the variables, as we wish to measure intrusiveness rather than lack of it. We also do not employ the constant or Segal's proxy for Supreme Court ideology, since we wish to measure intrusiveness rather than probability of upholding or reversing. Furthermore, we do not

employ Segal's variable Probcaus, which indicates whether the lower court found probable cause for the search. We argue that, for the most part, findings about probable cause do not reflect publicly observable facts but the private information the lower court obtained through its scrutiny of the case. It is exactly in its determination of probable cause that a lower court can try to exploit its informational advantage to alter the outcome of a search-and-seizure case in its preferred direction. It is worth noting that lower court determinations of probable cause do not affect the Supreme Court's decision on the merits, according to Segal's analysis (i.e., the coefficient on this variable was not statistically different from zero). This makes perfect sense from an informational standpoint: Once the Supreme Court has heard a case, it can make its ruling based on its own knowledge of previously unobservable facts, not what the lower court says about them.

We created a measure of the doctrinal preferences of the lower court in a two-step fashion, following Songer, Segal, and Cameron (1994).8 First, we regressed the votes of the appellate judges on their background characteristics (e.g., whether they were appointed by a Republican president or were a prosecutor before becoming a judge). The logit weights from this regression were used to form an index of conservatism for each judge for whom background information was available (low numbers on the index indicate a presumptively liberal judge; higher numbers a more conservative one).9 This is obviously far from perfect as a measure of doctrinal preferences. Nonetheless, presumptively conservative judges, based on simple background characteristics, tend to vote more often to uphold searches relative to presumptively liberal judges (Songer, Segal, and Cameron 1994). Hence, the index has a degree of face validity. Thus, the first stage yields a measure of ideology for each judge on the three-judge appellate panel.

The formal model assumes a single measure of ideology for a lower court, an abstraction adopted to simplify an already complex model. In the empirical analysis, however, we have to consider how the Supreme Court may determine the ideology of the lower court judges. Several possibilities present themselves. Most obviously, the Court may cue off the ideology of the opinion writer (measured by our variable *J1*); we explore this possibility below. Or the Court may try to judge the general ideological tenor of the lower court, perhaps best reflected in the mean ideology of the judges in the majority. Alternatively, the Court may use its limited information very efficiently, cueing off the

See Brace, Hall, and Langer 1998 and Giles, Hettinger, and Peppers 1998 for similar uses of regression-based techniques to impute judicial ideologies.

⁷ The following equation provides the mapping from fact space to attitude space discussed in the previous section. Whether this simple linear mapping is adequate to capture important doctrinal subtleties is an interesting question, but Segal's work demonstrates its empirical power.

⁹ The logit is 353 region -311 appointing president +381 religion +189 prosecutor +152 judicial experience, defined as: judge's region (1 = South, 0 = non-South), the appointing president's ideology (1 = liberal, 0 = moderate, -1 = conservative [Tate and Handberg 1991]), the judge's religion (1 = Catholic, 0 = other), prosecutorial experience (1 = yes, 0 = no), and judicial experience (1 = yes, 0 = no). A simple index based on the number of presumptively conservative traits works almost as well as the multivariate regression in predicting votes of appellate judges.

most liberal judge in the majority in conservative decisions and the most conservative judge in the majority in liberal decisions. Information about a dissenting judge, if any, also may be useful. Data limitations prevent a detailed analysis of these possibilities.¹⁰

We control for the doctrinal preferences of the Supreme Court in a simple way: We restrict our attention to the last three natural courts (courts in which there are no personnel changes) of the Burger years (Burger Courts 3-5 in Table 5-2 of Epstein et al. 1997). That is, we examine cases from January 7, 1972, to September 25, 1986—from the addition to the Court of William Rehnquist up to the addition of Antonin Scalia—which yields 273 usable appealed cases. During this period, in the arena of criminal justice the dominant coalition on the Court was relatively stable and quite conservative. Moreover, the bulk of the appellate court panels that the high court faced were almost certainly more liberal than the Supreme Court, at least with respect to search and seizure.11 This makes the detection of relevant patterns in the data much easier.

Method

Hypotheses. The data allow us to address hypotheses 1-3 in the signaling model. Since the Burger Court was almost certainly as conservative, if not more so, in search-and-seizure cases as most of the lower court panels it faced (Blasi 1983), the model predicts a series of patterns that are neatly captured in the conditioning plot shown in Figure 2. Such plots are powerful devices for detecting interactions or (as is relevant in this case) patterns across statistical regimes (Cleveland 1993). The plot is read in the following way. Each panel shows the relationship between the probability of granting certiorari and the intrusiveness of the search, under a specific condition. In the top two panels, the lower court admitted the evidence. In the bottom two panels, the lower court excluded the evidence. In the left-hand panels (top and bottom); the judge who wrote the opinion for the lower court was quite liberal. In the right-hand panels, the judge who wrote the opinion for the lower court was conservative (but, recall, less conservative than the Supreme Court).

The signaling model predicts several patterns when the data are arranged in this fashion (Figure 2). The

¹⁰ Because data on district court and retired judges sitting by designation were not available, more than one-third of our cases would have to be excluded if we were to use a composite measure of panel ideology. By focusing on the opinion writer, we cut missing data to below 10%. Moreover, it is not unrealistic to imagine that the Supreme Court cues off the opinion writer, especially as the justices' certiorari memos typically highlight his or her name.

top two panels should show a zero probability of review at all levels of intrusiveness: If the lower court took the conservative action, then the Burger Court should not review it (from hypothesis 1). The prediction for the lower panels is more complicated. Refer to the step function in the right-hand panel of Figure 1. The signaling model predicts such a step function in each of the lower panels of Figure 2, but it should be located farther to the right in the left-hand panel than in the right-hand panel (hypothesis 3.a.i.), and it may be beyond any level of intrusiveness actually observed. So, at any value of intrusiveness, the probability of review should be no greater in the lower right-hand panel than the lower left-hand panel (hypothesis 3.a.2). Finally, in the lower panels, if the probability of granting certiorari approaches zero, it should do so only for high levels of intrusiveness (hypothesis 2).

Choice-Based Sampling. The estimated models need to reflect the fact that the sample is choice based. This is easily done by weighting the observations (see Manski and Lerman 1977; Greene 1991, Section 36.5, provides a simple example). In the initial sample, the proportion of appealed cases actually heard by the Supreme Court was 5%. These were subsequently oversampled, so that they account for about 27% of the final sample. Consequently, cases granted appeal are overrepresented by .27/.05 = 5.4, and cases not granted review are underrepresented by .73/.95 = .77. To account for this, the granted cases need to be weighted by the factor .05/.27 = .19, and the rejected cases need to be weighted by the factor .95/.73 = 1.3. In all the models presented below, including the scatter plot smoothers, this procedure was followed.

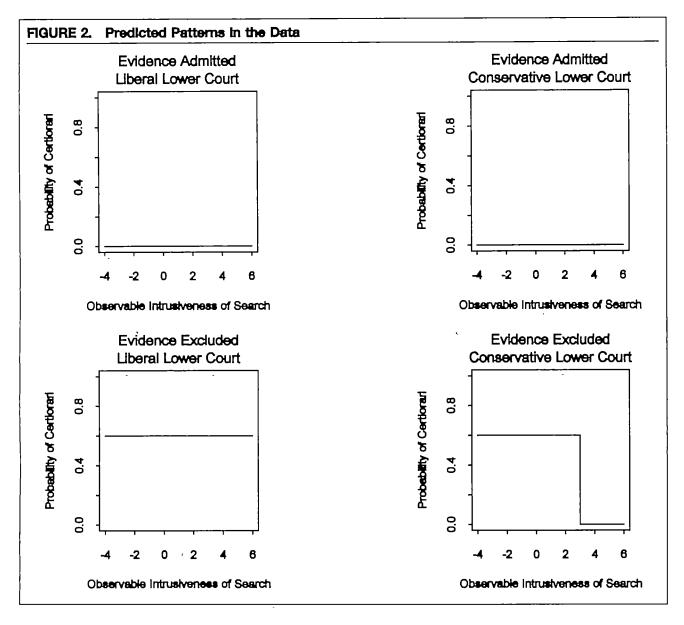
Data Display

Figure 3 displays all the data in the form of the critical conditioning plot. Liberal lower courts are defined as those whose ideology score (II) is at or below the median value (.152). To help uncover the systematic variation in the data without imposing any pattern ex ante, we include the fit from a nonparametric scatter plot smoother. The outstanding patterns in the data are clear, namely, the radical difference between cases in which the evidence was admitted (the top row) and those in which the evidence was excluded (the bottom row). In the former, the probability of review is zero, except at the very highest levels of intrusiveness, where it appears to increase slightly. In the latter, the probability of review is never near zero, except for very intrusive searches struck down by conservative courts.

Figure 3 uncovers another predicted pattern: The probability of granting certiorari as a function of intrusiveness is flat (or slightly increasing at high levels of intrusiveness) when the lower court admits the evidence; flat when liberal courts exclude the evidence;

¹¹ To test whether the Court's decisions remained stable under Chief Justice Burger, we added to the model dummy variables for each chief justice from Warren through Rehnquist (minus one for the excluded dummy) and an interaction for the presence of that chief justice and each passing term of the Court from 1962 through 1991. This, as we have previously shown, is the best measure of changing search-and-seizure doctrine between Courts. Nevertheless, if we look only within the Burger Court, we find that the coefficient for its interaction is 0.06, with a standard error of 0.05. Thus, we are reasonably confident in our assertion that under Burger, the Court's preferences remained stable.

¹² In each panel the smoothing curve is a locally weighted (loces) regression (span = 1) incorporating the Manski-Lerman weights. Other smoothers yield similar patterns, and the results are not particularly sensitive to the span of the regression. On these techniques, see Beck and Jackman 1998.



and strongly decreasing when conservative courts strike down a search. Finally, in the lower panels the estimated probabilities of granting certiorari—ranging from about 10% to more than 80%—are very high, in the context of known patterns about certiorari. For example, the overall probability of granting certiorari for search-and-seizure cases appealed to the Burger Court was 7%.¹³

Parametric Fitting: The Signaling Hypotheses

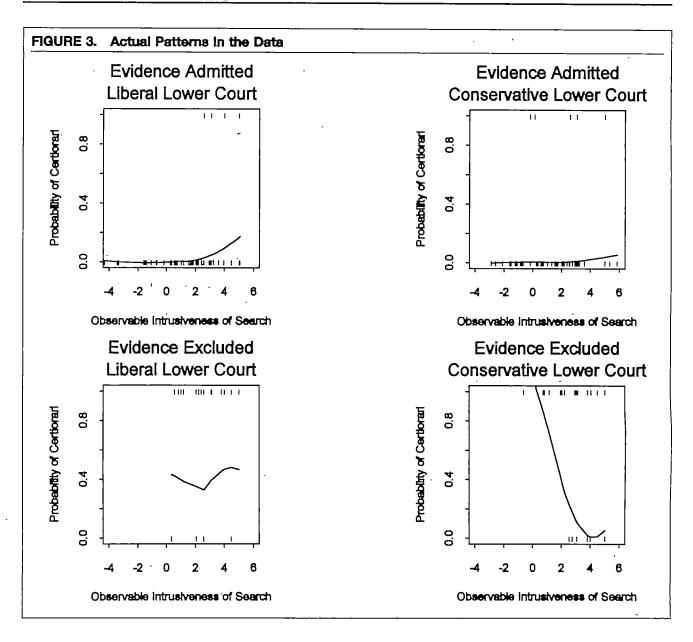
Bearing in mind that we model only one aspect of the certiorari process, Table 1 presents empirical tests of the theoretical model. As required by the theory, the statistical models are switching regime regressions (Goldfeld and Quandt 1973), which we estimate as logistic regressions. The regimes are the same shown in the critical conditioning plot in Figure 2. In models 1

and 2, the slope in the lower right-hand panel is estimated as a linear function of observed intrusiveness; in models 3 and 4 it is estimated as a step function.

In all four models, the coefficient on the variable admit is the intercept for cases in which the lower court admitted the evidence. Admit:intrude is the slope coefficient on the level of intrusiveness for the admitted cases. Across all four models, these variables have stable, statistically significant coefficients. It is straightforward to calculate the implied probabilities of granting certiorari, and these are shown in Figure 4 (using model 4). In the top two panels of that figure, the probability of the Supreme Court granting certiorari if the lower court admitted the evidence is essentially zero, except at the highest levels of intrusiveness, where it increases to about 15%.

The patterns predicted when the lower court excluded the evidence are more complex, and the models explore these patterns in some detail. In all four

Data compiled by the authors from U.S. Law Week.



models, the variable exclude is the intercept for cases in which the lower court excluded the evidence. In no model is this term statistically significantly different from zero, which indicates a baseline probability of granting certiorari for these cases of approximately 50%. Model 1 allows the slope on intrusiveness to vary linearly depending on the ideology of the lower court (as measured by JI, the ideology of the opinion writer). For liberal judges ($JI \leq .1$) the coefficient may take one value; for conservative ones (JI > .1) it may take another. As shown in Table 1, if the lower court was liberal, then the probability the Supreme Court would grant certiorari was not affected by the level of intrusiveness. But if the lower court was conservative, then the probability fell with the level of intrusiveness. ¹⁴

Model 2 is identical to model 1 but drops the statistically insignificant variable that allowed intrusiveness to affect the probability of certiorari for cases with evidence excluded by a liberal court. (The insignificance of this variable is compatible with the theory [see the lower left-hand panel in Figure 2].) Excluding this variable has only a very small effect on the overall fit of the model, as shown in Table 1 by the residual deviance. In model 2, the predicted behavior of the Supreme Court varies dramatically depending on the ideology of the lower court, given exclusion of the evidence. If the lower court was liberal $(J1 \le .1)$, then the predicted probability of granting certiorari was slightly more than 50% regardless of how intrusive the search. If the lower court was conservative (J1 > .1), then the probability declined dramatically with intru-

 $^{^{14}}$ Although our theoretical model explicitly calls for cut-points and regime changes, estimating the model with the original interval-level measurements for lower court ideology and the intrusiveness of the search does not change the results. Rerunning model 1 yields the following equation: .046 (1.08) - 6.56 (1.68) \times Upheld - 0.19

^(0.36) \times Intrude + 2.01 (2.23) \times Ideology + 1.13 (0.50) \times (Upheld \times Intrude) - 1.09 (0.71) \times (Ideology \times Intrude), with standard errors in parentheses.

Varlable Varlable	Model 1	Model 2	Model 3	Model 4
Admit	-5. 96	-5.96	-5.96	-5.96
	(1.27)	(1.27)	(1.27)	(1.27)
	(-4.7)	(-4.7)	(-4.7)	(-4.7)
Admitintrude	0.73	0.73	0.73	0.73
	(0.35)	(0.35)	(0.35)	(0.35)
	(2.1)	(2.1)	(2.1)	(2.1)
Exclude	0.38	0.08	0.14	-0.01
	(1.03)	(0.62)	(1.06)	(0.62)
	(0.4)	(0.1)	(0.1)	(-0.02)
(Exclude & liberal):Intrude	-0.14 (0.38) (-0.4)		0.07 (0.39) (0.2)	
(Exclude & conserv.):Intrude	-0.58 (0.36) (-1.6)	-0.49 (0.26) (-1.9)		
Step down (step at Intrude = 1)			-1.72 (1.25) (-1.4)	−1.57 (0.92) (−1.7)
Residual deviance,	. 61.8	61.9	62.9	62.9
Degrees of freedom	268	269	268	269

siveness, falling to less than 10% for very intrusive searches. 15

Models 3 and 4 more directly test the theoretical model by forcing the relationship in the lower right-hand panel of the conditioning plots in Figure 2 to take the form of a step function. We place the step at intrude = 1 (similar results obtain when the step is placed anywhere in the range 0-4). The step "down" from the intercept is given in Table 1. Model 3, like model 1, allows the probability of certiorari, given exclusion by a liberal lower court, to vary with the intrusiveness of the search. But as in model 1, this variable is not statistically significantly different from zero. Model 4 drops this term. Model 4 fits the data only negligibly worse than models 1 and 2 (residual deviance = 62.9).

The fit from model 4 is shown in Figure 4. As noted above, the predicted probability of granting certiorari for most upheld cases is zero and for liberal lower courts that excluded the evidence is about 50%. As shown in the lower left-hand panel of Figure 4, this constant probability can now be seen as the upper step in a step function. Among more conservative lower courts that excluded the evidence (the lower right-hand panel), the probability of review remains at about 50% for less intrusive searches (the upper step in the step function). For more intrusive searches, the probability falls to about 18% (the lower step in the step function).

Discussion

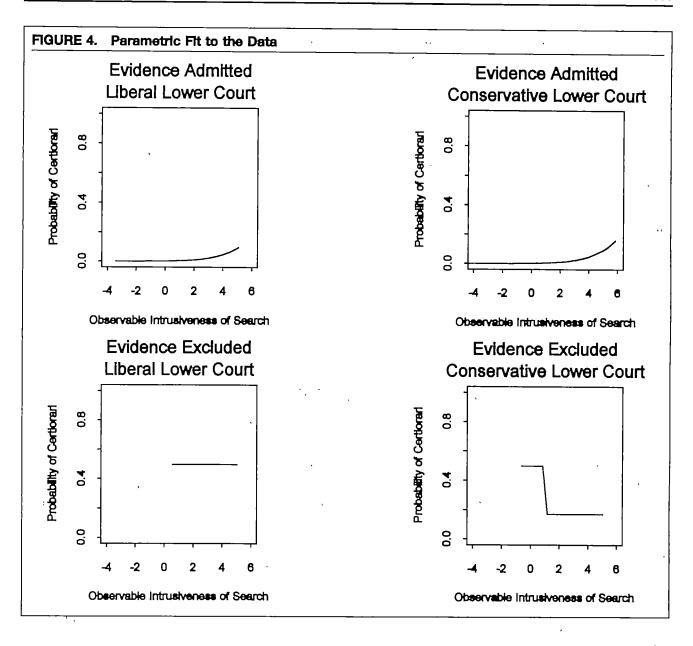
The patterns revealed by the statistical analysis and shown in Figure 4 strongly resemble those predicted by the signaling model. The dramatic difference between the top and bottom rows in the figure confirms the "Nixon goes to China principle." The more subtle predictions, involving the location of the step, are consistent with the lower panels.

Two discrepancies deserve brief discussion. First, among cases in which the lower court admitted the evidence, the probability of review rises slightly at the highest levels of intrusiveness. If some of the lower courts were more conservative than the Supreme Court, then the model predicts this should not completely escape review for very intrusive searches. The presence of some courts like this, or variables outside the model, could account for the slight increase in review probabilities at high levels of intrusiveness after admission of the evidence. Second, the lower step in the step function does not rest on zero but is located somewhat higher. If more liberal courts are mistakenly measured as less liberal, then this measurement error (as well as variables outside the model) could lead to an increase in the measured value for the lower step.

CONCLUSION

Our signaling theory of the Supreme Court's certiorari decisions was tested against a random sample of cases heard in the federal appellate courts. The theory is an incomplete view of the process: It emphasizes the role of certiorari in enforcing the doctrinal preferences of

¹⁵ The results are not sensitive to the exact location of the break between more liberal and less liberal lower courts, that is, the results are similar if the break falls anywhere between 0 and 3 on the ideology scale.



the Supreme Court throughout the judicial hierarchy but ignores its important role in the evolutionary creation of doctrine. The data are far from perfect. For example, we rely on fairly crude measures of judicial ideology and a relatively coarse coding of fact patterns in the cases. Despite these shortcomings, which ought to militate against finding patterns in the data, our analysis uncovers considerable support for the signaling theory as well as support for a variety of new propositions. Of course, we tested our model in only one area of decision making during the reign of one chief justice. Whether the data fit the model in other areas and during other regimes remains to be seen.

Our research makes three contributions. First, we use a formal model to advance our substantive understanding of certiorari. Most of the patterns uncovered here are new, despite the extensive research on the subject. We believe it unlikely that an analyst would uncover the patterns shown in figures 3 and 4 without a formal signaling model. Second, the research inte-

grates the study of the judicial hierarchy with the "new economics of organization" (Moe 1984). The application of formal principal-agent theory to the judicial hierarchy yields several benefits, both within the field of judicial politics and, more broadly, for the study of political hierarchies. Within the field of judicial politics, two phenomena, hitherto regarded as distinct, emerge as two components of a single underlying process that can be analyzed in a unified way. The two phenomena are judicial compliance (the responsiveness of lower courts to changes in the doctrinal directives of higher courts) and certiorari; both can be treated as interactive components in a political struggle over doctrine within the judicial hierarchy. From this perspective, the problem of doctrinal control in the judiciary is not sui generis but a particularly interesting case of an issue common to rule-governed hierarchies. To underscore the point, our model of strategic auditing, although tailored to the certiorari process, has broader applicability to other hierarchical settings in

which players have political preferences about rules. Third, the research provides empirical support for a formal model, which is still in relatively short supply.

APPENDIX

Proof of Proposition 1

Part 1 (2* does not exist). Note that the case deals with $\begin{array}{lll} F(t_b) - F(t_a)/1 - F(t_a) < k \ \forall \ \pounds. \ F(t_b) - F(t_a)/1 - F(t_b) > k \ \forall \ \pounds. \ \text{cannot arise because } \lim_{t \to \infty} F(t_b) - F(t_a)/1 - F(t_a) = 0. \ \text{This is true because } t_a, t_b \to -\infty \text{ as } \pounds \to \infty. \ \text{Thus, } F(t_a), F(t_b) \to 0 \text{ and } 1 - F(t_a) \to 1 \text{ as } \pounds \to \infty, \end{array}$ and the quotient goes to zero. For any nonzero k there must exist a sufficiently large \hat{x} so that $F(t_b) - F(t_a)/1 - F(t_a) < 0$ k. (A) Existence. If H does not review any cases, then the indicated strategy for L is clearly a best response. By assumption, $F(t_b) - F(t_a)/1 - F(t_a) < k \ \forall \ \hat{x} \ \text{when } s(t, \hat{x}) = 1 \ \forall \ x \in [x_L, x_H)$. Then, from equation 2, $r(exclude, \hat{x}) = 0$ $\forall \hat{x}$. That $r(admit; \hat{x}) = 0 \ \forall \hat{x}$ is discussed in the text. (B) Uniqueness. There are three cases to consider, none of which can be an equilibrium. (1) The indicated strategy for L, but H reviews at least one case with probability greater than zero. By construction, H can profitably deviate to the indicated strategy. (2) The indicated strategy for H, but L deviates in some fashion from deciding cases according to its preferred rule. Then there are cases where L can profitably deviate to the indicated strategy. (3) H reviews some cases with probability greater than zero, and L deviates from deciding cases according to its preferred rule for at least one case in the conflict region (i.e., $\exists x \in [x_L, x_H)$ s.t. $s(t, \hat{x}) < 1$) (deviations from the indicated strategy for L are never profitable for cases below x_L or above x_H [inclusive] and need not be considered further). By construction, the Bayesian posterior $\mu(x_L \le x \le x_H, exclude, \hat{x}) < k$ when $s(t, \hat{x}) = 1$ $\forall x \in [x_L, x_H)$. But $\mu(x_L \le x \le x_H, exclude, \hat{x})$ decreases when $s(t, \hat{x}) < 1$ for some x in the conflict region. Thus, $\mu(x_L \le x \le x_H, exclude, \hat{x})$ remains less than k when s(t, t) \hat{x}) < 1 for some x in the conflict region, and therefore H can profitably deviate to the strategy indicated in the proposition.

Part 2 ($\mathbf{\hat{x}}^*$ exists). By inspection, the strategies characterized in part 2 comprise best responses: $s^*(t, \hat{x})$ is compatible with $BR_L(r(m; \hat{x}), t; \hat{x})$, and $r^*(m_i, \hat{x})$ is compatible with equation 2 and the requirement that $r(m_2, \hat{x}) = 0 \ \forall \hat{x}$ (the latter is explained in the text). Q.E.D.

Proof of Hypotheses 1 and 2

Follows immediately from proposition 1. Q.E.D.

Proof of Hypothesis 3

(a) We consider the case in which $x_L < x_H$; via symmetry, similar arguments apply when $x_L > x_H$. Recall that \hat{x}^* is defined implicitly by equation 3. The partial derivative of equation 3 with respect to \hat{x}^* is

$$\frac{f(t_a)(1-F(t_b))-f(t_b)(1-F(t_a))}{(F(t_a)-F(t_b))^2}\frac{k}{1-k},$$

which will be nonzero provided

$$\frac{f(t_a)}{1-F(t_a)} \neq \frac{f(t_b)}{1-F(t_b)},$$

that is, the hazard rate at t_a is not equal to the hazard rate at t_b . A necessary condition for this is that $x_L \neq x_H$, which will also be sufficient provided F displays the monotone hazard

rate property (MHRP) (under MHRP, if t' > t, then the hazard rate at t' is greater than the hazard rate at t), as assumed. Then, via the implicit function theorem,

$$\frac{\mathrm{d} \hat{x}^*}{\mathrm{d} x_L} = -\frac{\mathrm{d} s}{\mathrm{d} x_L} \bigg/ \frac{\mathrm{d} s}{\mathrm{d} \hat{x}^*} = \frac{f(t_a)[1-F(t_b)]}{f(t_a)[1-F(t_b)]-f(t_b)[1-F(t_a)]} \,.$$

The numerator is positive. From the definitions of t_a and t_b , $x_H > x_L \Rightarrow t_b > t_a$, which, using MHRP, in turn implies the denominator is negative. (b) is obvious given (a). Q.E.D.

Proof of Hypothesis 4

The argument is similar to that of hypothesis 3. Again considering the case in which $x_L < x_H$, via the implicit function theorem,

$$\frac{\mathrm{d}\hat{x}^*}{\mathrm{d}x_H} = -\frac{\mathrm{d}s}{\mathrm{d}x_H} / \frac{\mathrm{d}s}{\mathrm{d}\hat{x}^*} = \frac{f(t_b)[1 - F(t_a)]}{f(t_b)[1 - F(t_a)] - f(t_b)[1 - F(t_b)]} > 0$$

using MHRP. The other case follows via symmetry. Q.E.D.

Proof of Hypothesis 5

As in hypotheses 4 and 5. For $x_L < x_H$

$$\frac{\partial \hat{x}^*}{\partial k} = -\frac{\partial s}{\partial k} / \frac{\partial s}{\partial \hat{x}^*}$$

$$= \frac{\left[1 - F(t_a)\right] \left[F(t_b) - F(t_a)\right]}{-f(t_a)\left[1 - F(t_b)\right] - f(t_b)\left[k(1 - k) + F(t_a)\right]} < 0,$$

as the denominator is negative (provided $0 < k \le 1$), and the numerator is positive. The other case follows from symmetry. Q.E.D.

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Presidential Pork: Executive Veto Power and Distributive Politics

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It is often argued that executive powers such as the veto serve to reduce particularistic spending by the legislature. I argue that the effect of the executive veto depends strongly on assumptions about both the nature of executive preferences and the institutional structure of executive-legislative bargaining. To support these claims, I develop a model of distributive policymaking subject to an executive veto. This framework incorporates variation in presidential objectives and formal powers into a dynamic bargaining model. In equilibrium, stronger veto power leads to a lower level of distributive spending, but the effects are mitigated to the extent that the president prefers spending in some districts over others. The model also generates new insights and predictions about fiscal policy under the separation of powers, including the effects of divided government, electoral rules, and term limitations for the executive.

major but largely open question in the study of American political institutions is the extent and nature of presidential influence in the legislative process. There is considerable debate as to both how much legislative power the president can assert and the processes and mechanisms by which it comes about. While it is often colorful and convenient to refer to the president as the legislator-in-chief or the third house of the legislature, there are obviously important differences between the roles of the legislators and the president.

First, there are large differences in electoral constituencies. Whereas legislators come from plurality elections in small districts, presidents are elected in national elections. A sequential set of primaries, diverse delegate selection rules, and the electoral college further complicate the president's coalition building. These differences certainly affect the preferences and goals that each brings to legislative bargaining. Second, the formal rules of the legislative process differentiate between legislators and presidents. Because presidents cannot formally introduce bills, their legislative powers such as the veto are primarily reactive. Any influence, therefore, usually must come through the legislature's strategic anticipation of the use of these prerogatives.

However important these distinctions between legislators and executives may be, very few models fully incorporate both issues. As I argue below, much of the literature makes rather stylized assumptions about the induced preferences of the president and the executive-legislative bargaining environment. In this article, I develop a new model of executive-legislative bargaining that allows both variation in the links between the president and his constituents as well as variation in his formal legislative powers. In combining both features, the model can address important issues concerning the relationships among presidential preferences, formal powers, and policy outcomes.

My model differs from previous work on the president's role in fiscal policy in two fundamental ways. First, it allows the chief executive to have preferences over the distribution of spending across districts. Rather than specify a particular source (electoral, legislative, or partisan) of these preferences, the model assumes only that the president wishes to maximize the net spending in a subset of districts, or his *constituency*.

Second, the model differs in the way it approaches the legislative bargaining process. Whereas much of the literature assumes that Congress makes a single take-it-or-leave-it offer to the president to alter an exogenous status quo or reversion, I relax this assumption by employing a sequential choice model of the legislative process and interbranch bargaining based on the work of Baron and Ferejohn (1989a), Harrington (1990), and Baron (1991). The sequential choice model developed here is an infinitely repeated, noncooperative game that focuses on the effects of presidential veto power on legislative proposals and voting. The model preserves the legislative monopoly over agenda control, but it allows the bargaining to take place over time rather than at a single moment. In the dynamic model, the agents no longer bargain in the shadow of an exogenous status quo but in view of endogenous expectations of future legislative decisions and presidential vetoes. These equilibrium expectations about proposals subsequent to presidential vetoes work to give the chief executive a greater degree of leverage than in a single-shot game.

I relax the common assumption that legislative proposals are generated from a single proposer, generally the median legislator. This assumption is often defended on grounds that the legislative decision-making process will generate median voter outcomes. In a purely distributive setting, however, median voter theorems carry little weight because the set of feasible allocations is multidimensional.²

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¹ A complete review of this literature would take us far afield. Book-length studies that focus on these questions include Bond and Fleisher 1990, Cameron n.d., and Edwards 1980.

² Significant work on the role of vetoes and supermajority override in multidimensional settings includes Hammond and Miller 1987 and Tsebelm 1994, which focus on the "core," that is, the set of policies that cannot be overturned by any decisive coalitions. Veto institutions are analyzed for their effects on the set of decisive coalitions and, therefore, the set of stable policies. Accordingly, this work concentrates on policy stability, legislative production, and "solving" majority rule cycling. The implications of executive veto power on

In the sequential choice model, these problems are handled by assuming that proposals are made sequentially according to a recognition rule. If the recognition rule contains some randomness, there will be uncertainty about which majority legislative coalitions will form subsequent to a presidential veto. This uncertainty gives the president a degree of leverage over the proposals of the current legislative majority.

REPRESENTATION, VETOES, AND PRESIDENTIAL INFLUENCE ON FISCAL POLICY

In previous models of the budgetary process, legislators often are assumed to behave so as to maximize net expenditure within their own electoral district. Such an assumption has a great deal of analytical power. Because it is easy to accept the link between budgetary success and the reelection of the incumbent, the analyst is free to proceed to more interesting questions, such as those regarding the collective ability of legislative bodies to control spending. When the role of the president in distributive politics has been studied, however, the reelection motive has been applied in exactly the same fashion: The president will seek to maximize the net benefits going to his electoral district. Since that district is the entire nation, the president is typically viewed as a counterweight to the distributive politics of the legislature (Carter and Schap 1987; Dearden and Husted 1990; Fitts and Inman 1992; Inman 1993; Lohmann and O'Halloran 1994). Therefore, it is assumed that the president uses his veto power (or other powers) to maximize the welfare of his entire constituency. In this literature, institutions such as the executive veto serve to thwart the tendencies of a legislature that is biased toward particularistic benefits for its members, at the expense of more general interests. Based on the assumption of presidential universalism, this literature puts forward strong normative arguments for increasing executive power in the legislative process. Among these proposals are the line-item veto and other reforms that move budgetary decision making from the legislative to the executive branch.3

Yet, there are reasons to be suspicious of the assumption of presidential universalism when applied to budgetary politics. It is not obvious that the simple strategy of ensuring budgetary efficiency accords as nicely with his reelection (or even history book) incen-

distributive politics has not yet been addressed. Banks and Duggan (1998) show that when a core exists, the stationary equilibrium to a sequential choice model is "close" to it, so that the game theoretic and the social choice frameworks are complementary.

tives as do other possible strategies. There are many reasons the president may not act to maximize the social welfare. The first and perhaps most important is electoral politics. Much of the previous literature assumes that the president is an agent of his entire constituency, but his electoral constituency is likely to be more important in determining his preferences over appropriation outcomes. In principle, he may try either to reward regions that provided support in his election or to direct resources to strategic "swing" regions to enhance his reelection prospects (or those of his party). Consistent with this view, Grier, McDonald, and Tollison (1995) find that the president's decision to veto a bill can be predicted by the votes of senators from electorally important states. Mebane and Wawro (1996) also find support for the proposition that the president specifically targets spending toward areas that are important for his reelection. These results suggest that the chief executive may use the veto and other perquisites of his office to favor some districts over others.

A second source of particularistic preferences of the president is the desire to direct distributive benefits to certain legislators in order to further his legislative agenda. It has long been recognized that the president often promotes his agenda by influencing individual legislators or "buying votes" (see Groseclose 1996; Groseclose and Snyder 1996; Snyder 1991). Discussions of such favor giving are usually limited, however, to the president's campaigning for members of Congress or Rose Garden photo opportunities. His role in allocating spending across districts has not received as much attention.⁴

Finally, the president may act on partisan motivations. As the leader of his party, he may feel pressure to favor legislative districts controlled by members of his own party. Empirically, this is borne out by Levitt and Snyder (1995), who find that districts represented by Democratic representatives fared much better in terms of federal spending during the Carter administration than they did during the Reagan years.

The problems associated with mischaracterizing presidential motives may be minimal if the institutional constraints are well specified. Yet, many rational choice models of the presidency use highly stylized representations of the legislative process, which may confound the problems alluded to above. In many treatments, the legislature is modeled as a monopoly agenda setter that proposes legislation subject to an executive veto (Carter and Schap 1987; Ferejohn and Shipan 1990; Kiewiet and McCubbins 1988). These models and others in the literature on agenda control typically predict only a limited role for the president in the process. Veto power in these models, to the extent that it influences legislative output at all, only permits the president to ensure that policies he desires less than the status quo are not enacted.

In terms of the budgetary process, Kiewiet and

³ Carter and Schap (1987) and Dearden and Husted (1990) demonstrate theoretically that, even when the president deares lower budgets than Congress, line-item vetoes may not lower expenditure below that associated with an all-or-nothing veto. This counterintuitive effect is the result of antiations where the line-item veto forces the legislature to increase expenditure on the executive's budget priorities more than enough to compensate for the reduction in spending on its priorities. This may explain weak empirical results about the effect of line-item vetoes in reducing expenditure in the states (Alm and Evers 1991, Holtz-Eakin 1988).

⁴ Lohmann and O'Halloran (1994) consider the effects of delegation to the president, who prefers to distribute favors to members of his own party.

McCubbins (1988) argue that the effects of the veto on the overall budget are asymmetrical. If the reversionary appropriations are lower than the desired budget of the legislature and the president, then the effect of veto power will be to lower spending only if the president prefers a lower budget than the legislature. It has no effect when the president prefers a larger budget than the legislature. By demonstrating the inability of the president to increase budgetary items, these theoretical results and the empirical support provided by Kiewiet and McCubbins are inconsistent with a large role of the executive in the budgetary process.5 Even models of asymmetric information, which may allow the president to achieve outcomes that he prefers to the status quo (Cameron and Elmes 1994; Matthews 1989; McCarty 1997), do not go very far in establishing the theoretical basis for a large role for the president in legislative decision making.

If there is a common implication of these behavioral and institutional assumptions, it is that they both predict the net effect of executive veto power is to lower expenditure. The behavioral assumption suggests that the executive will always prefer lower budgets, while the agenda control assumption produces lower spending because veto power will be ineffective in increasing it, except in rare circumstances.⁶

In the next section, I develop a model that captures the effects of variation in both the president's formal veto powers and his induced preferences over spending outcomes. The model finds that veto power constrains legislative spending, but the extent of this effect depends strongly on the president's preferences for universalistic outcomes over particularistic ones. Furthermore, the efficiency effects of the veto often come at the cost of spending inequities across districts.

A MODEL OF DISTRIBUTIVE POLITICS AND THE VETO

To focus on the role of executive veto power in altering the dynamics and outcomes of legislation on spending, consider an appropriations bill that distributes the benefits of a project with a total value of B among N districts. The tax cost of the project, T, will be distributed evenly among all districts so that each must pay T/N. A legislative proposal is simply a division of the benefits among the N districts, or $\mathbf{x} = \{x_1, \ldots, x_N\}$,

such that $\sum_{i=1}^{N} x_i \leq B$. The final allocation of these benefits will be determined by majority rule subject to an executive veto and supermajority override.

An important parameter in the analysis will be the benefit-cost ratio, R = B/T. Programs for which $R \ge 1$ will be called *efficient*; those for which R < 1 are termed *inefficient* because the benefits fail to exceed the costs. My evaluation of executive veto institutions will be based primarily on their effect on the passage of inefficient programs. Therefore, my results focus on the programs with the smallest benefit-cost ratio that are able to pass through the legislative process. This critical value will be referred to as the *minimally efficient* benefit-cost ratio, R^* . In addition to providing a measure of welfare, R^* is inversely related to spending levels because, for a given set of possible programs, fewer pass when R^* is high. As I demonstrate below, this value will be a function of both the president's veto power and the breadth of his coalition.

Rather than arbitrarily limit the opportunities for legislative consideration of this project, following Baron (1991), I assume that the legislature may consider the passage of the project for an infinite number of periods within a legislative session or term. I interpret this indefiniteness as a product of uncertainty about the time of the final period rather than as a truly infinite game. If the project does not pass in a given period, it may be taken up again in the future. Each legislator discounts future payoffs by 8, so that legislator i's payoffs are $8'u(x_i - T/N)$, where t is the time period in which the agreement passes, and $u(\cdot)$ is a utility function over net spending.

In order to investigate the role of the executive veto, the preferences of the president over the distribution of expenditure must be taken into account. I assume that he wishes to maximize the utility of his constituency, M, where m is the number of districts in M. Thus, the president wishes to maximize $\sum_{i \in M} \delta^i u(x_i - T/N)$. Because some concavity is necessary to give the president preferences over the distribution within his constituency, the model assumes that

$$u(r) = \begin{cases} \lambda r & \text{if } r \ge 0 \\ r & \text{if } r < 0 \end{cases}, \tag{1}$$

where $\lambda < 1$. Therefore, the marginal utility of spending is greatest when the district is receiving negative net benefits. This (weakly) decreasing marginal utility ensures that $u(\cdot)$ is concave.⁸ If the appropriation never passes, the payoff is u(0) = 0. The weak concavity of

⁵ Furthermore, any presidential influence on budgetary outcomes in the Kiewiet and McCubbins (1988) model depends crucially on the assumption that a continuing resolution at previous spending levels is enacted after a veto. If the relevant reversion is the statutory reversion of \$0, however, their model predicts that the budgetary outcome will only reflect legislative preferences. My model shows that the norm of continuing resolutions at previous spending levels is not a necessary condition for the executive to have influence over budgetary outcomes.

⁶ Conditions in the Kiewiet and McCubbins (1988) model imply that the veto power limits the ability of Congress to cut spending. This occurs when Congress wishes to reduce spending below the previous year's nominal level, but the president does not agree to the extent of the cuts. In the years studied by Kiewiet and McCubbins, due to inflation (which turns nominal freezes into real cuts) and the political climate for larger government, this scenario was rare. In the current climate, the situation is more plausible.

⁷ The choice of an infinite horizon rather than a long but finite horizon can be justified on several grounds. Osborne and Rubenstein (1994, 135) argue that "a model with an infinite horizon is appropriate if after each period the players believe that the game will continue for an additional period, while a model with a finite horizon is appropriate if the players clearly perceive a well-defined final period." Since my model focuses on bargaining over the course of a legislative term, uncertainty about the exact date of adjournment precludes such a well-defined final period. Furthermore, even if the exact end of the legislative term is known, it may be so distant in the horizon that it does not factor explicitly into the agents' calculations (Osborne and Rubenstein 1994).

Gilligan and Krehbiel (1995) use a similar utility function in the context of a sequential choice model.

 $u(\cdot)$ suggests that the president cares about the distribution of spending within his constituency, since his utility can be raised by taking a dollar from a constituency district receiving positive net benefits and giving it to one receiving negative net benefits. My model treats the choice of the constituency as exogenous in order to focus most of the attention on the implications of particularistic preferences regardless of their source.

As shown by Baron (1991), the possibilities for passage of the project and the distributional consequences are affected by the choice of legislative rules and recognition procedures.9 This analysis, however, considers the case of legislative bargaining under the closed rule with uniform random recognition, as proposed by Baron. In that case, proposer i is chosen with uniform probability 1/N in each period. 10 She subsequently makes a proposal. The legislature votes by majority rule, so that when N is odd, n = (N + 1)/2votes are needed for passage; if N is even, n = (N +2)/2 votes are required. The proposal is then submitted to the president for signature or veto. If he signs it, then the appropriations are made, each district is assessed an equal share of the taxes, and the game ends. If he vetoes it, the legislature may override with k > n votes.¹¹ In the event that the legislature is unable to override the veto, the session ends, and each legislator discounts the future at a rate of 8.12 An important consideration in deciding whether to vote for the proposal is the utility of defeating it and continuing to the next period. Let v_{ith} be the expected utility player i receives for defeating a proposal at time t with history of play h. This expected utility of the future will be referred to as the continuation value. These values will be determined in equilibrium.

Following Baron and Ferejohn (1989a), only stationary and symmetric strategies are considered. Stationary strategies are time and history independent.¹³ In this context, stationarity implies that proposals will only be

⁹ In fact, given that the model is an N-dimensional voting game, the mere existence of a solution will depend on there being at least minimal legislative structure to overcome the well-known voting cycle problem.

a function of the identity of the proposer as well as the basic parameters m, k, N, and δ . Symmetry is imposed so that proposers treat all legislators of the same type identically. Since the only ex ante differentiation of legislators in this model is based on membership in the president's constituency, the stationarity and symmetry assumptions suggest that strategies will depend only on whether the proposer is a member of the president's constituency. Thus, the continuation values, v_i , and optimal proposals of each legislator depend only on her constituency status. Let $v_i = v_p$ for members of the presidential constituency, and let $v_i = v$ for nonmembers. The stationarity assumption also implies that these are the ex ante expected utilities of the game. Much of the analysis will concern the relationship of v_p to v.14 In order to examine the role of the president, one must consider his continuation value, because it will determine his veto strategies. Stationarity and symmetry imply that the president's continuation value

Given a set of continuation values, voting and vetoing strategies are straightforward. A legislator (president) will reject (veto) any proposal for which the
utility is less than that person's discounted continuation
value. It is interesting to note that the continuation
values play the same role as the expected utility of the
reversion policy in other legislative bargaining models,
such as those based on Romer and Rosenthal (1978).
Unlike the reversion in most agenda control models,
however, continuation values are not exogenous; they
are derived from rational expectations of future legislative outcomes.

Given the strategies for all legislators and the president, consider the strategies of the proposers. A proposer must choose whether to make any proposal and, if so, how to allocate funds across districts. If she chooses to make a proposal, her problem is to formulate one that is capable of generating a coalition sufficient to pass the bill while keeping as much of the program's benefits as possible for herself. Let z_p and z be the net benefits retained by constituency and nonconstituency proposers, respectively. To maximize these benefits, she must choose the winning coalition that commands the smallest share of the benefits. Conditional on making a proposal, there are two types of strategy the proposer may employ. The bill must satisfy either the president and a simple majority of n members or a supermajority of k members. For simplicity, I refer to "n + the president" as the majority strategy and a coalition of k members as the override strategy. Since mixed strategies over these coalitions may be necessary (see Banks and Duggan 1998), let π_p and π be the probabilities that each type of proposer pursues the override strategy. If neither of these strategies leaves the proposer with enough benefits to compensate for her own taxes, a proposal will not be made. Such nonproposals occur when $B/T < R^*$, where this critical ratio will be determined by the parameters of the game.

¹⁰ While random recognition does not mimic any actual procedures of a legislature, it is a useful device for capturing the inherent uncertainty that legislators face in building distributive coalitions. Random recognition is a way of modeling the fact that legislators do not know exactly which coalitions will form in the future if the current coalition fails to enact the legislation. The assumption of uniform recognition probabilities is relaxed in a later section.

¹¹ Note that an override rule of $k \le n$ is superfluous, as any group of legislators large enough to pass the bill is also large enough to override. I will consider the case of an absolute veto for which no override is possible, that is, k > N.

¹² Baron and Ferejohn (1989a, 1989b) interpret 8 not only as the discounting of future costs and benefits but also as the probability that the legislator is returned to office. Since the bargaming in this model is assumed to take place within a legislative session, however, it is better to interpret 8 as a combination of time discounting and an exogenous probability that the legislative session terminates, rather than as a reelection probability.

¹³ Baron and Ferejohn (1989a) show that any division can be supported in nonstationary equilibria using infinitely nested punishment strategies. Baron and Kalai (1993) argue that stationarity is an attractive restriction because the fewest computations are required by the agents in the model, and therefore it adds an additional element of empirical plausibility.

¹⁴ I also take advantage of symmetry to suppress the superscript ℓ and stationarity to suppress the subscripts ℓ and h on x_{th}^{ℓ} and x_{th}^{ℓ} .

For my sequential choice model, a symmetric, stationary Nash equilibrium will constitute values of z_p , z, v, v_p , π , and π_p that are consistent with each player maximizing her utility, given the strategies of other players. I am particularly interested in the effect of veto rules and constituency size on the efficiency of programs that pass (R^*) and the distribution of expenditure across districts $(v_p$ and v). Is I will present these values for all the cases I discuss below. A more formal statement of the model and proofs of all the results are given in the Appendix.

Having described the model, I now discuss the results for the two important benchmark cases: no executive veto and a universalistic president.

No Executive Veto

In the case of no executive veto, presidential constituents and nonconstituents become ex ante identical because they have identical legislative prerogatives. Since all legislators have the same continuation values, a proposer can choose any simple majority from the legislature to receive benefits. A proposal will pass so long as the proposer can keep enough benefits after satisfying a majority to compensate for her district's share of the taxes. Column 1 of Table 1 presents the critical benefit-cost ratios and continuation values for this case, which is identical to the analysis of Baron (1991) for the closed rule in a majoritarian legislature. (The column headings and other material in Table 1 are explained as the cases are considered.)

As in Baron's analysis, R* defines a critical ratio of benefits to costs that must be exceeded in order for the program to be passed. As R* is bounded above by n/N < 1, some inefficient programs will be passed in addition to all the efficient programs. The inefficient programs pass because legislators will vote for those in which they receive negative net benefits since they rationally anticipate the possibility of receiving an even worse distribution of net benefits in future periods in which they are not selected by the proposer. Given these rational expectations, they vote for a bill that makes them worse off than they would be if the bill did not pass. An interesting implication of this result is the effect of the discount factor on R^* . The size of the minimally efficient program decreases as legislators weigh the future more heavily. In other words, the more they fear bad future outcomes, the more likely they are to accept bad current ones.

This case also has implications for the size of government when no executive veto is available. Given a fixed menu of possible government projects, more projects will pass as R^* diminishes. This will lead to more government spending than the optimal level, as projects unjustified on cost-benefit grounds will be enacted, which is the key result of Baron (1991). The

¹⁵ Because the continuation values at R^* define R^* , I present the values of ν_p and ν at R^* . This reduces notation and facilitates companisons across cases.

tendency of majority rule to lead to inefficiently large government is a robust theoretical result. Overspending is generated in models by Tullock (1959), Weingast, Shepsle, and Johnsen (1981), and others. The reason is that the majority does not internalize the costs it imposes on the minority. The sequential choice model reveals that both the minority and the majority (except the proposer) are made worse off by some of the proposals that pass.

Universalistic Presidents

One argument prominent in the literature on the executive role in budgetary politics is that the president can limit inefficient legislative logrolls and thus the size of government (Inman 1993). These analyses typically assume that the president is a universalist; that is, he seeks to maximize the net benefit of all districts. Given that objective, he can use the veto to prevent excessive spending. In the current model, this argument corresponds to the case in which the president's constituency is composed of the entire legislature, or m = N.

In this case, to avoid a veto the total utility of the president's constituency must exceed $N\delta v_p$. To override a veto, k legislators must receive utilities of δv_p . Since $k \leq N$, proposers have an incentive to build override coalitions. Since k > n, a larger coalition is involved than in the "no veto" case. The critical benefit-cost ratio and the continuation values for the universalistic case are given in column 2 of Table 1.

Even under universalism, inefficient programs may pass, as R^* is bounded from above by k/N. Only in the case of an absolute veto (k = N) does the critical ratio reach the efficient value of one. Since k > n, however, Table 1 shows that the critical value for the universalistic president with a veto must be higher than the one for no veto. By forcing proposers to build larger winning coalitions, the executive veto eliminates some of the inefficient programs that would be enacted in its absence. Furthermore, the effectiveness of the veto in limiting spending increases in accord with the number of votes required for override. The lowest possible benefit-cost ratio approaches unity as k goes to N. This analysis again suggests a normative rationale for stronger veto provisions. Intuitively, the veto power increases the efficiency of the level of government expenditure because, as the president cares about the payoffs to each district, all benefits must be shared more equitably. When the benefits are spread over more districts, it becomes somewhat less attractive to implement costly programs. Yet, as I show below, if the president wishes to use the veto to favor particular districts, benefits may not be spread more equitably as veto power increases via an increase in k. In turn, this effect will undermine the beneficial effects of the veto.

¹⁶ Baron (1991) finds that legislative inefficiency is smaller under an open rule, which allows proposals to be amended, and Krehbiel

⁽¹⁹⁹²⁾ finds empirically that open rules are more likely for distributive legislation. Future research will examine the relationship of the executive veto to the choice of legislative rules.

Critical Ratios and	•	Universalistic	Particularistic (Absolute	
Continuation Values	No Veto (1)	President (2)	; <i>m</i> ≥ n (3)	<i>m</i> ≥ <i>n</i> (4)
R*	$\frac{n-\delta(n-1)}{N-\delta(n-1)}$	$\frac{k-\delta(k-1)}{N-\delta(k-1)}$	$\frac{\frac{n}{N}(N-m)-\delta(n-m)}{N-m-\delta(n-m)}$	$\frac{m}{N}$
V_{p}	$\frac{-(N-n)}{N(N-\delta(n-1))}$	$\frac{-(N-k)}{N(N-8(k-1))}$	0	0 '
v	$\frac{-(N-n)}{N(N-8(n-1))}$	$\frac{-(N-k)}{N(N-8(k-1))}$	$\frac{-(N-n)T}{N(N-m-\delta(n-m))}$	$\frac{-mT}{N(N-8(n-m))}$
•	Supermalority	Partisan Vet (Absolute		
٠	Rule (5)	$m < n \ (q = 0) $ (6)	$m \geq n \ (q = 1)$	
R*	$\frac{k-\delta(k-1)}{N-\delta(k-1)}$	$\frac{\frac{n}{N}(N-m)-\delta(n-m-1)}{N-m-\delta(n-m-1)}$	$\frac{m}{N}$	Ţ
V _P	$\frac{-(N-k)}{N(N-8(k-1))}$	0	0	
v	$\frac{-(N-k)}{N(N-8(k-1))}$	$\frac{(N-n)T}{N[N-m\delta(n-m-1)]}$	$-rac{T}{N}$	

Note: $R^* =$ critical benefit-cost ratio; $v_p =$ continuation value of presidential constituent, v = continuation value of nonconstituent; B = total benefits of program; T = tax cost of program, N = size of legislature; n = aimple majority; k = votes needed for override; m = size of presidential constituency; k = votes needed for override; k = size of presidential constituency; k = votes needed for override; k = size of presidential constituency; k = votes needed for override; k = size of presidential constituency; k = votes needed for override; k = size of presidential constituency; k = votes needed for override; k = size of presidential constituency; k = votes needed for override; k = size of presidential constituency; k = votes needed for override; k = size of presidential constituency; k = votes needed for override; k = size of presidential constituency.

PARTICULARISTIC PRESIDENTS

checount factor.

To analyze the effect of a president with nonuniversalistic preferences, I now consider the model for the case in which 0 < m < N. As an example to motivate the more general results, I will focus on $k \ge N$, so that the president has an absolute veto. The results are presented in columns 3 and 4 of Table 1.

Under an absolute veto, a majority coalition is the only feasible route, and the president must receive utility of at least $m\delta v_P$ for the proposal to pass. Successful proposers must allocate the benefits so that this constraint will be satisfied while also satisfying enough other members to create a majority. Including members of the president's constituency helps generate presidential support as well as form a majority for passage. Therefore, each constituency member has a much higher probability of receiving benefits than does a nonmember. Proposers within the president's constituency also fare better than nonmembers because the net benefits they keep for themselves increase the president's utility. Thus, fewer resources must be allocated to other districts in order to avoid a veto. Both these effects increase the expected utility of presidential constituents, so that $\nu_p > \nu$. The extent of this effect can be seen in columns 3 and 4 of Table 1. Because the president's allies are advantaged, his resolve to combat inefficient programs via the veto is diminished, which reduces the critical ratio required for passage.

Comparing the critical ratios of this case with those of the "no veto" case, I find that the absolute veto is more efficient than pure majority voting for all values of m. The president's desire to favor some districts over others does not completely undermine the effects of the veto. The intuition is clear. Under pure majority rule, all members expect negative benefits from an inefficient program. Therefore, the primary consequence of using the veto to favor some districts is to block inefficient programs, but this effect is clearly stronger when the president wants to favor more districts. In both columns 3 and 4 the critical benefit-cost ratio is increasing in m. Therefore, the larger the president's constituency, the smaller is the number of inefficient programs that pass.

For a more general set of results, the assumption that k > N is relaxed to consider the role of the veto override. Now proposers must determine whether to pursue a majority or an override coalition. Note that when m is much larger than k, this decision is irrelevant. The proposer will always build an override coalition, and the outcome is identical to the case of a universalistic president, discussed above. Alternatively, when m is reasonably small, the proposer will always choose to satisfy the president and a simple majority. In this case, the results are identical to the case of an absolute veto.

In general, however, it will not be an equilibrium for the proposer to choose either type of bill with certainty. If the proposer has a clear preference for the override proposal, then the continuation values of the president's constituents will tend to be reduced. Alternatively, if the proposer prefers a proposal acceptable to the president, then the continuation values of those outside his constituency will diminish. In equilibrium, this "bidding process" ends only when the proposer is indifferent between proposing to the majority coalition or to a size k coalition. Given this indifference, each proposer will play a mixed strategy, so that π and π_p may be strictly between 0 and 1.17

The solution to this more general model involves a number of cases that depend on the values of m, k, and n. For each case, the solution of a system of equations constitutes a symmetric, stationary Nash equilibrium. The systems of equations that generate a solution to this game are relegated to the Appendix. Since the equilibrium expressions involved in the case of a qualified veto and a nonuniversal constituency turn out to be very complicated and not very informative, I present a numerical computation to demonstrate the effects of both presidential constituency size and override rules on the size of budgets. Figure 1 plots the critical benefit-cost ratios for a 400-member legislature with a discount rate of .9 as a function of m and k.

Figure 1 demonstrates that inefficiency diminishes with an increase in both the size of the president's constituency and the number of votes required for an override. Perhaps more surprising is that for any m and k > n, inefficiency and spending are lower with a presidential veto than without it. The logic behind these results is straightforward, as the positive effects of constituency size and veto power are combined. The enhanced veto power increases the president's ability to shield his constituency from inefficient programs; at the same time, it gives proposers an incentive to build override coalitions to defeat a veto. This spreads the benefits across a greater number of districts and lowers the benefit of an inefficient program to the proposer. This result conforms to the conventional wisdom that executive veto power may well place a break on distributive spending. Yet, the result depends neither on universalistic preferences nor the placement of an exogenous reversion. Any constraint on the proposer's ability to play each district against another in the formation of a coalition will inhibit inefficient spending. Particularistic presidents are not sufficient to eliminate the beneficial effect of veto power.

The effect of increasing the president's constituency size is also apparent. A larger m will force proposers to spread the benefits across more districts and will lower the amount they can retain for themselves. Larger presidential constituencies also lessen the ability and

desire of the president to redistribute. Both influences tend to lower the critical benefit-cost ratio.

The previous discussion can be summarized as follows.

Result Summary 1. In the case of particularistic presidents,

- 1. the degree of inefficiency and the level of spending decrease when the president has a large constituency and veto powers.
- 2. the degree of inefficiency and the level of spending are lower than in the case of no executive veto, regardless of the size of the president's constituency and his veto powers.
- 3. the degree of inefficiency and the level of spending are higher than when the president is a universalist, given any level of veto power.
- 4. veto powers are effective in allocating spending toward districts favored by the president.

SUPERMAJORITARIANISM AND THE EXECUTIVE VETO

Although it appears that the veto is an effective constraint on legislative logrolls, it does not fare very well in comparison to alternatives such as supermajoritarian procedures. A model of legislative decision making under a k-majority rule is identical to the no veto case if n = k. This model generates the same benefit-cost ratio as the case of a universalistic executive veto with a k-vote override (column 2 of Table 1). Given the results of the previous section, it is easily shown that in terms of efficiency this k-majority rule dominates a k-veto institution. Furthermore, supermajoritarianism does not lead to the inequitable distribution of resources induced by the veto. These results suggest that budgetary reforms of internal legislative procedures may well be more desirable than those that involve enhancement of executive power.

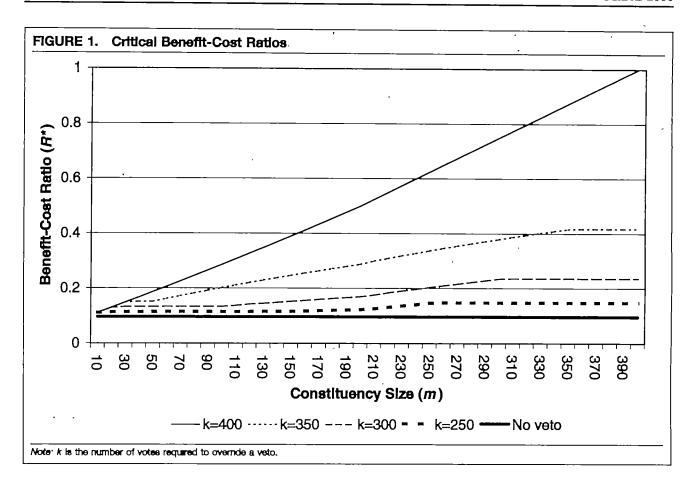
Result Summary 2. Bargaining under a k-majority

- 1. produces outcomes identical to a universalistic president subject to a k-vote override.
- performs better than a particularistic president subject to a k-vote override in terms of efficiency and spending.
- is more egalitarian than presidential veto power with a k-vote override because it does not skew results toward the president's constituents.

PARTISANSHIP AND DIVIDED GOVERNMENT

Until this point, the role of the president's constituency has been discussed in rather abstract terms. Empirically, however, it is likely that the president's constituency will coincide with membership in his party. If so, the results of my model suggest testable connections between distributive spending and the size of the president's party in the legislature. When it is small

¹⁷ These probabilities should not be interpreted as the likelihood of a veto occurring. If the president knows that his veto will be overridden, then he will have no incentive to execute it. In fact, vetoes never occur in equilibrium. Therefore, the framework should not be viewed as a model of vetoes per se but as a model of the effect that the existence of the veto institution has on the distribution and level of expenditure. The empirical content of π and π_p is the probability distribution of winning coalition sizes.



(such as divided government), spending should be more distributive than when it is stronger (unified government). This prediction, however, is based on a dubious assumption.

To this point, the model has assumed that the recognition probabilities are uniform and do not depend on a legislator's status in the president's constituency. If that constituency is partisan, however, then the ability of the majority party to exert disproportionate control over the legislative agenda may lead to recognition probabilities that are a function of a member's status vis-à-vis the president. Suppose there is divided government and the minority (president's) party is recognized to make proposals with probability zero. This may inhibit the president's ability to redirect benefits to his party members and thus upset the prediction that divided governments engage in more distributive spending. Again, such logic turns out to be false.

To explore the possible effect of partisan differences in recognition probabilities, let q/m be the probability that a member of the president's party is recognized, and let (1-q)/(N-m) be the probability that an opposition party member is recognized. An interpretation of these probabilities is that the president's party is chosen to make a proposal with probability q, while the actual proposer is drawn uniformly from the party's members.

The previous results for uniform recognition repre-

sent the case of q = m/N. This is the relevant benchmark for a weak party system. Now consider the case of a strong party system, where q = 1 if $m \ge n$, and q = 0 if m < n. The weak party model refers to the case of uniform proposal power. These results are identical to columns 3 and 4 of Table 1. For simplicity, columns 6 and 7 refer only to the case of an absolute veto.

In column 6, while the veto power without proposal power may not allow the president's party to share in the positive benefits of an efficient program, the negative power of the veto prevents the president's party from having to share in the costs of an inefficient program. This also serves to limit the inefficiency of enacted programs. Thus, even when the president's party has no proposal power, the veto may constrain the distributive choices of the legislature.

When the president's party is in the majority (column 7), one might suspect that the veto would help the majority further exploit the minority and increase distributive spending. My results show this is not the case, because the primary role of the veto is to prevent members of the majority from exploiting one another.

This result also underscores that veto power is more important for efficiency and spending than is the allocation of proposal powers. In the case of m > n, the critical values are identical across the cases (columns 4 and 7). When m < n, the strong party

legislature performs better, but the effect is quite small. For example, if N = 400, then the maximum difference across all values of m and δ between the critical values of columns 3 and 6 is about .003.

Thus, a strong partisan legislature does not contradict the implication of a correlation between divided government and distributive spending. McCubbins (1991) presents evidence of such a correlation for appropriations to 69 federal agencies from 1948 to 1985, and Lohmann and O'Halloran (1994) find a correlation between trade protection—a quintessential distributive policy—and divided government.

Result Summary 3. In terms of spending and efficiency, the strong party legislature performs only slightly better than the weak party legislature. The predictions about divided government are robust to assumptions about party control of the legislative agenda.

CONCLUSION

This article has presented a model of the influence of the executive veto on legislative spending decisions. In contrast to previous work, it focuses not only on how the veto may affect the overall level of spending but also on the distribution of spending across political jurisdictions. The results indicate that the veto may have quite large effects on the distribution of spending even when the executive chooses to favor small constituencies. Furthermore, the concentration of benefits to the presidential constituency does indeed serve to limit spending rather than increase it. For this reason, augmenting the power of the executive, regardless of his parochial or political interests, may move distributive spending toward the efficient level.

The model has many normative implications for the design of legislative and electoral institutions. With regard to legislative processes, it argues strongly for executive veto provisions in terms of efficiency but shows that such institutions will have large distributive effects. Control over the distribution of spending may give the executive a large bargaining advantage in other policy areas. This effect will certainly tend to inhibit reforms, such as an increase in the number of votes needed to override. In contrast, reforms of electoral institutions may have the same desirable effects without the distributional consequences. For example, the electoral college system may encourage presidential parochialism, as votes in some legislative jurisdictions are more important than in others.¹⁸ The presidential primary system tends to place weight on early and winner-take-all contests. The logic of my model suggests that unweighted electoral schemes or national primaries may minimize the incentive to favor one district over another.

The results also speak to a current debate in the comparative literature on presidentialism. Many schol-

ars have noted the especially dire consequences of presidentialism (relative to parliamentarism) when the president's party in the legislature is small (see Jones 1995; Linz 1994; Shugart and Carey 1995). My model predicts that such situations will also be characterized by a much higher level of distributive spending. Perhaps most important, spending will be heavily skewed toward the party of the president, perhaps further exacerbating underlying economic inequality. Since Shugart and Carey (1995) and Jones (1995) find evidence that certain electoral institutions are more likely to produce presidents with legislative minorities, the model suggests a strong interaction between electoral and legislative procedures in the production of fiscal policies.

In addition to the normative implications, a number of testable hypotheses emerge from this model. The first is that one might expect the president to use the veto to favor electorally important districts or legislative allies. Grier, McDonald, and Tollison (1995) and Mebane and Wawro (1996) have supported the hypothesis that the president uses the veto to favor election constituencies. The second is that patterns of spending across states should reflect electoral and/or legislative support for the president. Finally, there may be a relationship between the breadth of the president's constituency and the size of government.

Again, if one uses political party as a proxy for the president's constituency, the model suggests that government should grow faster during periods of divided government because the president's constituency is smaller. This implication could also be tested on data at the state level, which would provide not only cross-sectional variation in divided versus unified governments but also variation in the types of executive veto institutions, including the line-item veto. In the same vein, the model could be quite useful in explaining fiscal policy outcomes in other presidential democracies. These data would provide much more variation in presidential and legislative electoral institutions, party systems, and executive powers.

Finally, the model may be able to explain behavioral differences between presidents in their first as compared to second term. Since the lame duck has somewhat less proximate electoral concerns, he may have fewer parochial preferences and may attempt to maximize the utility of a greater set of districts. In turn, the model implies that fewer inefficient projects would pass, leading to lower government spending in the second term relative to the first. ¹⁹ This effect may help explain why there are more executive than legislative term limitations (Carey 1996).

¹⁸ Brams (1978) and Colantoni, Levesque, and Ordeshook (1975) analyze the effect of the electoral college on the allocation of campaign resources.

¹⁹ An alternative would be to give the second-term president a lower discount factor than legislators because of the reduced importance of the future. While a complete analysis of such a model is beyond the scope of this article, my intuition is that the implication would be the same as that of a larger constituency. As in the case of the individual legislator, a lower discount factor suggests that fears of future inefficient programs would not sufficiently intimidate the president into accepting a current one, which also eliminates inefficient programs.

APPENDIX

Formal Description of Model

Strategies. Let $X = \{x | \sum_{i=1}^{N} x_i \le B\}$ and $\Delta(X)$ be the set of probability distributions over it. A (behavioral) strategy for proposer i at time t and with history h is given by $\sigma_{ith} \in \Delta(X)$. Stationarity requires that $\sigma_{ith} = \sigma_i$ for all t and h. Symmetry implies that $\sigma_i = \sigma_p$ if $i \in M$, and $\sigma_i = \sigma$ otherwise. A (behavioral) strategy for a legislator i in the voting stage is given by $A_{ith}(x) \in \{0, 1\}$, where $A_{ith} = 1$ corresponds to a vote in favor, and $A_{ith} = 0$ a vote against. Similarly, stationarity requires that $A_{ith} = A_i$ for all t and h. Symmetry requires that $A_i = A_p$ if $i \in M$, and $A_i = A$ otherwise. Let $P_{th}(x) \in \{0, 1\}$ be the strategy of the president, $P_{th} = 1$ corresponds to signing the bill, and $P_{th} = 0$ implies a veto. $P_{th} = P$ for all t and h. A proposal x passes if $\sum_{i=1}^{N} A_i(x) \ge k$ or if $\sum_{i=1}^{N} A_i(x) \ge n$ and P(x) = 1.

Payoff. The utility functions are described in the text. Let v_{tth} be the expected utility of beginning the subgame of time t and history h. Stationarity and symmetry imply that $v_{tth} = v_p$ if $t \in M$, and v otherwise.

Solving the Model

Presidential Veto Stage. P(x) = 1 if and only if $\sum_{i=1}^{m} u(x_i - T/N) \ge \delta m \nu_P$. This implies acceptance when $\sum_{i=1}^{m} x_i \ge m(\delta \nu_P + T/N)$ if $\nu_i < 0$, or $\sum_{i=1}^{m} x_i \ge m(\delta \nu_P/\lambda + T/N)$ if $\nu_i \ge 0$.

Legislative Voting Stage. $A_i(x) = 1$ if and only if $u(x_i - T/N) > \delta v_i$. Thus, she accepts if $x_i \ge \delta v_i + T/N$ if $v_i < 0$, or $x_i \ge \delta v_i / \lambda + T/N$ if $v_i \ge 0$.

Proposal Stage. Let X_n be the set of proposals from player i, such that $\sum_{i=1}^{N} A_i(x) \geq n$ and P(x) = 1, and let X_k be those such that $\sum_{i=1}^{N} A_i(x) \geq k$. Therefore, the costs to the proposer of proposal $x \in X_k \cup X_n$ are $c(x) = \sum x_{\sim i} + T/N$. Any proposal $x \notin X_k \cup X_n$ fails, and the bargaining continues. If proposer i chooses $x \in X_k \cup X_n$, she keeps $z_i = B - c(x)$ and her payoffs are $u(z_i)$. If the proposer chooses $x \notin X_k \cup X_n$, her payoffs are δv_i . Let $X_k \subseteq X_k$ and $X_n \subseteq X_n$ be the lowest cost proposals for both of these sets. Thus, the best acceptable proposal solves $\max_{x \in X_k \cup X_n} u(B - c(x))$. If this optimal utility level is less than δv_i , the proposer chooses $x \notin X_k \cup X_n$.

I now turn to characterizing X_n^* and X_k^* . Given the best responses of legislators, these sets can be characterized by the number of constituency members and nonmembers for whom A=1. Let $\mu_p^{in}=\sum A_p(x)$ and $\mu^{in}=\sum A(x)$ for $x\in X_n^*$, and let $\mu_p^{ik}=\sum A_p(x)$ and $\mu^{ik}=\sum A(x)$ for $x\in X_n^*$. Clearly, $\mu_p^{in}+\mu^{ik}\geq n$ and $\mu_p^{ik}+\mu^{ik}\geq k$. Assuming symmetry, all the numbers are the same across members and nonmembers, so that all the X_n^* and X_n^* sets can be characterized by $\{\mu_p^{pn}, \mu_p^{pk}, \mu_p^{pn}, \mu_p^{pn$

LEMMA 1: $v_p \ge v$.

Since by lemma 1 constituency members have higher continuation values, all the minimal-cost winning coalitions will include the minimum number of constituency members necessary to satisfy the president. So first consider $i \notin M$. All elements of X_n^* are cost equivalent to selecting $\mu_p^n = m$ constituency members and $\mu^n = \max\{0, n - m - 1\}$ nonmembers, whereas the elements of X_k^* require the votes of $\mu^k = \min\{k - 1, N - m - 1\}$ nonmembers and $\mu_p^k = \max\{0, k + m - N\}$ members. For $i \in M$, any benefits retained by the proposer raise the president's utility, so

elements X_n^* do not require satisfying all m members of M. Furthermore, since the president's utility function is concave, the extraordinary benefits of the proposer have a lower marginal utility than benefits given to those left out of the winning coalition. Therefore, to satisfy the president, a constituency proposer may have to reallocate money to members who vote against the proposal. Let d be the amount of reallocated money. Without loss of generality, I assume that d is divided equally among those who choose $A_p = 1$. Such an additional allocation is unnecessary for nonconstituency proposers since they may always divide benefits equally among M.

Thus, proposals from members of M may take the following form.

- 1. Distribute benefits to generate the support of $\mu_p^{pn} = j$ members and $\mu_p^{pn} = \max\{0, n-j-1\}$ nonmembers.
- 2. Divide d units of the benefits among the m-j-1 remaining members so that $\sum_{i=1}^{m} u(x_i T/N) \ge \delta m v_P$ is satisfied. The fact that these remaining members have a higher marginal utility for the benefits than does the proposer serves to raise the president's utility.

The proposer will choose j and d to minimize costs. The set of override proposals is similar to that of nonmembers: X_k^{ro} requires support of $\mu^{pk} = \min\{k-1, N-m\}$ nonmembers and $\mu_p^{pk} = \max\{0, k+m-N-1\}$ members.

To generate symmetric equilibria, I assume that each proposer randomizes uniformly within X_n^* and X_k^* . If a proposer is indifferent between the elements of X_n^* and X_k^* , then she chooses an element of X_k^* with probability π_i . Therefore, the expected number of members of group l selected by a proposer of type l is $\pi_i \mu_l^{lk} + (1 - \pi_l) \mu_l^{ln}$. Given X_n^* , X_k^* , and π_l , one can compute the probability that legislator l receives $u^{-1}(\delta v_l) + T/N$ and therefore chooses $A_l = 1$. This probability is denoted ϕ_l^l . Due to the symmetry assumptions, there are only four possible values $-\phi_p^p$, ϕ_p , ϕ_p^p , and ϕ —corresponding to whether $i, l \in M$. Simple calculations show that $\phi_p^p = [\pi_p \mu_p^{pk} + (1 - \pi_p) \mu_p^{pn}]/(m - 1)$, $\phi^p = [\pi_p \mu_p^{pk} + (1 - \pi_p) \mu_p^{pn}]/(m - 1)$.

These strategies lead to the following expression for the net benefits of proposing:

$$\begin{split} z_p &= B - (m-1) \varphi_p^p u^{-1} (\delta v_p) - (N-m) \varphi^p u^{-1} (\delta v) \\ &- ((m-1) \varphi_p^p + (N-m) \varphi^p) \frac{T}{N} - (1-\pi_p) d; \\ z &= B - m \varphi_p u^{-1} (\delta v_p) - (N-m-1) \varphi u^{-1} (\delta v) \end{split}$$
 (A-1)

$$= B - m\phi_{p}u^{-1}(\delta v_{p}) - (N - m - 1)\phi u^{-1}(\delta v)$$

$$- (m\phi_{p} + (N - m - 1)\phi)\frac{T}{N}.$$
 (A-2)

The expressions for the continuation values are

$$v_{p} = \frac{1}{N} \left(u(z_{p}) + ((m-1)\phi_{p}^{p} + (N-m)\phi_{p}) \left(\delta v_{p} + \frac{T}{N} \right) + (1 - \phi_{p}^{p})u(d) \right) - \frac{T}{N}; \tag{A-3}$$

$$v = \frac{1}{N} \left(u(z) + (m\phi^p + (N-m-1)\phi) \left(\delta v + \frac{T}{N} \right) \right) - \frac{T}{N}.$$
(A-4)

The solution to the model requires solving equations A-1-A-4 for z, z_p , v_p , v, j, d, π , and π_p as a function of the

TABLE A-1.	Parameters for Equations	A-1-A-4	
Case/ Parameters	m < N - k + 1	$N-k+1 \le m < n-1$	<i>m</i> ≥ <i>n</i> − 1
ϕ_{ρ}^{ρ}	$(1-\pi_p)\left(\frac{j}{m-1}\right)$	$\frac{\pi_p(k+m-N-1)+(1-\pi_p)j}{m-1}$	$\frac{\pi_{p}(k+m-N-1)+(1-\pi_{p})j}{m-1}$
$\phi_{_{P}}$	$1 - \pi$	$\frac{\pi(k+m-N)+(1-\pi)m}{m}$	$\frac{\pi(k+m-N)+(1-\pi)m}{m}$
φ°	$\frac{\pi_{p}(k-1) + (1-\pi_{p})(n-j-1)}{N-m}$	$\frac{\pi_p(N-m)+(1-\pi_p)(n-j-1)}{N-m}$	$\frac{\pi_{p}(N-m) + (1-\pi_{p})(n-j-1)}{N-m}$
ф	$\frac{\pi(k-1) + (1-\pi)(n-m-1)}{N-m-1}$	$\frac{\pi(N-m-1)+(1-\pi)(n-m-1)}{N-m-1}$	π
	N-m-1	11 770 2	

parameters. The parameters ϕ_p^p , ϕ_p , ϕ_p^p , and ϕ are given in Table A-1 for the three relevant cases.

Additional equations are provided by indifference conditions for mixed strategies. To play mixed strategies, proposers must be indifferent between the override and the majority proposals. Since a constituency proposer always gets to keep more benefits in a majority proposal than does a nonconstituency proposer, it is impossible to have an equilibrium in which both types play mixed strategies. The only possibilities for mixed strategy equilibria are $\pi_p = 0$, $0 < \pi < 1$, and $0 < \pi_p < 1$, $\pi = 1$. It can also be shown that if m < n there are no equilibria for which $\pi = 1$ (this proof is tedious and not instructively valuable). This leaves the three cases listed in Table A-2. The condition that proposers who mix must be indifferent leads to an additional equation of the following general form:

$$\alpha \nu_P - \beta \nu - \gamma \frac{T}{N} = 0, \qquad (A-5)$$

where α , β , and γ are all nonnegative functions of the model's parameters. Table A-2 gives the values of α , β , and γ for each of the cases of mixed strategy equilibria.

Solutions are generated by solving a system of six equations for z, z_p , v_p , v, π , and π_p as a function of d and j and then choosing d and j to maximize z_p . The solutions are unique up to some choices of d and j. This fact is of minor concern, since z, z_p , v_p , and v are the main objects of analysis.

Proof of No Veto Case (Column 1): The case of no legislative veto is identical to m=0. Thus, override coalitions are unnecessary, and every successful proposal requires only n votes. Therefore, $\pi=0$, $\mu^n=n-1$, and $\phi=(n-1)/(N-1)$. To generate the lowest possible benefit-cost ratio for a successful program, I focus on the case of the solution in which $\nu<0$ (recall $z>\delta\nu$ for a successful program). Plugging these values into equations A-2 and A-4, I obtain $z=B-(n-1)\delta\nu-n(T/N)$ and $\nu=N^{-1}(\lambda z+(n-1)\delta\nu-(N-n)T/N)$. Solving these equations generates $z^*=[(N-\delta(n-1))B+(n-\delta(n-1))T/N]$

1)n/N]/[$N + \delta(\lambda - 1)(n - 1)$]. There is a continuum of stationary, symmetric Nash equilibria such that a proposal is made if and only if $z^* \geq \xi$, where $0 \geq \xi \geq \delta \nu$. Following Baron (1991), I focus only on those in which a proposal is made if $z^* \geq 0$. By stationarity, if this condition holds in any period, it will hold in all periods, including the first. Therefore, in equilibrium a proposal will pass either in the first period or not at all. From the expression for z^* , a proposal will be made and passed in the first period if and only if $B/T \geq R^* = [n - \delta(n-1)]/[N - \delta(n-1)]$. Q.E.D.

Proof of Presidential Universalism Case (Column 2): In the case of a universalistic president, it is always cheaper to satisfy k members than the president. Therefore, $\pi_p = 1$, $\mu_p^{pk} = k-1$, and $\phi = (k-1)/(N-1)$. Again focusing on $v_p < 0$, equations A-1 and A-3 generate $z_p = B - (k-1)\delta v_p - k(T/N)$ and $v_p = N^{-1}(\lambda z_p + (k-1)\delta v_p - (N-k)T/N)$. Solving these equations generally, $z_p^* = [(N-\delta(k-1))B + (k-\delta(k-1))T]/[N+\delta(\lambda-1)(k-1)]$ and $v_p^* = [\lambda B - (1+(\lambda-1)k/N)T]/[N+\delta(\lambda-1)(k-1)]$. Focusing on the same equilibrium as in benchmark 1, I find that a proposal will be made and passed if and only if $B/T \ge R^* = [k-\delta(k-1)]/[N-\delta(k-1)]$. Q.E.D.

Proof of Particularistic President Case (Columns 3 and 4): The minimally efficient benefit-cost ratio R^* must solve $z_p=0$ and equations A-1-A-4 above. It is easy to show that when $z_p=0$, z<0, v<0, and $v_p<0$. Since overrides are not possible, $\pi_p=\pi=0$. Each reference to equations A-1-A-4 assumes the substitution of the appropriate parameters from Table A-1.

Column 3. m < n: To avoid vetoes, proposals must also satisfy

$$\lambda z_P + j \delta v_P - (m - j - 1) \frac{T}{N} + d \ge m \delta v_P. \quad (A-6)$$

Given that $z_p = 0$, satisfying equation A-6 requires $d \ge (m-j)\delta v_p + (m-j-1)T/N$. Either this condition must bind, or d = 0. Otherwise, a constituency proposer could keep more benefits. If d = 0, then we claim $j^* = m - 1$. If

TABL	TABLE A-2. Parameters for Indifference Conditions							
	$m < N - k + 1$ $1 > \pi > 0$	$N - k + 1 \le m < n - 1$ 1 > π > 0	$m \ge n-1$ $1 > \pi > 0$	$m \ge n - 1$ 1 > π_p > 0, π = 1				
α γ β	m (k − n)/δ m + k − n	N – k (k – n)/ð N – n	N − k (k − m − 1)/δ N − m − 1	N + J - m - k + 1 $(k - n)/\delta$ N + J - m - n + 1				
Note: av	$\gamma_{\rho} - \beta v - \gamma (T/N) = 0.$							

not, then the condition implies that $v_P < -(m-j-1)/\delta(m-j)$, whereas equation A-3 would imply $v_P = -(m-j-1)/[N-\delta N+\delta(m-j)]$, which is a contradiction, since $N > \delta N$. Since either the veto constraint must bind or $j^* = m-1$, equation A-3 becomes $v_P = \delta v_P$, which implies that $v_P = 0$ at the minimally efficient proposal. Substituting $v_P = 0$ into equation A-2, we find that equations A-2 and A-1 imply that $z = \delta v$. Substitution into equation A-4 yields $v = -(N-\kappa-1)T/(N-\delta\kappa-\delta)N$, where $\kappa = m([n-j-1]/[N-m]) + N-m-1$. Substituting z_P, v_P , and v into equation A-1 gives $R^* = [n(N-\delta\kappa-\delta) - \delta(n-j-1)(N-\kappa-1)]/(N-\delta\kappa-\delta)$. This is a decreasing function of j, so that the minimum is obtained when j = m-1. Substitution of κ and κ leads to κ = $[n(N-m)-\delta N(n-m)]/[N(N-m-\delta(n-m))]$, which leads to $v = -(N-n)T/[N(N-m-\delta(n-m))]$.

Column 4. m > n: At $z_p = 0$, using arguments similar to those above, equation A-5 must be a binding constraint with d>0. As above, $\nu_p=0$. I now claim that at the minimally efficient program $j^*=n-1$. Suppose not. Then the constituency proposer could lower d by T/N by replacing one nonconstituency member with a constituency member and still satisfy the veto constraint at a cost of $\delta v_p - \delta v = -\delta v$. Since v > -T/N, this deviation pays for the proposer. Thus, at R^* , $j^* = n - 1$. Given these results, equations A-1 and A-5 imply that B - m(T/N) = 0, so that $R^* = m/N$. Note, however, that from inspection of equations A-2 and A-4 a nonconstituency proposer would not make this proposal, as it implies z = v = -T/N, so that $z < \delta v$. At this critical value only members of M make proposals that pass, while nonmembers make proposals that fail (or none at all). Thus, in periods in which a nonmember is chosen, every member's payoff is the discounted continuation value. This implies that $v = -mT/[N(N-\delta(N-m))].$

Proof of Partisan Veto Model (columns 6 and 7):

Column 6. If m < n and q = 0, then all proposers are nonconstituency members who must allocate $m(\delta v_P + T/N)$ to M to avoid a presidential veto. Thus, each constituency member receives benefits with certainty, so that $v_P = \delta v_P$ and $v_P = 0$. This implies $z = B - \delta(n - m - 1)v - n(T/N)$ and $v = [z + (n - m - 1)\delta v - (N - n)T/N]/(N - m)$. Since the minimally efficient program is the one in which z = 0, we have $v = -(N - n)T/[N(N - m - \delta(n - m - 1))]$ and $B - [n - [\delta(n - m - 1)(N - n)]/[N - m - \delta(n - m - 1)]]T/N = 0$. This leads to $R^* = [(n/N)(N - m) - \delta(n - m - 1)]/[N - m - \delta(n - m - 1)]$.

Column 7. If m > n and q = 1, a nonmember can never be a proposer. Therefore, $v = [(n - j - 1)\delta v - (N - m - n + j + 1)(T/N)]/(N - m)$. Using an identical argument as in the proof of column 4, it can be established that $j^* = n - 1$, and the veto constraint binds with d > 0. Since $j^* = n - 1$, v = -T/N. The continuation value for a member of M is $v_P = [z_P + c + j\delta v_P + (m - j - 1)(T/N)]/m$. Yet, since $z_p = 0$, and $d = (m - j)\delta v_P + (m - j - 1)(T/N)$ at the minimally efficient program, this reduces to $v_p = 0$. Finally, inserting $v_p = 0$ and $j^* = n - 1$ to equation A-1, I get B - m(T/N) = 0, so that $R^* = m/N$.

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Issues, Economics, and the Dynamics of Multiparty Elections: The British 1987 General Election

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Te offer a model of multiparty elections that combines voters' retrospective economic evaluations with consideration of parties' issue positions and the issue preferences of voters. We show that both policy issues and the state of the economy matter in British elections. In 1987 voters made a largely retrospective evaluation of the Conservatives based on economic performance; those who rejected the Conservative Party chose between Labour and Alliance based on issue positions. Through simulations we move the parties in the issue space and reestimate vote shares as well as hypothesize an alternative distribution of views on the economy, and we show that Labour had virtually no chance to win with a centrist party as a viable alternative. The predictions from our 1987 simulations are supported in an analysis of the 1992 British election. We argue for multinomial probit in studying three-party elections because it allows for a richer formulation of politics than do competing methods.

There are at least two theories of voter choice in multiparty elections. The first emphasizes retrospective voting, that is, the incumbent party is evaluated based on economic performance during the recent past. The second emphasizes prospective voting based on issue evaluations of each party. These are complementary theories because each alone may be inadequate to explain voter decisions in a multiparty setting. The retrospective model cannot explain how voters choose between competing nonincumbent parties if they decide to vote against the incumbent, but the prospective issue voting model does not allow voters to punish or reward the incumbent party for economic performance. In this article we offer a framework that integrates and tests both theories. To test them we need to measure voters' views of the economy and of the parties' positions on the issues.

We use the 1987 British general election to test our model for several reasons. First, it has been studied by other researchers, and we want to show how our new methods affect previous scholarship. Second, it was an important event in the evolution of party politics in Britain. It marked the beginning of a rightward shift by

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Labour, and we want to explore whether a further moderation in 1987 could have won them the election. The 1987 contest also presaged later developments in party politics; the Social Democrat Party (SDP), who had only recently broken off from the Labour Party, ran with the Liberals as the Alliance. British voters were offered relatively clear rightist (Conservative), centrist (Alliance), and leftist (Labour) choices. Finally, the 1987 election took place during an economic boom and was the last in which Margaret Thatcher led the Conservative Party.

Our model of multiparty elections in Britain combines retrospective economic evaluations with the relative positions of the parties and voters in the issue space. The model allows voters simultaneously to evaluate all three parties-Conservatives, Labour, and the Alliance—rather than limit their choices to pairs of parties, as is usually done. We show that policy issues and the state of the national economy both mattered in the 1987 general election. Voters made a retrospective evaluation of the Conservatives based on economic performance, and those who rejected the Conservative Party chose between Labour and the Alliance based on issue positions. By simulating issue and economic effects on the election, we show that Labour had virtually no chance to win with the Alliance in the race. Even without the presence of a third party, Labour would have needed to moderate its policy positions to compete seriously with the Conservatives. Using the 1992 British election as an important out-of-sample test for our 1987 results, we find support for our predictions. In conclusion, we will argue that our multinomial probit method is a superior approach for studying multiparty elections, as it allows for a richer formulation of politics than do competing methods.

THE BRITISH CASE

First we will determine how retrospective voting operates in a three-party race. We will show that past economic performance influenced whether an individ-

ual voted for the incumbent Conservatives, but we also will show that economic considerations played little role in voter evaluations of the nonincumbent parties. Second, we will look at prospective issue voting in multiparty elections in a new way: We examine (1) how parties can influence their vote share by moving in the issue space with the existing configuration of parties and (2) how the Conservatives and Labour could have influenced their vote share if the issue space had not included a viable centrist third party. This gives us the opportunity to measure both the effect of prospective issue voting in a multiparty election and the electoral significance of a third party. We will show that, given the party configuration in 1987, none of the three could have substantially improved its vote share by moving in the issue space, but without the Alliance, Labour could have substantially increased its share by moderating its issue positions. This, of course, is consistent with events that followed. Using data from the 1992 election, we show that voters did perceive a change in Labour's issue positions after the 1987 contest; our 1987 results provide predictions supported by the 1992 data. Our analysis demonstrates that prospective issue voting is an important component of multiparty elections, since it yields strong evidence that individuals are much more likely to vote for parties closer to their own position on a variety of issues.

Research on the British case typically focuses only on vote shifts or judgments relating to the two main parties, Conservative and Labour. The Liberal or Alliance parties usually are ignored, and those who vote for them tend to be excluded from analyses (e.g., McAllister and Studlar 1992, Mishler, Hoskin, and Fitzgerald 1989). Most of the aggregate-level popularity function literature models Conservative popularity (as either a percentage or share of the two-party vote) and only accounts for the Alliance as an intervention effect, not as a choice facing voters (see, e.g., Sanders 1996). Another strategy is to assume that the Liberals or the Alliance are positioned midway between the Conservatives and Labour (which is not true on all issues) and proceed to assign vote shares to the parties (McAllister and Mughan 1987). These are ways to make tractable the study of systems with more than two parties and highlight the difficulties, both methodological and conceptual, of studying multiparty systems. Such strategies, however, downplay the role of minor parties and thus minimize any third-party effects, as some of these authors readily acknowledge (e.g., Mishler, Hoskin, and Fitzgerald 1989, 216).

A large part of the dichotomous approach to British elections has been driven by estimation procedures; binomial probit and logit are readily available tools for analyzing two-party races. Yet, another factor is the willingness to make a strong assumption about party competition in Britain (Alvarez and Nagler 1998a). The literature typically assumes rather than demonstrates that British voters choose primarily between Labour and Conservatives, without much consideration of centrist parties (either the Alliance or the Liberal Democrats). This is a matter that can be examined empirically, and we would be well served to

adopt methods that allow us to understand fully how voters view their choices.

Comparisons restricted to the Conservatives and Labour provide limited insight into voter behavior in the face of three parties. Some authors recognize this problem. For example, Heath et al. (1991, 218) note that "one important reason why Labour does rather poorly according to this model is that the Alliance position was closer to Labour's than it was to the Conservatives' position, thus leaving the Conservatives with a larger territory over which to gather votes." Despite the general significance they accord to longterm factors in understanding British elections, their comment suggests it is important to consider multiparty politics. It is a fallacy to pretend that the third party does not affect the fortunes of the other two parties. A model that considers voter choice among all three parties allows us to determine the influence of issues in British elections as well as the effect of economic evaluations in the multiparty setting. It also enables us to determine the effect not only of the third party but also of its withdrawal.1 Ignoring the three choices voters face underestimates the effects of issues and of third or minor parties on election outcomes.

Our well-specified model of voter choice in the 1987 election has at least four features that make it superior to past efforts. First, we separate the three choices and allow the voter to consider simultaneously all three parties. Second, we do not make unduly restrictive assumptions about the influence of a third party on the relative probabilities of choosing either of the other two parties. Third, we include explicit measures for the distance between respondents and parties on seven different issues. With these measures we estimate both the influence of each issue on the vote choice of individuals for all three parties and the effect of movement by the parties on these issues. Fourth, we include measures of voter evaluations of past economic performance in terms of inflation, unemployment, and taxation. These variables allow us to examine retrospective economic voting for all three parties.

With our model we can test directly the competing hypotheses about British general elections. First, we examine the importance of the positions taken by Labour on such key issues as defense and nationalization, which we find account for a portion of Labour's lack of success in 1987. Second, we measure the influence of a major short-term force, voter perceptions of the economy. We predict how different the 1987 election would have been if the perceptions had been the same as they were in 1992, and we find that

¹ Whitten and Palmer (1996) use multinomial logit to study the choices of British voters for the three parties in the 1987 election. That method provides statistical efficiency compared to successive binary logits, but Whitten and Palmer are mistaken to imply that its results differ from a series of binary choice models comparing two-party pairings. More important, by using only voter-specific characteristics as independent variables rather than measures that incorporate the party positions on issues, Whitten and Palmer are unable to determine the effects of movements by the parties on the issues, which is a fundamental substantive concern of the recent literature on British elections. See Alvarez and Nagler (1998a) for further discussion.

Labour and the Alliance would have received greater shares of the vote. Third, we estimate where Alliance voters would have gone if that party had not been in the race. In a contest between only Labour and the Conservatives in which both maintained the same respective positions on the issues, Labour would have received slightly more of those votes. If Labour had adjusted its positions in the absence of the Alliance, it would have received a larger share, which would have given it about as many votes as the Conservatives. All this allows us to pit the two major competing theories of elections, economic and issue voting, against each other.

ISSUE VOTING IN BRITAIN

The literature on British elections debates the relative importance of short- versus long-term effects and the importance of Labour's shifting policy positions throughout the 1980s. Before the 1970s such long-term forces as social class and party loyalty dominated empirical studies of British voting behavior. More recent work has begun to stress shorter term factors, including economic performance and issue positions of the parties (e.g., Alt 1979; Clarke, Mishler, and Whitely 1990; Clarke and Whitely 1990; Crewe 1992; Mishler, Hoskin, and Fitzgerald 1989; Price and Sanders 1993; Sanders 1996). Most of this literature analyzes aggregate-level popularity functions to establish that shortterm factors, principally economic, do matter. In addition, researchers have sought to establish the influence of prime ministerial popularity and such short-term variables as the Falklands War (Clarke, Mishler, and Whitely 1990; Stewart and Clarke 1992). These effects are generally held to be of growing importance due to the erosion of longer term party and class ties (Clarke and Stewart 1984).

Ivor Crewe is especially forthright in arguing that shorter term factors specifically underlay the unprecedented third consecutive Conservative victory in 1987 (Crewe 1992). He notes that in "prosperity... lies the key to Conservative victory" (p. 352). Not only did people believe that both the economy in general and family living standards improved between fall 1986 and summer 1987, but also there was a "close coincidence" between economic perceptions and the Conservative/Labour vote. By that Crewe seems to mean that Conservative voters generally thought the economy had done well, whereas Labour voters did not.²

Crewe (1992) also stresses two other factors. First, he notes that the Labour leader, Kinnock, was a drag on the party vote, although he marked a major improvement over the previous leader, Michael Foot. Thatcher, however, was an electoral asset for the Conservative Party. Second, Crewe claims that issue positions in general favored Labour (although defense is alleged to be a liability, as it is to most Left parties,

including in a small way the Alliance).³ But for Crewe the central issue was the economy, and on this Labour was clearly at a disadvantage, given the public perception that times were generally good economically. His findings for 1987 thus square quite well with the time-series evidence presented in studies such as Sanders (1996) or Clarke and Whiteley (1990).

By contrast, Heath et al. (1991) focus on short-term political factors, the "deliberate strategies pursued by parties" (p. 220). Among those they identify as important is the fielding of more candidates nationally by the center parties, both through the very formation of the SDP and in terms of decisions by the Liberals. This directly affected support for the major parties and contributed indirectly to other factors, such as tactical voting (Table 13.15, p. 220). Their work also provides a sustained look at the importance of issue effects in the 1987 election.

Despite the strengths of aggregate-level studies, they necessarily downplay issue effects. Good data are available on popularity and, of course, the economy, but similar series on policy positions are not. Yet, Labour's persistent rightward movement over the last two decades has generated many fierce debates within the party, in the press, and in the academic community about its effect on Labour's electability (Shaw 1994, 1996). For some, the 1987 election marked a "major watershed" for the party in terms of its attempts to sell itself to the voters (Harrop 1990; Wring 1996, 116).

Heath et al. (1991) present one of the first systematic looks at the effect of Labour's policy shifts that is specifically grounded in a treatment of issues in the 1987 election. They examine the position of respondents on five 11-point policy scales and assume that voters cast their ballot for the party closest to their own position on each issue. These individual issue distances are then combined into an overall scale, and the same arithmetic calculation is made. As the authors themselves recognize, this accords each issue equal weight and means that issues on which the parties are seen as close together carry less weight in the overall calculation (Heath et al. 1991, 217-8). They conclude that Labour would have garnered 28% of the vote, Alliance 29%, and the Conservatives 43% if voters had chosen the party closest to themselves on the scale Heath et al. computed. It is important that gains to be made by moving issue position are eliminated by the presence of other parties: "A party's fortunes depend not on its absolute position but on its positions relative to other parties" (p. 219, emphasis in original). Hence, Labour could have gained more votes by a shift toward the center, but further gains were limited by the presence of the Alliance (p. 218). Thus, the presence of a third party at the center can have important consequences for the effect of policy shifts on electoral outcomes.

Garrett (1992), in some ways, takes a sophisticated middle path between the two bodies of literature stressing either short-term or longer term forces. He anchors a discussion of the 1987 election firmly in an extensive analysis of class and socioeconomic variables,

² See also Butler and Kavanagh 1988 and more recently Sanders 1996 on the importance of perceptions of Conservative competence in managing the economy.

³ We provide contrary evidence in Table 4, however.

		Conservative	Labour	Alliance	N
Inflation ^e	Decreased	62.5%	12.5	25.0	
	Same	56.4%	21.5	22.1	1,592
	Increased	32.5%	40.8	26.8	1,439
Unemployment ^a	Decreased	59.6%	25.0	15.4	52
	Same	60.2% .	19.5	20.3	1,402
	Increased	30.4%	41.2	28.3	1,538
Taxes*	Decreased	65.9%	17.6	16.5	85
	Same	48.4%	27.0	24.6	2,426
	Increased	23.8%	53.5	22.7	269
Defense ^b	Conservative	64.3%	19.6	16.1	448
	Moderate	56.3%	18.0	25.7	1,491
	Liberal	21.5%	52.9	25.6	1,064
Phillips Curve trade-off	Conservative	72.1%	12.8	15.1	337
	Moderate	64.4%	13.3	22.4	595
	∐berai	35.2%	38.4	26.4	2,078
Redistribution ^b	Conservative	74.9%	6.9	18.1	822
	Moderate	49.7%	22.1	28.2	728
	Liberal	25.2%	48.8	26.0	1,427
Crlme ^b	Conservative	50.8%	27.6	21.6	1,642
	Moderate	44.3%	29.0	26.8	953
	Liberal	23.4%	47.1	29.5	376
Class	White Collar	53.4%	20.6	26.0	1,701
	Blue Collar	34.6%	43.4	22.0	1,323
Gender	Men	44.8%	30.9	24.3	1,454
	Women	45.6%	30.3	24.2	1,602
Total sample		45.2%	30.6	24.2	3,056

Note: Table entries are the percentage of respondents belonging to each row-variable extegory who voted for the designated party. Percentages sum to 100 sorces rows.

"For inflation, unemployment, and taxes, each row corresponds to voters who claimed the economic indicator had decreased, stayed the same, or increased since the 1983 election

^bFor defense, Phillips Curve trade-off, redistribution, and onme, each row corresponds to voter self-placement on the sease. See Appendix A for the complete question wording.

but he argues that the Conservative social reforms shifted the preferences of the electorate as a whole to the right, closer to the Conservative Party. For Garrett, this shift documents that the Conservatives' continued success was predicated on their issue positions and that issue voting was widespread in the 1987 election. He shares the concerns of Heath et al. for social forces, but he also appreciates Crewe's concern for what might be termed relatively short-term factors. In contrast to Crewe, however, who clearly sees retrospective and performance criteria as paramount, Garrett views issue voting as much more central to the Conservative victory.

In sum, the literature is divided over whether issues and retrospective economic evaluations matter much in British elections. In part, the two sides talk past each other because they have no unified model of vote choice that considers multiple parties and that can offer precise estimates of the influence of issues and the economy.

AN INITIAL LOOK AT VOTING IN 1987

The 1987 election offers an especially rich opportunity to test these theories. Since 1945, there have been three viable national parties in British politics, but not until the dual elections of 1974 did the Liberals, in this period the electorally weakest party, obtain roughly 20% of the national vote. In 1983, the Liberal/SDP

combination (the Alliance) garnered 25.4% of the vote, just 2% less than Labour, and it seemed British voters finally had a "real" third choice.

Table 1 presents the percentage of voters who supported each party, along with their demographic traits, economic perceptions, and issue positions. The data are from the 1987 British Election Study (Heath, Jowell, and Curtice 1989). In accord with common class-based theories of British elections, a strong class orientation can be seen in Table 1. The Conservative Party was supported by 53.4% of white-collar voters but only 34.6% of blue-collar workers. Similarly, 43.4% of the latter voted Labour, compared to 20.6% of white-collar workers. There was no large distinction between the two groups in Alliance voting: 22% blue collar and 26% white collar.

We also examine voting behavior according to whether respondents thought inflation, unemployment, and taxes had decreased, stayed the same, or increased since 1983. Economic factors appear to enter strongly into the choice between the Conservatives and Labour but less strongly into the decision to vote for the Alliance. The Conservatives did almost twice as well among those who felt inflation had stayed the same as among those who felt it had increased. Similarly, Labour did almost twice as well among voters who felt inflation had increased as among those who felt inflation had stayed the same. Again, there is very little difference between the two groups in the Alliance

vote: 22.1% versus 26.8%. Similar patterns hold for unemployment and taxes.

The data in Table 1 indicate that issue perceptions also may have played a large role in this election. We present the party percentages based on voters' selfreported views about defense, the relative importance of government efforts to fight either inflation or unemployment, redistribution of income, and crime. Voters to the right on defense spending, relative emphasis on fighting inflation or unemployment (i.e., the Phillips Curve trade-off), redistribution of income, and crime went Conservative, whereas those to the left on these issues were more likely to vote for Labour. The Alliance seems to have been favored about equally by voters who described themselves as moderate or liberal on the issues, and it ran worse among voters who described themselves as conservative on the issues. Unlike recent U.S. elections, there is no gender-gap among the parties. Each did almost identically well among men and women.

These bivariate relationships tell us little about the relative importance of class, economic evaluations, and issue positions. To assess which factors were more important in this election, and to test the various accounts advanced about British elections in general, we need to use multivariate methods and develop a fully specified model of vote choice, which we do in the next section.

MODELS OF MULTIPARTY ELECTIONS

Any model of voter choice in a multiparty setting should allow voters to consider simultaneously all viable parties. It should allow voters to weigh party positions on the issues and consider party performance on the economy. The model also should allow voters of different demographic traits to have different preferences for different parties. It should permit the effects of each independent variable to have a differential influence on the odds that a voter will pick each party, recognizing that the parties use different strategies to gain voter support. Also, and this is especially important in multiparty races, it should not impose the independence of irrelevant alternatives (IIA) property on the voters.

The Π A property holds if the ratio of the probability of voter i choosing alternative C to the probability of voter i choosing alternative L does not depend upon the other alternatives available in the choice set. Technically,

$$\frac{P_{i}(C)|\{S\}}{P_{i}(L)|\{S\}} = \frac{P_{i}(C)|\{T\}}{P_{i}(L)|\{T\}},$$
(1)

where S and T are sets of alternatives, both containing C and L; $P_i(C)|\{S\}$ denotes the probability of the i^{th} voter choosing the Conservatives from choice set S; and $P_i(C)|\{T\}$ denotes the probability of choosing the Conservatives from an alternative set of choices, T. $P_i(L)$ is defined similarly for Labour.

Equation 1 says that given two choices (C and L) the ratio of the probability of selecting C to the probability

of selecting L does not change if the choice set is altered from S to T. In other words, even if S contains only C and L, but T contains C and L as well as ten other choices, the i^{th} individual's relative probabilities of choosing C and L would be unchanged. In the British case, ΠA implies:

$$\frac{P_i(C)|\{C,L\}}{P_i(L)|\{C,L\}} = \frac{P_i(C)|\{C,L,A\}}{P_i(L)|\{C,L,A\}}.$$
 (2)

Or IIA implies that the presence or absence of the Alliance would not change the relative probabilities of any single voter choosing between the Conservative and Labour parties. This is a very strong substantive assumption to make about voters, and it should not be made since it is probably untrue. It suggests that voters are incapable of seeing one party as a substitute for another. For instance, under IIA a voter could not see the Alliance as a substitute for Labour. Let us say a voter has a probability of .5 of voting Labour and a probability of .5 of voting Conservative in a two-party race. When the Alliance is added, if IIA is maintained, the voter could not have a probability of .5 of voting Conservative and a probability of .25 each of voting Labour and Alliance. Yet, such a scenario is plausible for a voter who sees no difference between Labour and the Alliance and views them as identical leftist alternatives.4

Because such a strong substantive assumption should not be made about voters in multiparty elections, we specify and estimate a model of voter choice using multinomial probit that does not impose IIA.⁵ We assume that each voter's utility is composed of an observed, systemic component and an unobserved, random component. The systemic component of utility can be specified with individual-specific variables, such as characteristics of the voter, and choice-specific variables, such as characteristics of the party; this allows a detailed examination in one empirical model of how differences among voters and parties influence voter choice. We assume that the random disturbances have a multivariate normal distribution, and thus we estimate the model with multinomial probit.

We follow the Alvarez and Nagler (1995) implementation of multinomial probit, which assumes that the respondent's utility is a function of choice-specific and individual-specific characteristics:

$$V_{q} = a_{i}\psi_{i} + X_{q}\beta + \varepsilon_{q}, \qquad (3)$$

where:

 V_{ij} = utility of the i^{th} voter for the j^{th} party; a_i = characteristics of the i^{th} voter;

⁴ This is an extreme case, as most voters could distinguish between Labour and the Alliance, but it clarifies the point.

⁵ The multinomial probit technique is relatively new to political science. We use the implementation detailed by Alvarez and Nagler (1995, 1998b), which is also used in Alvarez (1997, 1998) and Alvarez and Nagler (n.d.). Others using the technique are Lacy and Burden (1999) on the 1992 U.S. presidential election; Dow (1997) on French presidential elections; Lawrence (1997) on U.S. presidential primary elections; Quinn, Martin, and Whitford (1999) on Dutch elections, and Schofield et al. (1998) on both Dutch and German elections.

 X_{ij} = characteristics of the j^{th} party relative to the i^{th} voter:

- ψ_j = a vector of parameters relating the characteristics of a voter to the voter's utility for the j^{th} party;
- β = a vector of parameters relating the relationship between the voter and the party (X_{ij}) to the voter's utility for the party; and
- ε_{y} = random disturbance for the i^{th} voter for the j^{th} party.

We estimate one set of β 's (one for each prospective issue) and two sets of ψ 's. One set of ψ 's examines the relative effect of each voter attribute on the likelihood of the voter choosing Conservative over Alliance, and the second set examines the relative effect of each voter attribute on the likelihood of the voter choosing Labour over Alliance.

The flexibility of the multinomial probit technique gives us the opportunity to answer several questions about the 1987 British general election. First, we can determine the effect of a wide array of voter attributes on choices, controlling for many other competing factors. Second, we can examine the dynamics of party positions on prospective issues. Since the multinomial probit technique allows us to consider the position of each party relative to voters, we can see how changes in party position influence expected vote share. We also can see what might occur in British elections if one party—the Alliance—were to drop out altogether. Through such analyses, we can begin to understand how parties compete for the same voters. This lets us ascertain how British voters "group" the parties in the issue space.

Yet, the British electorate presumably did not view these three choices as truly distinct in 1987. Common wisdom held that the Alliance was a substitute for Labour, or at least closer to Labour than to the Conservatives. Despite this common wisdom, few empirical works consider the possibility of choice grouping or the possible violation of the IIA assumption (e.g., see McAllister and Studlar 1992 or Stewart and Clarke

⁶ A complete discussion of the derivation of the three-choice MNP model is given in Alvarez and Nagler (1995) and Hausman and Wise (1978). Beginning with the utility specified in equation 3 for each party, we assume that the disturbances are distributed multivariate normal, with mean zero and covariance matrix Σ . We write the probability that voter ι will choose candidate 1 as:

$$P_{i1} = \int_{-\infty}^{\frac{R_1 - R_{21}}{\sqrt{\sigma_1^2 + \sigma_2^2 - 2\sigma_{12}}}} \int_{-\infty}^{\frac{R_1 - R_{21}}{\sqrt{\sigma_1^2 + \sigma_2^2 - 2\sigma_{12}}}} b_1(\eta_{21}, \eta_{31}; r_1) d\eta_{21} d\eta_{31}, \quad (4)$$

with b_1 being the standardized bivariate normal distribution and r_1 being the correlation between η_{21} and η_{31} :

$$r_1 = \frac{\sigma_1^2 - \sigma_{13} - \sigma_{12} + \sigma_{23}}{\sqrt{(\sigma_1^2 + \sigma_2^2 - 2\sigma_{12})(\sigma_1^2 + \sigma_3^2 - 2\sigma_{13})}}.$$
 (5)

We obtain similar expressions for P_{12} and P_{13} . We assume the disturbance variances are one, which implies that the off-diagonal elements of Σ give correlations between pairs of disturbances. We estimate two of these error correlations in our MNP analyses presented below (we estimated other combinations of error correlations and the results of those estimations are not significantly nor substantively different from those presented here).

1992 on the 1987 British election). It is important that the multinomial probit model we use also allows us to test for the violation of the IIA assumption. If we find that IIA is violated, then we learn something very important about voting behavior. The relative probabilities of a voter choosing between two parties are dependent upon the presence of a third party. This tells us that the party system, the issue positions of parties, and the general electoral context are all important features determining voter decisions in multiparty elections.

A Model of the 1987 Election

We view the voters' utility for each party to be a function of the voter's position on the issues relative to the party and of characteristics of the voter, which we describe below. The inclusion of issue variables allows us to test how much "issues matter" in British elections. given the debate in the literature discussed earlier. Furthermore, the effect of class in British elections has diminished considerably (Crewe 1974; Crewe, Sarlvik. and Alt 1977; Sarlvik and Crewe 1983). Working from the framework of the spatial model of elections, we include variables for the distance between the voter and each party on defense, government emphasis on inflation versus unemployment (i.e., the Phillips Curve trade-off), taxes, redistribution of income, nationalization of industry, crime, and social welfare programs.⁷ The party position on each issue is taken to be the mean of the party placement on the 11-point issue scale by all respondents. We use the absolute value of the difference between respondents' position and the party's position as a measure of issue distance on each item.8

Hand in hand with the realization that issues are replacing class in framing voter decisions in British elections is a growing realization of the effects of voter perceptions of the economy. This has occupied much of the empirical work on British elections (Alt 1979; Lewis-Beck 1988; Studlar, McAllister, and Ascui 1990). Accordingly, we include variables for the voter's perception of recent changes in inflation, unemployment, and taxation levels (McAllister and Studlar 1992). This allows a test of the retrospective voting model in a three-party setting.

⁷ We use the term "Phillips Curve trade-off" to describe the inflation versus unemployment variable throughout this article. The British Election Survey framed the question using the trade-off depicted by the Phillips Curve, asking voters to place themselves and the parties on a scale where one endpoint is prioritizing employment and the other is prioritizing inflation reduction. The question did not explicitly use the term Phillips Curve, since some respondents would not be familiar with it, but the wording described the academic and popular understanding of the macroeconomic trade-off implied by the term. * By using the distance from the respondents' self-placement to the mean placement of each party-rather than the distance from the respondents' self-placement to the respondents' placement of the party—we avoid contaminating our measure with the tendency of respondents to project their favored candidates closer to their own ideal issue position. In another analysis we examined models with both of these measures of the party's position on each issue and found that it did not influence our results appreciably (Alvarez and Nagler 1998b).

TABLE 2. Multinomial Probit Estimates for the 1987 Election, Dependent Variable: Respondent Vote Choice (Alliance Coefficients Normalized to Zero)

Independent Variables	Conservatives/ Allance	Issue Distances	Labour/ Alliance
Defense		14* (.02)	
Phillips Curve			
trade-off		09* (.02)	
Taxation		13* (.01)	
Nationalization	'	14* (.01)	
Redistribution	1	07* (.01)	
Crime		08* (.03)	
Welfare		11 * (.01)	
Constant	.35 (.51)	` '	1.84* (.46)
South	09 (.07)		30* (.09)
Midlands	−.23*`(.0 8 ́)		11 (.10)´
North	12 (. 10)		.43*`(.11)
Wales	49* (.22)		.95* (.18)
Scotland	42 * (.15)		.48* (.15)
Respondent's	` ,		` ´
view of			
Inflation	.23* (.07)		01 (.0 7)
Respondent's		•	
view of			
unemploymer	nt .23* (.04)		.00 (.04)
Respondent's			
vlew of			
taxes	.02 (.04)		07 (.04)
Blue collar	.02 (.09)	•	.47* (.09)
Union member	−.45 * (.07)		.26* (.07)
Public sector			
employee	.08 (.06)		.01 (.08)
Homeownership			37* (.08)
Age	.03 (.03)		16* (.03)
Female	.21* (.09)	·	04 (.08)
Family Income	.06* (.02)		05* (.02)
Education	−.63* (<u>.22)</u>	001 (00)	46 * (.21)
σ _{LA}		.32* (.09)	
σ _{CL}		43 * (.21)	
Number of		0.101	
observations		2,131	
Log-likelihood		1,476.5	

Note: All three economic variables are coded such that respondents who felt that inflation, unemployment, or taxes had decreased since the last election were coded with higher values. Values in the Conservatives/Alliance column indicate the effect of each independent variable to the utility of the respondent choosing the Conservative Party relative to the utility of choosing the Alliance Values in the Labour/Alliance column indicate the effect of each independent variable on the utility of the respondent choosing the Labour Party relative to the utility of choosing the Alliance. To compute the probability of choosing any party given values of the Independent variables, see note 6. Asteriak signifies attainable significance at the .05 level. Standard errors are in parentheses.

We also include demographic measures of the respondents. To allow for the possibility that class did matter in the 1987 election, we have a number of control variables. First, as an indicator of class affiliation, we determine whether the voter occupies a blue-collar job. Union membership has long been considered a staple of Labour support, but it is possible this source has weakened in view of the diminution of the

trade union movement during the first two Thatcher administrations. We include a dummy variable to examine the effect of union membership on voter choice. As a third control for class, we have a dummy variable that measures whether the voter is a public sector employee.¹⁰

Demographic variables other than class have loomed large in research on British elections. The regional divisions in recent voting patterns have spurred a flurry of works, even though the growing North-South political cleavage may be more an artifact of economic divisions than specific to certain regions (Crewe 1992; McAllister and Studlar 1992). We included regional dummy variables to test these assertions. Due to the massive sale of council houses during the Thatcher years, a policy no doubt designed to appeal to the moderately well-off working classes, it is asserted that homeownership played some role in Thatcher's success in 1987 (Crewe 1992). Accordingly, we have a dummy variable for homeownership. We also include measures of the respondent's age, sex, income, and education.

Multivariate Model Results

We present the multinomial probit estimates in Table 2. The dependent variable is respondents' self-reported vote choice. The estimates for the effects of the issue distance variables are given in the first seven rows. The other independent variables follow, arranged in two columns. The first column contains coefficients for the effect of each variable on vote for Conservatives relative to the Alliance; the second column contains coefficients for effects on votes for Labour relative to the Alliance. At the bottom of Table 2 are the estimates for the two error correlations.

These estimates shed substantial light on a number of hypotheses. First, note that all the issue distance variables have statistically significant effects on vote choice. The farther a voter from each party on each issue, the less likely is the voter to choose that party. In addition, some issues (defense, taxation, and nationalization) appear to have a stronger effect than others, especially redistribution, crime, and government policy on unemployment and inflation.

Perceptions of the national economy also affect choices. Perceptions of recent changes in both inflation and unemployment significantly affect Conservative voting relative to Alliance voting. For both variables, the positive sign implies that people who thought inflation or unemployment had improved in the past year were significantly more likely to vote for the Conservatives than for the Alliance. Neither of these variables nor taxes had a significant influence on the choice between Labour and the Alliance. This suggests that voters did not distinguish between the two based

⁹ Categories seven through fifteen of the occupation variable were coded blue collar, see Appendix A for a list of the occupation codes.

¹⁰ We examined alternative operationalizations for class in this election. In particular, we used the Heath-Goldthorpe categorization of individuals into occupation groups (salariat, routine nonmanual, petty bourgeoisie, foremen and technicians, and working class [Heath, Curtice, and Jowell 1985]). This alternative specification did not add explanatory power to our model, and we do not present those results here.

on retrospective economic evaluation. As the retrospective model predicts, the incumbent party was punished or rewarded according to respondents' views of the economy. Apparently, neither the Alliance nor Labour convinced voters that they were the superior alternative on this dimension, as the two challenging parties divided equally the spoils. Notice that this is different from the inference one could draw from Table 1. The bivariate results there suggest that Labour won the votes of people dissatisfied with the economy, but that relationship disappears in a fully specified model.

The variables used to control for class (union membership, public sector employee, blue-collar worker, family income, and education) confirm the posited class bias in voting among the three parties. Union members are significantly less likely to vote for the Conservatives than for the Alliance but are more likely to vote Labour than the Alliance. Family income shows a similar effect; those better off are more likely to vote Conservative than Alliance and more likely to vote Alliance than Labour. Labour had a large advantage over the other two parties with blue-collar voters. ¹¹

Last, we give the estimated error correlations at the bottom of Table 2. These account for factors not included in the systemic component of our multinomial probit specification, which are correlated across parties. The two error correlations are statistically significant: We estimate a positive correlation between Labour and the Alliance (.32) and a negative correlation between the Conservatives and Labour (.43).¹² Methodologically, these significant error correlation estimates demonstrate that the IIA condition is violated, and estimation techniques such as multinomial logit or conditional logit that impose this assumption on the data are likely to produce incorrect inferences about voter decisions in this election. This supports our argument that IIA is a restrictive assumption to make about voters in multiparty elections.

about voters in multiparty elections.

But the multinomial probit estimates of both the random and systemic components of our model are

11 As mentioned in note 10, we used the Heath-Goldthorpe classification of occupations. We reestimated our model with four dummy variables: salariat, petty bourgeoisie, routine nonmanual, and foremen and technicians (working class was the excluded category). A likelihood ratio test between this expanded model and the one presented here produced a χ^2 value of 5.8, which is well below the critical χ^2 value of 15.5 with 8 degrees of freedom. This means that the Heath-Goldthorpe variables do not contribute statistically to the explanatory power of our model. Further examination of the expanded model showed that all coefficients of interest in our analysis—in particular, the estimates for issue distances, retrospective economic evaluations, and the error correlations—are not signifi-

cantly different between these two specifications.

¹² If none of the error correlations were significantly different from zero, then we could not reject an "independent probit" model, in which the utility errors are independent of one another. The independent probit specification is essentially the same as a conditional logit model, except that the former assumes a normally distributed (but independent and identically distributed) error process, and the latter assumes an independent and identically distributed logistic error process. Conditional logit and multinomial probit share the same systemic component of the models, with the utility for each choice conditional upon the characteristics of the choice as well as the voter, but conditional logit assumes independent disturbances. See Alvarez and Nagler (1998a) for further discussion.

based on the assumption of sincere voting in the 1987 British election. Sincere voting occurs when people vote for their most preferred candidate without taking into account that candidate's chances; strategic voting occurs when people vote for a less preferred candidate rather than waste their vote on a candidate with little chance of winning (Abramson et al. 1992). Theoretically, based on the calculus of voting (Downs 1957; McKelvey and Ordeshook 1972; Riker and Ordeshook 1968), there are reasons to suspect that strategic voting occurs, but empirical tests in multiparty or multicandidate elections have always shown that it does not occur to the extent theory predicts.¹³ To test for strategic voting, we reestimated the model in Table 2 with three variables measuring the competitiveness of each party in the voter's constituency in 1983. We expect these coefficients to be negative if voters acted strategically in 1987, since they would measure the unwillingness to vote for parties with little chance of winning the district. Each coefficient was negative, and two (Conservatives and Labour being last in the constituency) were statistically significant. Most important, however, is the fact that the other estimates in our model are not influenced by the addition of the strategic voting variables. Thus, the interpretations of the multinomial probit results given here are not influenced by sophisticated voting behavior.14

We now turn to two other questions about this particular British election and about multiparty politics more generally. First, how can we measure precisely the relative effects of economic factors and issue positions in multiparty elections? Second, how can we understand party competition for voters in the issue space? We answer both with our multinomial probit results in the remaining sections of this article.

Effects of Economics and Issues

As with most discrete choice models, it is difficult to interpret directly the coefficients from the multinomial probit model. The model is highly nonlinear, and the effect of estimated coefficients depends upon the values taken by the other variables and coefficients. Thus, we move to a series of secondary analyses.

Our first illustration of the substantive meaning of the multinomial probit estimates from Table 2 is through "first differences" (King 1989). First, we set all the variables at hypothetical values and determine the probability that this hypothetical voter will support each of the three parties. The hypothetical voter we postulate here has a .34 probability of supporting the Conservatives, a .40 probability of supporting Labour, and a .26 probability of supporting the Alliance. 15 Then

¹³ Various empirical analyses are available: Canadian elections (Black 1978), American presidential primaries (Abramson et al. 1992), and British elections (Cam 1978; Franklin, Niemi, and Whrtten 1994; Galbraith and Rae 1989; Heath, Curtice, and Jowell 1991; Johnston and Pattie 1991; Niemi, Whitten, and Franklin 1992).

¹⁴ This analysis of strategic voting is reported more fully in Alvarez and Nagler (n.d.).

¹⁵ The actual values of the independent variables we use to construct the hypothetical voter influence the baseline probability estimates,

we alter one independent variable at a time and recompute predicted probabilities for the hypothetical voter, with all other variables held at their initial values. The difference between these probabilities is the effect of the change in the independent variable of interest, holding the other variables constant.

The estimated first differences for the three retrospective evaluations of the national economy and for four of the issue distance variables are given in Table 3. At the top are the baseline probabilities for the hypothetical voter. Then, for each economic evaluation variable of interest, we show the marginal effect on support for each party of changing the values of that independent variable from one end of the scale to the other. Note that views on inflation and unemployment had a stronger effect on party choice by our hypothetical voter than did changes in views on taxation. The first differences for change in Conservative support are .24 (inflation), .24 (unemployment), and .06 (taxation). Thus, our hypothetical voter would be .24 more likely to vote Conservative if he thought that either inflation or unemployment had improved in the past year rather than grown worse but only .06 more likely to vote Conservative if he thought taxation had become better over the past year. Notice that these changes in support seem to come equally from Labour and the Alliance. The probability of this voter choosing Labour could change by .13 based on his view of inflation; his probability of choosing the Alliance could change by .11 based on inflation. The results for unemployment are similar. Apparently, neither the Alliance nor Labour convinced voters that it was the superior economic alternative. Again, we find support for the retrospective model: The estimated effect of changes in views on the economy is huge. Yet, we also find that the retrospective model does not illuminate the choice between the two nonincumbent parties. The estimates for issue effects presented next, however, do show how voters chose between the two.

We present similar results for four issues—redistribution, welfare, crime, and defense—in Table 3. We examine the effect on party support of moving the hypothetical voter from the left of the scale to the median voter's position and then to the right of the scale. The results are perfectly consistent with our intuition about the parties' appeal. The likelihood for supporting Labour is highest when the hypothetical voter is to the extreme left on the issue scale. At the median position, he is more favorable to the Alliance than either extreme. At the far right, the likelihood of supporting the Conservatives is maximized.

Although movement from the far left of the scale to the far right is extreme, it helps demonstrate the power of issues in British politics, controlling for class and other demographic effects. These four issues have a dramatic influence on whether the hypothetical voter supports the Conservatives or Labour. Going from the

TABLE 3. Effects of Economics and Issues in the 1987 Election

in the 1987 Election								
	Conservatives	Labour	Alllance					
Baseline	.34	.40	.26					
Inflation								
Better	.52	.30	.18					
Worse	.28	.43	.29					
Difference	.24	13	11					
Unemployment								
Better	.52	.31	.17					
Worse	.28	.43	.29					
Difference	.24	−.12	12					
Taxatlon								
Better	.36	.35	.28					
Worse	.31	. 4 5	.24					
Difference	.06	10	.04					
Redistribution								
More equal	.28	.47	.25					
Medlan (5)	.31	.38	.30					
Less equal	.42	.34	.24					
Difference	−.14	.13	01					
Welfare								
More	.25	.50	.25					
Medlan (4)	.29	.40	.31					
Less	.50	.29	.21					
Difference	- <i>.</i> 25	.21	.04					
Crlme								
Ctvll-rights	.30	.44	.26					
Medlan (8)	.36	.38	.26					
Law and order	.36	.38	.26					
Difference	06	.06	.00					
Defense								
No nukes	23	.54	.23					
Median (6)	.37	.34	.29					
More nukes	.44	.30	.25					
Difference	–.21	.23	02					

Note: Probabilities are evaluated for a respondent with typical values for other variables. The hypothetical voter is set to be at the sample mean distance from each party on each issue. This voter is male, a blue-coller union member, from the north, and owne his home. He is also of mean income, education, and age. Last, his perceptions of the economy were also set to the sample mean values. Probabilities are computed with the respondent first set at one extreme on the variable of interest, then at the other extreme. For issue variables, we also include a row with the respondent at the median position on that issue. The value of the median position is given in parenthesis.

left to the right on each of these issue scales increases the probability of support for the Conservative candidate by as much as .21. Defense has the strongest effect, followed by welfare, redistribution, and crime. Notice that there is little change in the probability of choosing the Alliance as the voter moves from the left to the right on these four issues. The Alliance had little appeal to voters at either extreme, but the hypothetical voter's proximity to the center has a major effect on his choice between the Alliance and Labour.

Table 4 displays the mean positions of the parties and respondents on each of the seven issue scales. These are 11-point scales, and the party positions (an average taken from voter placements of the parties in the survey) show a wide gap between the Conservatives and Labour in this election. On welfare and defense (the two issues with the largest effect in Table 3), the difference between them is more than 3 points on these

but they do not have much influence on the important quantities—the probability differences. In other words, different hypothetical voters still would produce the probability differences we present in Table 3.

TABLE 4. Mean Placement of Parties and Selves by Respondents, 1987

	Conservative	Labour	Allance	Respondents
Defense	8.0	2.1	4.9	5.2
ı	(3,526)	(3,533)	(3,062)	(3,615)
Philips Curve	6.4	2.3	3.8	3.5
trade-off	(3,545)	(3,541)	(3,258)	(3,619)
Taxatlon	7 .2	3.0	4.4	4.4
	(3,510)	(3,478)	(3,162)	(3,580)
Nationalization	9.2	2.9	5.6	6.4
	(3,490)	(3,446)	(3,000)	(3,519)
Redistribution	8.4	2.9	4.8	5.0
	(3,512)	(3,495)	(3,141)	(3,586)
Crtme	7.5	5.6	6.3	7.7
	(3,415)	(3,337)	(3,059)	(3,578)
Welfare	7.9 ·	2.6	4.3	4.5
	(3,552)	(3,519)	(3,209)	(3,600)

Note: Entries for the first three columns are the mean placement of the parties on the 11-point scale by all respondents giving a valid response. Column four is the mean self-placement of the respondents. Numbers in parentheses are the sample size on which each cell is based.

scales.¹⁶ For crime, the issue with the least effect in Table 3, notice that the parties staked out very similar positions, separated by only about one point.¹⁷

There are implications here for the theoretical literature about American national elections. In early debates by Shepsle (1972) and Page (1978), both maintained that in the context of different models of campaign dynamics, candidates and campaigns often have the strategic incentive to obfuscate their positions to the electorate, echoing work by Key (1966). If the voters cannot perceive a distinction between the parties on certain issues, then it is difficult for those issues to factor strongly in voter decision making (Alvarez 1997). Here we see strong support that view, since the issues on which the parties are most separated are the issues with the most influence on our hypothetical voter. We also see that on every issue except crime the Alliance was closer to the mean respondent position than were either of the other two parties.

The Effect of the Alliance

Since the multinomial probit estimates can be used to predict the probability of an individual choosing each

16 Consistent with common wisdom, by 1992 Labour had moved much closer to the mean on many issues. The British Election Study that year used a 7-point scale on three issues (defense, welfare, and women's rights) rather than the 11-point scale used on all other issues in 1992 and on all issues in 1987. To facilitate comparison we compared the ratio of the distance from Labour to the mean respondent's position to the standard deviation of all respondents' opinions on an issue. For every measure, Labour moved closer to the mean. On defense, it went from 1.19 standard deviations to the left of the mean to only .44 standard deviations to the left.

¹⁷ The issue placements in Table 4 illustrate the error, made m many analyses, of assuming that the Alliance party lies exactly between the Conservatives and Labour (cf. McAllister and Mughan 1987; Mishler, Hoskin, and Frtzgerald 1989). On some issues such as defense, the Alliance stands virtually at midpoint, but on many social issues—examples here are redistribution and welfare—it is much closer to Labour than the Conservatives. Incorrect assumptions about party positions clearly will lead to inaccurate understanding of British elections.

of the three parties, we can also predict the aggregate vote share of each party. More important, the estimates allow prediction of aggregate vote share in a two-party race with the Alliance omitted. We can use the estimates from the systemic component of utility for the remaining two parties as well as the information in the estimated correlations between the disturbance terms to determine the probability of each voter choosing Labour or the Conservatives. This allows us to estimate the effect of the Alliance on the election. We predict a split of 44.9%, 29.8%, and 25.3% among the Conservative Party, Labour, and the Alliance in a three-way race, and this would give Labour a 39.9% share of the two-party vote between the Conservative Party and Labour. If the Alliance had not been present, leaving a two-way race between the Conservative Party and Labour, we predict a 57.4%-42.6% split favoring the Conservative Party over Labour. The standard errors on each of these numbers is less than 1%. Removal of the Alliance helps Labour somewhat, but to see its full effect on the election we need to take a more dynamic view and consider the behavior of the parties as well as the voters. In the next section we simulate the effects of changes in issue position by the three parties. This offers some insight into post-1987 elections, as Labour in fact changed positions, particularly after its failure in 1992.

The Issues

Changes in respondents' issue positions may not be as interesting to consider as changes in parties' positions. A major criticism of the Labour Party is that in the 1980s it took stands that placed it far from the center of the distribution of voters. What would happen if Labour convinced the electorate that it was more moderate? Could the party have done better in 1987 by moving to more moderate positions? To find out, we simulated movement of Labour's perceived position on each of the seven issues, one issue at a time. We then recomputed the issue distance from each respondent to Labour and calculated new vote shares for each party, with Labour at the postulated position.

We perform these calculations both with the Alliance present and under the counterfactual scenario of a two-party race between Labour and the Conservatives. As the simulation will show, the effects of movement by the parties are much smaller in a three-way race than in a two-way race. In the first portion of Table 5 we show, for a three-way race, for each issue, the position at which Labour would have received its lowest vote share, its actual (mean perceived) position, and the position that would have maximized its vote share (indicated by Labour-Max*). We also show the predicted vote share of all three parties had Labour been perceived at Labour-Max* on each issue, as in the final row of the table we show the predicted vote shares for the parties had Labour moved to Labour-Max* simultaneously on all seven issues. Labour's predicted vote share based on its actual perceived issue positions was 29.8%. According to Table 5, the best Labour could do by a shift on any one issue would be a vote

TABLE 5. Issue	Simula	ations								
					For Labor Support In a Three-Party Race			For Labour Support in a Two- Party Race (Alliance Removed)		
Labour		Vote Shares at Labour-Max*			Labour-	Vote Shares at Labour-Max ₂ *				
lssues	Mln	Actual	Max*	Conservative	Labour	Allance	Max ₂ *	Conservative	Labour	
Defense	11.0	2.1	5.0	44.0	31.9	24.1	6.0	55.0	45.0	
Phillips Curve										
trade-off	11.0	2.3	1.0	44.9	30.0	25.0	2.0	57.4	42.6	
Taxation	11.0	3.0	4.0	44.9	29.8	25.3	4.0	57.2	42.8	
Nationalization	11.0	2.9	6.0	43.9	32.4	23.7	6.0	54.6	45.4	
Redistribution	11.0	2.9	3.0	45.0	29.8	25.2	4.0	57.3	42.7	
Crlme	1.0	5.6	8.0	44.7	30.4	25.0	8.0	56.9	43.1	
Welfare	11.0	2.6	3.0	44.9	29.8	25.2	3.0	57.3	42.7	
All Issues				42.0 -	36.2°	21.8ª		50.2 ^b	49.8 ⁶	
Standard deviation				0.88	1.08	0.98		1.19	1.19	

Note Labour-Max* is the position Labour would have to be perceived to take on each issue to get the maximum vote share in a three-party race. Labour-Max* is the position Labour would have to be perceived to take on each issue to get the maximum vote share in a two-party race. "Estimated vote shares if Labour were perceived to be at Labour-Max* for all seven issues.

share of 32.4% by moving from the left to the center on the nationalization issue. If Labour managed to shift public perception of its positions on all seven issues, then it would receive a vote share of only 36.2%; this is an increase of 6.4% but would still leave Labour almost 5% behind the Conservatives. The prediction is quite precise: The standard error of the predicted vote share for Labor is only 1.08%. Thus, Table 5 provides guidance as to where Labour should move on the issues, but it is clear that issue positions alone cannot account for Labour's poor electoral fortunes.

The apparent magnitude (or lack thereof) of these issue effects might be surprising in view of the estimates presented in Table 3. For instance, we predict that a voter who changes his view on defense could increase his probability of voting for Labour by .24; yet, Table 5 indicates that Labour will gain only 2.1% by moving toward the center on defense, not 24%. This is because the effect of a movement by a party is limited by two factors. First, a shift toward some voters necessarily means a shift away from other voters. Second, the issue space is crowded. Labour is competing with two other parties, and any movement toward the center will leave it closer to another party with which it must share voters.

The second problem described above might lead supporters of Labour to hope for better results in a less crowded issue space with no viable centrist party. The multinomial probit estimation technique allows us to predict the probability of a voter choosing Labour or Conservative without the Alliance as an option. As stated earlier, we can use the information obtained from the multinomial probit estimates, the effect on the voter's utility of his/her distance from the parties, as well as the correlation among disturbances to estimate voter behavior in the absence of the Alliance. For the

second portion of Table 5, we performed simulations identical to those in the first, moving Labour on each issue and reestimating vote shares to find Labour's optimal position, the position at which it would maximize its vote share (Labour-Max₂) in a two-party race without the Alliance. As reported in Table 5, Labour's maximum share of the vote in a two-way race by moving on any single issue is 45.4%. Movement on all seven issues raises Labour's expected hypothetical vote share to 49.8%, with a standard deviation of 1.2%. Note that this is significantly higher than Labour's largest share of the Labour-Conservative portion of the three-party vote reported in the first portion of Table 5 (36.2/(36.2 + 42.0) = 46.3%). Thus, we very clearly see the problem the Alliance caused for Labour. By positioning itself in the center, the Alliance not only took votes from Labour but also minimized the ability of Labour to improve its vote share by strategic movement on the issues.

Table 6 presents results of identical simulations focusing on the Conservatives. In the first portion, our estimates reveal that the Conservatives took a more conservative stance than electorally necessary on all issues but crime. Thus, slight moderation in their message in 1987 would have produced small vote share gains for them, ranging from just under 1% for a centrist shift on nationalization and income redistribution to almost 3% for moderation on defense. Notice also the combined effect of moving the Conservatives to their optimal position on all the issues at once: They win with 61% of the vote. Finally, as shown in the second portion of Table 6, in a two-way race against Labour with the Conservatives at their best position on each issue, they score overwhelming victories on each issue.

Similar simulations for the Alliance in a three-way race are reported in Table 7. On every issue other than crime it managed to position itself closer to the public than either the Conservatives or Labour. Thus, it had

Estimated vote shares if Labour were perceived to be at Labour-Max₂ for all seven issues.

¹⁸ The standard error of the predicted vote share is obtained by sampling over the parameter values using the estimated distribution and computing the predicted share for Labour over 1,000 such trials.

TABLE 6. Iss	ue Simi	ulations							-
					servative Su hree-Party F		For Conserv Party Vote (
	(Conservath	/e	,	Vote Shares Cons-Max				hares at s-Max ₂ *
Issues	Mln	Actual	Max*	Cons.	Labour	Alliance	Cons-Max*	Cons.	Labour
Defense	11.0	8.0	6.0	48.0	29.0	23.0	6.0	59.8	40.2
Phillips Curve									
trade-off	11.0	6.4	3.0	47.1	28.9	24.0	2.0	59.7	40.3
Taxation	11.0	7.2	5.0	47.4	29.0	23.6	4.0	59.7	40.3
Nationalization	1.0	9.2	6.0	47.1	28.8	24.1	6.0	60.1	39.9
Redistribution	11.0	8.4	6.0	46.1	29.3	24.5	4.0	58.8	41.2
Crlme	1.0	7.5	8.0	45.1	29.8	25.2	8.0	57.5	42.5
Welfare	11.0	7.9	4.0	47.5	28.7	23.8	3.0	60.1	39.9
All Issues				61.4	22.6ª	16.0°		73.1 ^b	26.9 ^b
Standard dev.				1.34	0.91	1.11		1.28	1.28

Note: Cons-Max* is the position at which the Conservative Party would have to be perceived on each leave to get the maximum vote share. Cons-Max* is the position at which the Conservative Party would have to be perceived on each leave to get the maximum vote share in a two-party race. "Estimated vote shares if the Conservative Party were perceived to be at Cons-Max* for all of the seven issues.

less to gain by modifying positions, and in Table 7 its actual position is quite close to its optimal position on most issues. The delicate balancing act of the Alliance is apparent in the second and third columns of the table. It was slightly to the left of its optimal position on defense, nationalization, and crime, and it was slightly to the right of its optimal position on the Phillips Curve, taxation, redistribution, and welfare issues. Had the Alliance taken its optimal position on any particular issue, it never would have exceeded the vote share of Labour. An optimal position on all issues—that is, even more delicate balancing between the other parties—would have put the Alliance very close to Labour's share (less than one percentage point) but still behind.

The Economy

Parties, especially the incumbent, have other strategic levers than issue positions. There has long been debate over whether the economy matters in national elections and whether the incumbent party has sufficient tools to manipulate the macroeconomy for electoral purpose (Alesina 1987; Alt 1985; Alvarez, Garrett, and Lange 1991; Hibbs 1977; Lewis-Beck 1988; Tufte 1978). As was shown in tables 2 and 3, variables measuring the voter's retrospective evaluation of recent economic performance mattered greatly in the 1987 British election, controlling for issue, class, and other demographic effects.

Yet, these static results tell us little about the dynamics of the macroeconomy and voter preferences. We would like to know what to expect if overall macroeconomic conditions change. In Table 8 we give simple survey frequencies on the three types of economic retrospective evaluations in 1987 and 1992. We are interested in how the British economy appeared to voters in 1987 relative to another benchmark; for this we chose the 1992 election, since the questions in the 1992 British Election Study are almost identical to those in 1987.

The distributions of public opinion shifted dramatically on inflation and unemployment. In 1992, many more British voters than in 1987 felt that the economy had worsened considerably since the previous general election; 19% more believed inflation had increased a

		Alliance		Vote Shares at All-Max*		x*
Issues	Mln	Actual	Max*	Conservative	Labour	Alllance
Defense	11.0	4.9	6.0	44.4	30.1	25.6
Phillips Curve trade-off	11.0	3.8	2.0	45.0	28.9	26.0
Taxation	11.0	4.4	4.0	45.0	29.6	25.4
Nationalization	1.0	5.6	6.0	44.5	29.7	25.7
Redistribution	11.0	4.8	4.0	45.1	29.6	25.3
Crlme	1.0	6.3	8.0	44.6	29.7	25.7
Welfare	11.0	4.3	3.0	45.3	29.2	25.5
All-Max* on all issues				44.3 °	28.1	27.6ª
Standard deviation				0.78	0.81	0.96

Note: All-Max* is the position at which the Alliance would have to be perceived on each listue to get the maximum vote share.
Estimated vote shares if the Alliance were perceived to be at All-Max for all seven lesues.

Estimated vote shares if the Conservative Party were perceived to be at Cons-Max's for all of the seven Issues.

		1987			1992		
	Prices	Unemployment	Taxes	Prices	Unemployment	Taxes	
Increased a lot	48.7%	52.0%	10.8%	67.7%	76.0%	23.6%	
increased a little	43.6	21.8	25.8	28.9	16.6	34.4	
Stayed the same	5.2	9.0	19.7	2.0	3.5	17.8	
Fallen a little	2.2	15.5	40.6	1.2	2.7	21.7	
Fallen a lot	0.3	1.7	3.0	0.2	1.2	2.5	
	100%	100%	100%	100%	100%	100%	
N	3,798	3,734	3,442	1.600	1,586	1.518	

lot, and 24% more felt the same way about unemployment. Even taxes were perceived as increasing, and 12.8% more voters believed they had risen a lot since the last election.

This implies that, relative to 1992, the economy in 1987 was more favorable to the incumbent Conservatives, and that may have enhanced their victory that year. We can see the effect of the economy in 1987 by using the multinomial probit results to simulate predicted vote shares for the three parties under the scenario of a 1987 economy as bad as it appears to have been in 1992.

The series of simulations in Table 9 change the aggregate distribution of economic perceptions among voters in 1987 to match those held by voters in 1992. We first show the sample distribution of vote shares in 1987 and then simulate the vote for altered perceptions of inflation, unemployment, and taxes. The result is surprising. If the economy had been "bad" for the Conservatives in 1987 (i.e., with voter evaluations like those in 1992), the Conservatives still would have won the 1987 election. The difference between the first and last row in Table 9 shows only a 3.7% decline in the Conservative vote, which is split between Labour (a gain of 1.5%) and the Alliance (a gain of 2.2%). The state of the macroeconomy was an important predictor of which party individuals supported in 1987, but it does not seem to have determined the outcome of the 1987 British election. Had the economy been as bad as it was in 1992, the Conservatives still would have won more than 40% of the vote.

TABLE 9. Economic Simulation: Estimated 1987 Vote Outcomes with 1992 Economic Perceptions

,	Conservative	Labour	Alilance
1987 survey	45.0%	29.9	25.1
1992 Inflation	44.0%	30.1	26.0
1992 unemployment	42.6%	30.4	27.0
1992 taxation	44.6%	30.5	24.9
1992 Inflation/			
unemployment	41.6%	30.6	27.7
1992 Inflation,			
unemployment,			
and taxes	41.3%	31.3	27.4
Standard deviation	0.97	0.88	1.07

Note Entree for all rows except the first are predicted vote shares after setting the distribution of respondent opinions on the economy to match 1992 views. N=2,131.

The 1992 Election

The 1992 election in Britain was contested under much different circumstances than was the 1987 election. It was the first post-Thatcher election. John Major was the Conservative leader, so the argument that Thatcher's personal appeal could help (or hurt) the party was no longer relevant. Also, as we demonstrate below, Labour had continued its move toward the center on the issues. In addition, the three-party structure was even clearer: The Liberals and the Social Democrats had formalized their 1987 Alliance by merging into the Liberal Democrats. Perhaps most important, the economy was faltering, which should have given a significant boost to Labour. The Conservatives attempted to nullify Labour's potential advantage by arguing that the country was the victim of a worldwide recession, and a Labour government would only compound the economic problems with additional taxes.

Our analysis of the 1987 election provides insights into the 1992 election. In Table 10 we show, for both 1987 and 1992, the mean actual issue placement of the parties and respondents, as well as the optimal placement for the parties in 1987. We can see that respondents perceived changes in the parties' issue positions. Labour was more than 2 points away from its optimal position on two issues in 1987: defense and nationalization. In both cases it moved significantly toward its optimal position (i.e., toward the mean of all voters) in 1992. The Conservatives were more than 2 points away from their optimal position on all six issues in 1987. They only moved significantly closer to their optimal on three of those issues in 1992: defense, nationalization, and welfare. Although this might be viewed as a less concerted effort than that of Labour to achieve an optimal position, the movement by the Conservatives nonetheless represented a larger aggregate move than Labour made. Thus, on balance, we would expect the shifts by the two parties to favor the Conservatives.

We performed an out-of-sample test of the 1987 model by using it to predict the 1992 election. We compared our predictions based on simulated movements by the parties on the issues, both with and without simulated changes in economic perceptions, to what actually happened in 1992. First, we report predictions assuming no change in economic perceptions. Rows 2–5 of Table 11 show what could have been expected in 1992 if perceptions of the economy remained fixed at their 1987 positions but the parties

TABLE 10. Mean Placement of Parties and Selves by Respondents, 1987 and 1992, and Optimal Party Placements for 1987

	Conservatives		Labour			∐b. Dem.	Respondents_		
	1987 Actual	1992 Actual	1987 Optimal	1987 Actual	1992 Actual	1987 Optimal	1992 Actual	1987 Actual	1992 Actual
Defense	8.0	6.1	6.0	2.1	3.9	5.0	4.8	5.2	5.1
Phillips Curve trade-off	6.4	6.4	3.0	2.3	3.0	1.0	4.2	3.5	3.5
Taxation	7.2	7.1	5.0	3.0	2.8	4.0	4.0	4.4	4.1
Nationalization	9.2	8.4	6.0·	2.9	3.6	6.0	5.4	6.4	5.6
Redistribution	8.4	7.9	6.0	2.9	3.1	3.0	4.6	5.0	4.5
Crlme	7.5		8.0	5.6		8.0	_	7.7	_
Welfare	7.9	7.1	4.0	2.6	3.3	3.0	4.3	4.5	3.7
European coop.	_	5.6		_	5.2	_	4.9	_	5.8
Women's rights	_	4.2		_	3.7	_	3.9	_	2.7

Note: The 1992 7-point scales for defense and welfare have been converted to 11-point scales by folding around the midpoint (4) on the Initial 7-point acale and rescaling to 11. Respondents were not asked about crime in 1992, and were not asked about European cooperation or women's rights in 1987.

moved on issues to their actual 1992 positions. In those rows we see that if only Labour had moved to its 1992 positions on the issues and the other parties had remained fixed at their 1987 positions, then Labour would have picked up an additional 2.2% of the vote compared to the report of the 1987 vote. But we know from Table 10 that the Conservatives moved more aggressively on the issues in 1992, so it is no surprise that we predict the Conservatives will increase their share by 4.8% when all three parties move to their actual 1992 positions (see row 5). In this case, Labour gains only 0.4% of the vote. Both Labour and the Conservatives gain at the expense of the Alliance. This is the estimated effect of the movement by the parties on the issues in 1992, isolated from the effects of changes in economic perceptions and other factors.

The next four rows of Table 11 show our expectations for 1992 based on 1992 issue positions of the parties and 1992 economic perceptions by the respondents. By computing estimated vote shares when all three parties move to their 1992 issue stance and the economy is perceived as it was in 1992, we can make the following predictions about the 1992 election based on our 1987 model estimates. First, the Conservatives should gain only a very modest increase in vote share, less than 1%. Second, Labour should obtain a higher vote total based on better issue positions and the state of the economy, with its vote rising by 2.4%, to 32.3%. Last, the Alliance is the net loser, since its vote is predicted to drop by 3.3%.

What really happened in 1992? The 1992 survey results are given in the last row. We correctly predicted the Conservative share of the vote (within the standard error). We also correctly predicted that Labour would gain votes and that this would come at the expense of the Alliance/Liberal Democrats. Yet, we underestimated the magnitude of Labour's gain, which we predicted at 32.3% of the 1992 vote, compared to its showing of 37.1% in the survey.

To account for the drop in Liberal Democratic

TABLE 11. Predicted Vote Shares for 1992 Based on 1987 Model

	Three-Party Vote Shares			Two-Party Vote Share	
	Conservative	Labour	Alliance	Conservative	Labour
1987 survey ^e	45.0	29.9	25.1	57.5	42.5
Economic perceptions fixed at 1987 values					
Labour at 1992 position ^b	44.0	32.1	23.9	54.8	45.2
Conservatives at 1992 position ^b	50.3	28.2	21.5	61.7	38.3
Alllance at 1992 position ^b	45.4	29.9	24.7		
All parties at 1992 position	49.8	30.3	19.9	59.3	40.7
(standard error of prediction)	(0.9)	(0.9)	(0.9)	(1.1)	(1.1)
Economic perceptions adjusted to 1992 values	` '	• •	• •	• •	. ,
Labour at 1992 position ^b	40.3	34.0	25.8	51.6	48.4
Conservatives at 1992 position ^b	46.5	29.8	23.7	58.9	41.1
Alllance at 1992 position ^b	41.8	31.5	26.8	54.6	45.4
All parties at 1992 position	45.9	32.3	21.8	58.2	43.8
(standard error of prediction)	(1.2)	(1.0)	(1.2)	(1.3)	(1.3)
1992 survey	45.2	37.1	17.1	` '	. ,

Note Entries are predicted vote shares after setting the 1987 party positions to match the observed 1992 positions. Rows 2-4 move one party at a time to the 1992 position. The fifth row moves all three parties to their 1992 positions. The last four rows move parties to their 1992 issue positions, while giving respondents the 1992 economic perceptions distribution. N = 2,131.

^bThe other two parties are fixed at their 1987 issue positions.

[&]quot;This is the distribution of the 2,131 voters in 1987 for whom we can compute estimates

support relative to Labour, we estimated a model for the 1992 election similar to our 1987 model.¹⁹ The results are reported in Table 12.²⁰ A comparison of the 1992 parameter estimates with those from the 1987 election (Table 2) reveals the economy was one factor that helped Labour gain votes in 1992 relative to the Liberal Democrats. In 1992 each of the economic perception coefficients is larger for Labour relative to the Liberal Democrats than the corresponding coefficient for the 1987 model for Labour relative to Alliance (Table 2). Thus, the economic dissatisfaction in 1992 contributed more to Labour than we had anticipated based on the 1987 results.

CONCLUSION

We have uncovered factors that drive the decision of the British voter in choosing simultaneously among three parties. In 1987, retrospective economic evaluations of individual voters were crucial in determining the likelihood of voting for the Conservatives but had very little effect on the choice between Labour and the Alliance; rather, issue positions had a huge influence on the likelihood of voting for Labour. Voters supported the incumbent party primarily based on economic performance, and the choice between the other two parties was based on issues.

The Alliance took more votes from Labour than from the Conservatives. More important, the Alliance severely limited Labour's strategic options and left it almost no chance of winning in 1987. The Alliance simply crowded Labour in the issue space. Even a dismal economy and movement to the correct position on the issues would not have helped Labour with the Alliance in the race. Of course, our analysis also reveals that Labour did not help its own cause by choosing issue positions so far to the left in 1987. Our analysis gives a measure of how valuable centrist positions would be to Labour in the absence of a viable third party. What does this say about "short-term" versus "long-term" effects? It says they both matter. Our contribution here is to measure the short-term effects and demonstrate their magnitude. Similar analyses of

TABLE 12. Multinomial Probit Estimates, from Imputed Data Sets for the 1992 Election (Liberal Democratic Coefficients Normalized to Zero)

to Zero)			
Independent	Conservatives/	Issue	Labour/
Varlable	LDP	Distance	LDP
Defense	_	-0.14* (0.06)	
Phillips Curve		, ,	
trade-off		-0.01 (0.02)	
Taxation		-0.11*(0.02)	
Nationalization		-0.06*(0.01)	
Redistribution		-0.06*(0.02)	
Welfare		-0.10* (0.02)	
European coop.		-0.19 (0.09)	
Women's rights		-0.16 (0.13)	
Constant	1.80* (0.73)		5.94* (0.89)
South	-0.38* (0.16)		-0.83* (0.22)
Midlands	0.11 (0.17)		-0.13 (0.21)
North	-0.16 (0.16)		0.12 (0.20)
Wales	-0.19 (0.27)		0.58 (0.33)
Scotland	-0.15 (0.16)		0.08 (0.20)
Respondent's			
v lew of			
Inflation	0.16** (0.10)		0.15 (0.11)
Respondent's			
vlew of			
unemployment	0.09 (0.07)		0.10 (0.0 9)
Respondent's			
view of taxes	0.10** (0.06)		-0.20* (0.07)
Blue collar	0.06 (0.04)		-0.15* (0.0 5)
Union member	-0.25* (0.12)		0.38* (0.13)
Women	0.20* (0.09)		-0.15 (Ò.11)
Age	0.04 (0.03)		-0.15* (0.0 4)
Homeownership	0.04 (0.14)		-0.51* (0.16)
Family Income	0.03* (0.01)		-0.07* (0.02)
Education	-1.05* (0.26)		-1.55 * (0.32)
σ_{CA} .		0.33 (0.24)	
σ _{LA} Number of		-0.64 (0.47)	
observations	0.4	100	
Log-likelihood	•	103 313.62	
TO TIME TO CO	- 10	10.02	

Note: Values in the Conservatives/LDP column indicate the effect of each independent variable on the utility of the respondent choosing the Conservative Party relative to the utility of choosing the LDP. Values in the Labour/LDP column indicate the effect of each independent variable on the utility of the respondent choosing the Labour Party relative to the utility of choosing the LDP. To compute the probability of choosing any party given values of the independent variables; see note 6. *p=.05, **p=.10. Standard errors are given in parentheses.

British elections over time (a project beyond the scope of this article) could offer similar measurement of the long-term factors.

Our findings show that retrospective economic evaluations and prospective issue voting matter greatly in British elections. The translation of both models of voting from their usual two-party setting to the more complicated multiparty scenario shows their resilience and the differences between the two types of political settings. In the multiparty situation, past economic performance can be used by voters when deciding whether to support the incumbent party, and prospective issues factor heavily into the calculations by both parties and voters. Issue positions are the criteria used

¹⁹ This required imputing issue positions for respondents because the 1992 survey used a split-sample design: Half the respondents were asked only about defense, welfare, and womens' rights; and the other half of respondents were asked only about unemployment/inflation, taxation, nationalization, redistribution, crime, and European cooperation. We used the software program Amelia (Honaker et al. 1999) for the imputation. Respondents' issue positions were imputed based on all other individual characteristics used in the model of vote choice. Before implementing the imputation procedure, we dropped respondents who failed to answer all issue questions that they were asked. Thus, since respondents' opinions were omitted by the survey design and not by the respondents' choice, we can be quite certain that the opinions are missing completely at random. We imputed five data sets, and the complete estimation results for this analysis are available from the authors and at the APSR web site. For general references on imputation, see Schafer 1997, Rubin 1987, and Little and Rubin 1987.

²⁰ We report the mean of the parameter estimates across the five estimations; the standard errors are computed accounting for both the estimation uncertainty and the uncertainty across the imputed data sets, as described in King et al. 1998.

by voters to decide which nonincumbent party to support.

These findings come as the consequence of a significant methodological advance. We used a technique relatively new to the study of multiparty elections, multinomial probit, which enabled us to study voter choices for the three major parties in the 1987 British election simultaneously and without restrictive and erroneous assumptions about the parties and the electorate. The multinomial probit technique also provides a unique perspective on the dynamics of multiparty politics, since we can determine the effects on voters of changes in party positions. We also can determine the effects of a change in the party system on voter choices, in this case removal of the Alliance from the election. The ability of the multinomial probit technique to study these components of party politics is unique, since none of the alternatives advanced in the literature can answer these questions.

Similar to previous work, we focus on the role of prospective and retrospective issues in the 1987 election, but we differ by examining both effects simultaneously as well as other influences on voter choice—region, class, and other social attributes of individual voters. Our finding that prospective and retrospective issues have such a strong effect on voters, while controlling for region, class, and demographic characteristics, should end the debate about whether issues and the economy "matter" in British elections. Instead, the focus should shift from whether to how much and to how their influence in particular elections compares to their influence on other British elections, and to elections in other nations. Only then can British elections be placed in their appropriate comparative context.

This work has general implications for the study of other multiparty systems, which usually are seen as qualitatively distinct from two-party systems (Duverger 1954; Sartori 1976). Ever since the collapse of the Weimer Republic, political science has been captivated with the notion that two-party systems imply stability whereas multiparty systems invite fluidity, and hence chaos.21 With very few exceptions, however, most approaches to the study of voter behavior in multiparty systems suppress the very multipartyism that makes such systems interesting. In these systems we do not expect parties to converge to the center, so the strategic calculation of parties about where to position themselves on the issues relative to the voters is crucial for understanding the politics of these systems. Parties are also more likely to leave or enter multiparty systems. We have demonstrated that the presence of a third party crowding the issue space increases the likelihood that an extreme party will win the election. We have also shown that when a party drops out of a multiparty system, the remaining parties have an opportunity to redefine themselves in the issue space to appeal to a larger set of voters.

APPENDIX A: SURVEY QUESTIONS FROM THE 1987 BRITISH ELECTION STUDY

[Q22a] Since the last general election in June 1983, would you say that *prices* have increased or fallen? Please choose a phrase from the card:

- (1) Increased a lot
- (2) Increased a little
- (3) Stayed the same
- (4) Fallen a little
- (5) Fallen a lot
- (8) (Don't know)
- [Q22b] What about unemployment?

[Q22c] Taxes?

Author's Note: In questions 28, 29, 34, 35, 39, and 40, items (b), (c), and (d) are identical to the wording in question 23 and are thus not reproduced below. For each of these questions, box A was coded as 1, box K was coded as 11, and boxes C through J were coded as 2 through 10.

[Q23] Defense:

Some people feel that government should get rid of all nuclear weapons in Britain without delay. These people would put themselves in box A.

Other people feel that government should increase nuclear weapons in Britain without delay. These people would put themselves in box K.

And other people have views somewhere in-between, along here or along here.

(a) In the first row of boxes, please tick whichever box comes closest to *your own* views about nuclear weapons in Britain.

Now where do you think the Conservative and Labour parties stand:

- (b) First the Conservative Party. In the next row of boxes, please tick whichever box you think comes closest to the views of the *Conservative Party*.
- (c) Now in the next row, please tick whichever box you think comes closest to the views of the Labour Party.
- (d) And finally, please tick whichever box you think comes closest to the views of the SDP/Liberal/Alliance? [Q28] Unemployment vs. Inflation (Phillips Curve trade-off):

Some people feel that getting people back to work should be the government's top priority. These people would put themselves in box A.

Other people feel that keeping prices down should be the government's top priority. These people would put themselves in box K.

And other people have views somewhere in-between, along here or along here.

(a) In the first row of boxes, please tick whichever box comes closest to *your own* views about unemployment and inflation.

Now where do you think the Conservative and Labour parties stand:

[Q29] Taxation and Government Services:

Some people feel that government should put up taxes a lot and spend much more on health and social services. These people would put themselves in box A.

Other people feel that government should cut taxes a lot and spend much less on health and social services. These people would put themselves in box E.

²¹ One articulation of this view is Epstein 1967. In general, it is argued that party system attributes (such as the number of parties) are strong factors in determining a nation's political stability (Almond 1960; Duverger 1954; Huntington 1968, Lipset and Rokkan 1967; Powell 1981, 1982; Sartori 1976) More recently, Bartolini and Mair (1990, 143) argued: "The number of parties matters, and variance in this number contributes substantially to the explanation of variance m electoral stability."

(a) In the first row of boxes, please tick whichever box comes closest to *your own* views about taxes and government spending.

[Q34] Nationalization:

Some people feel that the government should nationalize many more private companies. These people would put themselves in box A.

Other people feel that government should sell off many more nationalized industries. These people would put themselves in box E.

(a) In the first row of boxes, please tick whichever box comes closest to *your own* views about nationalization and privatization.

[Q35] Redistribution:

Some people feel that government should make much greater efforts to make people's incomes more equal. These people would put themselves in box A.

Other people feel that government should be much less concerned about how equal people's incomes are. These people would put themselves in box E.

(a) In the first row of boxes, please tick whichever box comes closest to *your own* views about redistributing income. [Q39] Crime (Law and Order):

Some people feel that protecting civil rights is more important than cutting crime. These people would put themselves in box A.

Other people feel that cutting crime is more important than protecting civil rights. These people would put themselves in box E.

(a) In the first row of boxes, please tick whichever box comes closest to *your own* views about law and order.
[Q40] Welfare:

Some people feel that the poor in Britain are entitled to more help from government. These people would put themselves in box A.

Other people feel that the poor in Britain should get less help from government and do more to help themselves. These people would put themselves in box E.

(a) In the first row of boxes, please tick whichever box comes closest to your own views about government help for the poor.

[Q50a3] Class (Blue-Collar): What (is) your job? (Items 7 through 15 were coded as blue collar.)

1. Large employers 10. Semiskilled manual Small employers 11. Unskilled manual 3. Professional: self-employed 12. Own account 4. Professional: employee 13. Farmers: employers, etc. 5. Ancillary 14. Farmers: own account 6. Junior nonmanual 15. Agricultural workers 7. Personal service Armed forces 8. Manual foreman 17. Inadequate information 9. Skilled manual

[Q50] Union: Are you *now* a member of a trade union or staff association?

[Q50f] Public Sector Employee: Which of the types of organization on this card (do) you work for (items 2 through 5 were coded as public):

Private firm or company

2. Nationalized industry/public corporation

3. Local authority/local education authority

4. Health authority/hospital

5. Central government/civil service

Charity or trust

7. Other

[Q55] Education: How old were you when you completed your continuous full-time education? [Divided by 10 for MNP estimation.]

[Q60ab] Homeowner. Do you—your household—own or rent this (house/flat/accommodations)?

[Q64] Family Income: And which of the letters on this card represents the *total* income of your household from *all* sources?

1. Less than £3,000	7. £12,000-14,999
2. £3,000-4,999	8. £15,000-17,999
3. £5,000-5,999	9. £18,000-19,999
4. £6,000-7,999	10. £20,000-24,999
5. £8,000-9,999	11. £25,000-29,999
6. £10,00011,999	12. over £30,000

APPENDIX B: SURVEY QUESTIONS FROM THE 1992 BRITISH ELECTION STUDY

Items on respondents' views of inflation [Q26a], unemployment [Q26b], taxes [Q26c], and employment versus inflation (the Phillips Curve question) [Q35], taxation and government services [Q36], nationalization [Q37], redistribution [Q38], education [Q911], and union membership [Q903c] were identical to the versions of those questions asked in 1987, except that the comparative reference for questions 26a–26c is now to the general election of 1987.

Author's Note: For question 39, box A was coded as 1, box K was coded as 11, and boxes B through J were coded as 2 through 10. For questions 40, 41, and 42, a 7-point scale was used. Box A was coded as 1, box G was coded as 7, and boxes B through F were coded as 2 through 6.

[Q39] European Community

Some people feel that Britain should do all it can to unite fully with the European Community. These people would put themselves in box A.

Other people feel that Britain should do all it can to protect its independence from the European Community. These people would put themselves in box K.

Other people have views somewhere in between, along here or along here.

(a) In the first row of boxes, please tick whichever box comes closest to your own views about the European Community.

Now where do you think the Conservative and Labour parties stand:

- (b) First the Conservative Party. In the next row of boxes, please tick whichever box you think comes closest to the views of the Conservative Party.
- (c) Now in the next row, please tick whichever box you think comes closest to the views of the Labour Party.
- (d) And now, please tick whichever box you think comes closest to the views of the *Liberal Democrats*.

 [Q40] Defense

Some people believe that we should spend much less money on defense. Suppose these people are at one end of the scale at point A.

Others feel that defense spending should be greatly increased. Suppose these people are at the other end, at point G.

And of course, some other people have views somewhere in between, at points B, C, D, E, or F.

(a) Where would you place yourself on this scale, or haven't you thought much about this?

(b) Where would you place the Conservative Party on this scale?

(c) Where would you place the Labour Party on this scale?

(d) Where would you place the Liberal Democrats on this scale?

[Q41] Welfare

Some people feel that the government should see to it that every person has a job and a good standard of living. Suppose these people are at one end of the scale at point A.

Others think the government should just let each person get ahead on their own. Supposed these people are at the other end, at point G.

And of course, some other people have views somewhere in between, at points B, C, D, E, or F.

- (a) Where would you place yourself on this scale, or haven't you thought much about this?
- (b) Where would you place the Conservative Party on this scale?
 - (c) Where would you place the Labour Party on this scale?
- (d) Where would you place the Liberal Democrats on this scale?

[Q42] Women's Rights

Recently there has been discussion about women's rights. Some people feel that women should have an equal role with men in running business, industry, and government. Suppose these people are at one end of the scale at point A.

Others feel that a woman's place is in the home. Suppose these people are at the other end, at point G.

And of course, some other people have views somewhere in between, at points B, C, D, E, or F.

- (a) Where would you place yourself on this scale, or haven't you thought much about this?
- (b) Where would you place the Conservative Party on this scale?
 - (c) Where would you place the Labour Party on this scale?
- (d) Where would you place the Liberal Democrats on this scale?

[Q55a] Blue Collar

Do you ever think of yourself as belonging to any particular class? If YES: Which class is that?

- 1. Yes, middle class.
- 2. Yes, working class.

[Q919b] Homeowner: Does your household own or rent this accommodation?

[Q921] Family Income: Which of the letters on this card represents the *total* income of your household from *all* sources?

1. £3,999 or less	9. £20,000-22,999
2. £4,000-5,999	10. £23,000-25,999
3. £6,000-7,999	11. £26,000-28,999
4. £8,000-9,999	12. £29,000-31,999
5. £10,000-11,999	13. £32,000–34,999
6. £12,000–14,999	14. £35,000–37,999
7. £15,000–17,999	15. £38,000 or more
8. £18,000–19,999	

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Political Parties and Foreign Aid

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The influence of partisan politics on public policy is a much debated issue of political science. With respect to foreign policy, often considered as above parties, the question appears even more problematic. This comparison of foreign aid policies in 16 OECD countries develops a structural equation model and uses LISREL analysis to demonstrate that parties do matter, even in international affairs. Social-democratic parties have an effect on a country's level of development assistance. This effect, however, is neither immediate nor direct. First, it appears only in the long run. Second, the relationship between leftist partisan strength and foreign aid works through welfare state institutions and social spending. Our findings indicate how domestic politics shapes foreign conduct. We confirm the empirical relevance of cumulative partisan scores and show how the influence of parties is mediated by other political determinants.

here is no lack of pleas for a better integration of domestic factors into the analysis of foreign policy and international relations. The idea that international politics is shaped by the internal politics of states stems from an old, diversified, and enduring tradition of international relations (Waltz 1954). In recent years, this tradition has been rejuvenated by the growing recognition that the most fundamental transformations of our time cannot be explained by international factors alone (Kratochwil 1993, 63). A broader, more historical perspective on domestic politics is required, and for this we must turn to comparative politics. In this article, we use the literature on political parties and on the welfare state to assess, with a structural equation model, the effect of partisan orientations and domestic institutions on foreign aid.

The growing integration of the world economy has raised questions on the policy options left to national governments and on the relevance of partisan politics (Kurzer 1993, 252; Scharpf 1991, 244–8). Recent studies have reaffirmed the influence of partisan orientations on domestic public policy, but the debate remains open on the magnitude and the nature of this influence (Boix 1998; Clayton and Pontusson 1998; Garrett 1998a, 1998b; Rhodes 1996; Swank 1998). The idea that "parties make a difference" appears even more problematic for international relations. After Ernst B. Haas's (1958) classical work on European integration, few studies of the domestic sources of foreign policy have dealt with political parties in a systematic, com-

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parative manner (Holsti 1991, 192–3; Risse-Kappen 1991; Simmons 1994, 281). Traditionally understood as above parties, beyond the water's edge, foreign policy has been portrayed either as consensual or as idiosyncratic, more associated with leaders and circumstances than with partisan orientations (Nincic 1992, 90–1). A few exceptions concern issues for which policy is easier to measure, such as defense spending (Chan and Mintz 1992), international trade (Verdier 1994), and foreign aid (Noël and Thérien 1995), but even in these cases, much remains to be done.

This article starts from the idea that parties and institutions matter, not only domestically but also with respect to foreign policy. We argue that, at least for development assistance, parties make a difference. Leftist parties, in particular, have an effect on a country's level of foreign aid. This influence, however, works in the long run, through the mediation of welfare institutions and social spending. In other words, partisan politics plays a role, but it does so within the broader and more enduring context of domestic structures. Beyond the immediate issue of foreign aid, our findings contribute to the larger debates on the relevance of parties and on the domestic sources of foreign policy, because we help specify the effects of partisan politics and the channels that lead from domestic conflicts to foreign conduct.

In the first part of the article we survey the literature on political parties and foreign aid, and we find that existing studies are inconclusive. In the second part we propose a new theoretical model, from which we derive two hypotheses regarding the long-run influence of social-democratic and Christian-democratic parties on development assistance. The third and fourth parts present the data used in this analysis and the results we obtained with a LISREL structural equation model. In the last part we draw on comparative politics to explain the different national cases. More specifically, we show how partisan orientations make a difference in foreign aid policy and how their effect is mediated by other institutional variables. In the conclusion we discuss the implications of this study for the broader debate on the integration of domestic and international politics.

STATE OF THE ART

The literature on the domestic foundations of foreign policy has focused on mass and elite opinion, without much reference to domestic institutions and collective political actors (Hudson and Vore, 1995, 219; Risse-Kappen 1991, 484-5). More recent studies have dealt with various nongovernmental actors, such as interest groups, multinational corporations, and the media, but surprisingly little has been done on political parties (Gerner 1995, 21). Case studies exist, particularly on the workings of multiparty coalition governments, but few analyses provide a comparative perspective (for an exception and discussion see Hagan 1993, 26–31; 1995, 137). This neglect is surprising given the centrality of political parties in the government of liberal democracies, their importance in the study of public policy, and their acknowledged role in some key foreign policy changes (Blais, Blake, and Dion 1993, 40; Hagan 1994, 139).

Most of the studies on the parties-aid relationship are qualitative, but a few have used quantitative methods. Qualitative studies suggest political parties have an influence on foreign aid. From his seminal comparative study of "like-minded countries" (Canada, Denmark, the Netherlands, Norway, and Sweden), Olav Stokke (1989, 281) concludes that the efforts made to maintain the question of foreign aid "above party politics" do "not imply... that the political colour of a government does not matter." Parties of the Left, in particular, are expected to be more generous toward Third World countries than parties of the Right. This commonsensical proposition seems corroborated by a number of observations. As Stokke explains, the Nordic countries, where social-democratic parties have been in power for a long time, rank among the leading development assistance contributors. Conversely, countries such as Japan and the United States, where social-democratic parties are absent or have virtually no influence, maintain a foreign aid effort constantly below the Organization for Economic Cooperation and Development (OECD) average based on the percentage of a nation's GNP. In the 1980s, these observations were reinforced by the contrasted evolution of France and the United Kingdom. With the Socialists in power, France increased its assistance, just as the Conservative government of Margaret Thatcher reduced its aid budget (Adda and Smouts 1989, 34-5; Byrd 1991, 60-1). In a major study, David H. Lumsdaine (1993, 163) has reaffirmed the "Left-does-matter" thesis for the entire community of aid donors: "Parties with strong socialist or domestic redistributive concerns . . . strengthened aid when in power, while conservative governments tended to retrench." According to him, governments project their domestic orientations on the international scene. More committed to equality and social spending at home, parties of the Left are expected to be predisposed to spend more on development assistance as well.

Although with much less emphasis, qualitative analyses also suggest that, like religious groups, Christian parties tend to be favorable to development assistance

(Cooper and Verloren van Themaat 1989, 82; Lumsdaine 1993, 158; Pratt 1989, 195). Drawing a parallel between the social principles supported by Christian democrats and social democrats, Stokke (1996, 22) argues that the norms in which Christian democracy is rooted are conducive to a generous stance toward the Third World because "these norms are universal and do not stop at the borders of nation states." In countries such as Sweden, Norway, Denmark, and the Netherlands, religious parties or party factions have indeed mobilized behind foreign aid (Kärre and Svensson 1989, 263; Södersten 1989, 189; Stokke 1989, 282). Other cases, however, indicate the situation is far from clear-cut. In Italy, for instance, Christian democrats have been dominant for the entire postwar period, but the government has maintained one of the lowest foreign aid contributions among OECD countries. Overall, in the qualitative literature, the effect of religious parties on aid policy does not appear as well established as that of leftist parties.

Quantitative studies of the parties-aid relationships have dealt only with the influence of leftist parties on development assistance and, even on this question, they appear much less conclusive than qualitative studies. Louis-Marie Imbeau (1988, 1989) examined 17 countries over the 1963-81 period, using the percentage of votes obtained by each party as an indicator of party strength. He found that "more left-oriented countries give more aid" but acknowledges the fragility of his results (1989, 109). Of the four years considered by Imbeau, only two (1976 and 1981) yielded a significant and positive measure of association; one year (1966) even presented a negative correlation between the power of the Left and the level of foreign aid. With such "ambiguous" results, Imbeau (1989, 157) finally concluded that "what is important seems not to be which ideology is present, but rather how it is translated into policy." For him, the role played by parties must be understood as complementary to other factors, such as inertia, donors' interests, and donors' perceptions of recipients' needs (p. 155).

Marijke Breuning (1995) also suggests there is no simple, straightforward relationship between partisan orientation and development assistance. Her content analysis of parliamentary debates on foreign aid in three countries (Belgium, the Netherlands, and the United Kingdom) shows, for 1975–91, that "the political affiliation of the minister has the potential to affect the debate in Parliament as a whole"; yet, the same analysis indicates that "whatever agenda-setting the ministers do... is not determined by their party affiliation" (1995, 246). Breuning's conclusions reinforce the traditional view of foreign policy as standing beyond partisan differences, since she finds "a consensus about the state's role in the foreign assistance issue area" (p. 252).

Our own recent study of 16 countries for six of the years between 1965 and 1988, supports the skepticism of Imbeau and Breuning. Using the difference between the percentage of seats in the cabinet held by parties of the Left and Right as an indicator of leftist power, we found no correlation between the parties in power and

the level of foreign aid (Noël and Thérien 1995). For three of the six years considered, we even obtained negative correlations. We suggested, however, that further tests should be conducted, given the plausibility of the partisan hypothesis, established by a host of qualitative observations and studies (p. 532).

The contrast between qualitative studies that agree parties matter and quantitative ones that fail to confirm this idea is striking. In these circumstances, a lack of quantitative support should not lead us to dismiss hastily the conclusions of qualitative assessments. The hypothesis of a relationship between partisan orientation and development assistance may have been tested the wrong way. There are many indicators of partisan strength and a variety of models to capture the relationship between parties and policy.

THEORETICAL MODEL

There are at least three problems with the conventional representation of partisan influence. First, because the usual conceptualization and measurement of partisan variables assume governments have immediate effects, they fail to capture the long-run dimension of partisan politics. Second, most of the quantitative studies neglect the importance of Christian democracy, the significance of which is underlined in recent studies of European politics. Third, the standard understanding of partisan influence looks only at direct effects, assuming there is always a simple, unmediated relationship between parties and public policies. Our theoretical model addresses these three issues in order to account better for the parties-aid relationship.

The first problem concerns the time frame of partisan influence. The usual indicators of partisan strength measure, in various ways, the distribution of power in a given year. This short-term perspective may well explain why quantitative studies have failed to confirm the widespread observation that, "in country after country, the politicians and political parties that strongly advocated aid were those on the left" (Lumsdaine 1993, 139). The effect of social democracy on public policy is likely to be a long-run phenomenon, felt only after a party has been in power for many years. In foreign as in domestic policy, patterns tend to be established at critical junctures and change only incrementally thereafter. The dominance of a party over a long period probably matters more than the distribution of power at any moment. This is what Evelyne Huber, Charles Ragin, and John D. Stephens (1993) demonstrate in their work on the welfare state. Using cumulative indicators of partisan power, these authors obtain more conclusive results than previous studies on the effect of parties (pp. 724-5). Accordingly, our model of the partisan determinants of development assistance includes variables that measure the cumulative presence of parties in power and compares them to the more conventional single-year indicators.

The second problem concerns the role of religious parties. As explained above, the specific contribution of Christian-democratic parties to foreign aid policy has been recognized in the qualitative literature but omit-

ted in quantitative studies. Until recently, the situation was similar in the comparative study of the partisan foundations of the welfare state. Understood as historical anomalies or as functional equivalents of socialdemocratic or conservative parties, Christian-democratic parties have been largely neglected, except in case studies (Kalyvas 1996, 1-7; van Kersbergen 1995, 1-2, 23-9). Recent works, however, have established the distinctive and coherent character of Christian democracy as a centrist phenomenon, characterized by its emphasis on social mediation and moderate policies (Kalyvas 1996, 263–4; van Kersbergen 1995, 28–9). Christian-democratic parties have constructed distinct welfare states that institutionalize their conceptions, inspired by religious ideas, of social capitalism and societal accommodation (van Kersbergen 1995, 2-5, 230-1). Qualitative studies suggest similar dynamics have been at play with respect to foreign aid. Two political traditions, insists Stokke (1996, 22-4), have shaped foreign aid policies. The first derives from the social-democratic understanding of solidarity, and the second stems from the Christian-democratic notion of brotherhood. A model that seeks to explain the effect of parties on development assistance should include a variable for Christian democracy, along with the more conventional variable measuring social-democratic power. This new variable should also, of course, be considered in the long run in order to capture the effect of Christian-democratic cumulative power.

Finally, quantitative studies assume a direct, unmediated relationship between partisan orientation and foreign aid. This assumption is associated with the conventional idea that parties have an independent influence on each policy area; they will implement their platform section by section, in a straightforward fashion. This understanding of public policy as a set of distinct items may be reinforced by the use of multiple regression techniques to test the assumed relationships. Regressions may suggest the presence of indirect effects, but they provide an image of policy as determined by a set of independent variables, whose effects are additive more than interactive. "The multiple regression equation," write Schumacker and Lomax (1996, 38), "is by definition additive and does not permit any relational specification of variables. This limits the potential for variables to have direct, indirect, and total effects on each other."

The historical institutionalist approach to comparative politics offers a useful theoretical corrective. Indeed, beyond their claim that institutions matter, historical institutionalists seek to "demonstrate the relationships and interactions among a variety of variables in a way that reflects the complexity of real political situations" (Thelen and Steinmo 1992, 13). Their aim is to develop "more explicit theorizing on the reciprocal influence of institutional constraints and political strategies and, more broadly, on the interaction of ideas, interests, and institutions" (p. 14). In this perspective, the model we propose indicates how the interactions among partisan and other institutional variables may account for foreign aid. More specifically, this model associates partisan power with social-

democratic welfare state institutions and social spending, two variables previously identified as significant in the explanation of foreign aid.

Our model of the parties-aid relationships is novel in three major ways. First, it considers partisan cumulative power, assuming partisan effects are a long-run phenomenon. Second, it includes a variable for Christian democracy to integrate an important insight of the qualitative studies on foreign aid and of the recent literature on the welfare state. Third, it starts from the idea that partisan effects may be indirect and incorporates two potentially important intervening variables, social-democratic welfare state institutions and social spending. Combined with what is already known about parties, the welfare state, and foreign aid, these three propositions yield two hypotheses:

Hypothesis 1. Leftist cumulative power has a positive effect on foreign aid, through the influence of socialist attributes and social spending.

HYPOTHESIS 2. Religious cumulative power has a positive effect on foreign aid, through the influence of social spending.

In the first hypothesis, the logic at work is the following. When they hold power for a long time, parties of the Left tend to institutionalize social-democratic welfare state principles and practices, which are themselves the best predictors of a country's foreign aid effort. The second hypothesis derives from the observation that Christian-democratic cumulative power favors high levels of social spending, and a country's social spending is strongly associated with its foreign aid (Huber, Ragin, and Stephens 1993; Noël and Thérien 1995).

Methodologically, our hypotheses involve a series of interrelated effects that can best be represented and tested with a structural equation model. Theoretically, our approach stems from the general idea that, for political parties, the road to power requires the construction of social bases, a continuous process that involves transforming the state to institutionalize various forms of social solidarity and class alliances (Esping-Andersen 1985, 37-8). In the process, certain ideas and principles prevail and are entrenched for long periods, during which they shape not only the actors' strategies but also their goals and discourses, often in more than one policy area (Thelen and Steinmo 1992, 9). This is why foreign aid is closely linked to the domestic social policies that parties build over time.

THE DATA

Our model includes one dependent variable (Foreign Aid), two independent variables for leftist and religious party strength (measured in two different ways), and two additional independent variables that have proved important in accounting for a country's development assistance commitment (Socialist Attributes and Social

Spending).¹ The analysis is comparative and cross-sectional. It includes the most important aid donors, 16 of the 20 countries belonging to the OECD Development Assistance Committee (DAC), for two different years, 1980 and 1991.²

The dependent variable, foreign aid, is measured with OECD data on official development assistance (ODA) as a percentage of GNP (OECD, Development Co-operation, various years).3 With respect to partisan orientation, we use two types of indicators to measure the strength of leftist and religious parties. Both derive from the comparative literature on the influence of political parties and are based, with some adjustments, on the classification of Castles and Mair (1984). The two types of indicators are very different theoretically and empirically. The first type (Leftist Power and Religious Power) reflects the distribution of partisan power in a given year. Leftist power is borrowed from Blais, Blake, and Dion (1993, 48) and measures "the difference between the percentages of seats in the cabinet held by parties of the left and of the right." We calculated religious power in a similar fashion, as the percentage of cabinet seats held by religious parties, for each of the two years under study.

The second type of indicators (Leftist Cumulative Power and Religious Cumulative Power) was developed by Huber, Ragin, and Stephens (1993) and measures leftist and religious party power over the years. Unlike the first type of indicators, which give discrete values for each year considered, the second type provides scores that are additive from 1946 onward. The measure of party strength is also somewhat different. Whereas leftist and religious power consider the percentage of seats in the cabinet, cumulative leftist and religious power use the fraction of leftist/religious seats in parliament of all governing parties' seats (Huber, Ragin, and Stephens 1993, 725). The key difference, of course, concerns the instantaneous effect expected with the first type of indicators, compared to the stress on cumulated power associated with the second type. We have argued above that cumulative indicators of partisan power appear preferable because they better represent the historical strength of the different parties. These indicators may be unstable in the early years, when a shift in government can modify a country's

¹ We also considered the rate of unemployment and the consumer price index, two independent variables that reflect the economic context within which countries make their development assistance decisions. The relationship between the rate of unemployment and foreign aid, however, is not significant (r = 0.02 in 1980 and r = 0.04 in 1991). The relationship between the consumer price index and foreign aid also proves nonsignificant (r = 0.12 in 1980 and r = 0.01 in 1991).

² In 1991, these 16 countries provided 94.3% of total world aid (OECD 1995).

The choice of this measure can be justified easily. First, it constitutes the standard indicator in the literature on foreign aid. Second, other measures of a country's commitment to development assistance—the multilateral character of the country's contribution, the grant-loan ratio, or the proportion of aid that is untiled—tend to correspond to its overall level of foreign aid (Lumsdaine 1993, 41–2, 268). Third, this indicator is politically meaningful since the mternational community expresses aid targets in these terms (Opeakin 1996, 22).

	1980			1991		
	Socialist Attributes	Social Spending	Foreign Aid	Socialist Attributes	Social Spending	Forelgn Aid
LeftIst power	.26	.13	.08	.14	.48*	.31
Religious power	.17	.58*	.10	.22	.17	.22
LeftIst cumulative power	.76**	.27	.52*	.70**	.43	.66**
Religious cumulative power	31	.56*	06	28	.32	−.19
Socialist attributes		.27	.77**		.52*	.88**
Social spending			.62*			.73**

score markedly, but they become stable for the later years that we consider in this cross-sectional study (1980 and 1991). In any case, the comparison of year-to-year and cumulative indicators of partisan power should help clarify their respective merits.

We had the opportunity to work with data sets provided by Blais, Blake, and Dion and by Huber, Ragin, and Stephens. The former were used as expanded and corrected in the authors' most recent analysis (Blais, Blake, and Dion 1996, 515). The latter, which ended in 1988, were updated to 1991 with the help of data compiled in the European Journal of Political Research (Koole and Mair 1993; Woldendorp, Keman, and Budge 1993).

The two additional independent variables (socialist attributes and social spending) were established as important domestic determinants of foreign aid in a previous study (Noël and Thérien 1995). We adopt the same definitions and sources. Socialist attributes are represented by Gøsta Esping-Andersen's scores (1990), which capture the degree of program universalism in a country's welfare state. Social spending is measured by total social transfers as a percentage of GNP. Transfers are the sum of the OECD National Accounts data (various years) for "social security benefits" and "social assistance grants."

The analysis is cross-sectional and is conducted for two years, 1980 and 1991. These two years are distant enough and yet sufficiently recent to speak of mature welfare states and of a fully established international aid regime (Noël and Thérien 1995, 529). The first year is the one for which Esping-Andersen's scores are compiled. The second is the most recent year for which a complete data set was available. Following other welfare state scholars, (Janoski 1994, 56; Stephens 1996), we assume the socialist attributes scores compiled for 1980 held constant to 1991. Strictly speaking, this is probably not true. Welfare states have changed, and some countries have abandoned earlier commitments to universality (Pierson 1994). Overall, however,

4 We are grateful to the authors, who shared their data with us and offered helpful advice.

it seems fair to speak of a "frozen welfare state landscape" (Esping-Andersen 1996, 24). Institutions change slowly, welfare state institutions, in particular, have proved particularly resilient in the face of important challenges (Pierson 1996).

RESULTS

To explore the various relationships among our different variables, we start with bivariate and multivariate estimations and then develop a structural equation model. First, we must compare the two types of indicators of partisan strength. This is done in Table 1, which presents the relationship between partisan orientation and foreign aid for 1980 and 1991 as well as the other bivariate relationships necessary to build our model

The first indicator in Table 1, leftist power, represents the strength of the Left in the cabinet in a given year. It does not account significantly for foreign aid. We obtained the same result for 1965, 1970, 1975, 1980, and 1988, and concluded that "while parties may matter in the formulation of aid policies ... left-right partisan orientations do not generate a statistically significant explanation of foreign aid behavior" (Noël and Thérien 1995, 532). The second indicator of leftist party strength, however, yields very different results. Leftist cumulative power, the indicator borrowed from Huber, Ragin, and Stephens, is strongly and positively associated with foreign aid for both 1980 and 1991. This result reconciles the quantitative evidence with qualitative accounts that stress the role of political parties in development assistance policies. It also indicates how partisan influence operates.

Parties do not have the year-to-year influence on foreign aid suggested by an indicator such as leftist power; their influence is felt over the years, as power is cumulated. This mechanism operates in other issue areas as well. Leftist cumulative power, for instance, "shows significant correlations with measures of income distribution and redistribution" (Huber, Ragin, and Stephens 1993, 740). This indicator embodies an understanding of power and public policy that is more structural than pluralist. What matters, in this perspective, is not simply the formation of a government but the exercise and maintenance of power. A party's influence is directly related to its capacity to govern for long periods. This understanding of partisan influence

⁵ Esping-Andersen gives a score of 0 when 60% or less of the population between age 16 and 65 is covered by a sickness, unemployment, or pension program, a score of 2 when coverage is between 61% and 85%, and a score of 4 when more than 85% of the working-age population is covered. Programs are scored individually, and the measure of socialist attributes is the sum of these scores for the three programs (Esping-Andersen 1990, 73–8).

TABLE 2. Regression Testing Hypothesized Predictors of Foreign Aid for 1980 and 1991

	198	Ю	199	1
Independent Variable	b	Beta	b	Beta
Socialist attributes	.07* (.02)	.68	.06** (.02)	.56
Social spending	`.03 ^{**} (.01)	.62	`.03 ** (.01)	.50
Leftlet cumulative	` ,		` ,	
power	005 (.005)	21	.0004	.02
Religious cumulative	` ,		, ,	
power	006 (.005)	-24	004 (.003)	19
Constant	·18	5	` –. 2 1	
Adjusted R ²	.75	5	.86	
F-value	12.24	4**	24.91	**

seems fitting for a study of development assistance, since the level of a country's foreign aid does not change frequently or abruptly.

Table 1 also shows there is no correlation between the two indicators of religious partisan strength and foreign aid. Bivariate analysis, however, offers only a partial view of the relationships involved. Religious cumulative power, for instance, may not be correlated with foreign aid, but in 1980 it was a significant determinant of social spending, which is itself associated with foreign aid. Even the leftist cumulative power-aid relationship may not be as straightforward as suggested in Table 1. In a multiple regression including other determinants of foreign aid identified in the theoretical model, that relationship becomes nonsignificant. Table 2 presents the results of this regression for 1980 and 1991.

By themselves, socialist attributes and social spending provide a good explanation of foreign aid (adjusted R^2 of .74 in 1980 and .86 in 1991). As is clear from Table 2, the addition of leftist and religious cumulative power does not significantly improve these results (adjusted R^2 of .75 in 1980 and .86 in 1991).6 Our aim, however, is not simply to account for foreign aid in the most parsimonious way; we want to elucidate the role played by political parties, directly or indirectly.

Consider the effect of leftist parties. We know from Table 1 that their cumulative power is associated with foreign aid. When that power is combined with socialist attributes and social spending in a multiple regression, however, it becomes nonsignificant. This is consistent with our hypothesis about the role of social-democratic parties and institutions: The relationship between leftist cumulative power and foreign aid seems to be a

typical case of developmental sequence, with socialist attributes and social spending as intervening variables (Asher 1976, 18).

In the case of religious parties, the situation is more complex. As is shown in Table 1, the relationship between their cumulative power and foreign aid is not significant. When social spending is added as an independent variable in a multiple regression containing only these two variables, however, religious cumulative power becomes significant, but the relationship is negative (Beta of -.60 in 1980, p < .05, and -.47 in 1991, p < .01). Hence, Christian-democratic power not only has no direct effect but also, when social spending is held constant, even works against foreign aid. While religious cumulative power does increase social spending, which is associated with increased foreign aid, it appears that any effect of religious parties on foreign aid is unimportant or even negative.

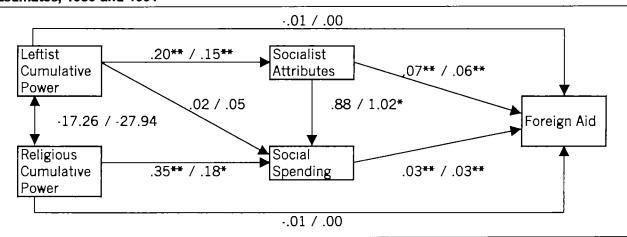
For a better understanding of the indirect and total effects of parties on foreign aid, we will use a structural equation model. A technique derived from multiple regression and path analysis, structural equation modeling makes it possible to integrate into a single framework the two paths that may lead from political parties to foreign aid and to estimate simultaneously all the possible effects. This technique also provides an assessment of the overall explanatory power of the combined variables. Covariance matrices were analyzed, and the parameters of the models were estimated with a maximum likelihood fitting function (LISREL 8.12). Figure 1 presents the complete model, with the results for both years under study.

This structural model is adequate, and it clarifies the role of leftist and religious cumulative power in explaining foreign aid. The most commonly used indices of model fit are satisfying. The probability level of the chi-square statistic (.26 for 1980 and .39 for 1991) suggests a good fit (Jaccard and Wan 1996, 86). The goodness of fit index (GFI), the nonnormed fit index (NNFI), and the incremental fit index (IFI) also indicate that our model is consistent with the data (Marsh, Balla, and Hau 1996; Tanaka 1993). We also tested the similarity of the 1980 and 1991 models, following Bollen's (1989, 355-9) testing hierarchy. We started with the least restrictive hypothesis, that both models have "the same form, without restricting any of the nonfixed parameters to have the same values across groups" (Bollen 1989, 358). The results indicate an excellent fit to the data ($\chi^2 = 1.998$, df = 2, and p =.368). More restrictive hypotheses also show a good fit. Even in the most restrictive case, with all parameter matrices constrained to be equal, the fit to the data is excellent ($\chi^2 = 9.664$, df = 16, and p = .884). A comparison of the most and the least restrictive hypotheses gives results consistent with the assumption that the same logic operates in both years ($\chi^2 = 7.666$, df = 14, and p = .906). With all the parameters stable over time, the 1980 and 1991 models appear equivalent.7

⁶ To make sure one or two cases were not driving the findings, we screened the data to detect univariate and multivariate outliers. Univariate outliers were inspected using standardized scores for each variable, and multivariate outliers were sought with Mahalanobis distance. No outliers were identified; the largest score for univariate outliers was 2.24, and the smallest probability for multivariate outliers was p = 0.103. Partial residual plots also were examined, and no departure from linearity was found.

⁷ Classical models of structural equations with observed variables assume that "endogeneous and exogenous variables are directly

FIGURE 1. Causal Relationships among Hypothesized Predictors and Foreign Aid, with LISREL Estimates, 1980 and 1991



Note: Figure entries are unstandardized coefficients, p < .05, p < .01. Chi-equare and the goodness of fit index (QFI) are indicators of overall fit. They estimate how well a hypothesized model fits the observed model derived from the data (Yadama and Pandey 1995, 51). Chi-equare is a test of the null hypothesis and should be noneignificant. QFI measures "the relative amount of variances and covariances jointly accounted for by a model" (p. 53). incremental fit indices such as the nonnormed fit index (NNFI) and the incremental fit index (IFI) "compare a sense of nested covariance structure models" (pp. 61-2). NNH corrects for sample size and IFI for degrees of freedom.

1 25 with 1 degree of freedom; p=26 (1980) 74 with 1 degree of freedom; p=.39 (1991)

QFI. .97 (1980) and .98 (1991) NNFI .94 (1980) and 1 06 (1991)

1.00 (1980) and 1.00 (1991)

This structural equation model lends support to our first hypothesis, regarding leftist orientation, but not to the second, on religious parties. A country's development assistance effort is shaped by socialist welfare state attributes and government social spending, two variables that are themselves associated with the influence that social-democratic parties have established over the years. Contrary to what is often suggested by casual observations in the literature on foreign aid, however, religious parties do not seem to have an influence on development assistance. Table 3 summarizes the effect of each independent variable on foreign aid by providing its unstandardized total effect.

In both years, the total effect of leftist cumulative power is significant, as are the total effects of socialist attributes and social spending. The effect of religious cumulative power is weaker and nonsignificant. Overall, the pattern is the same for 1980 and 1991, except

observed with no measurement error" (Bollen 1989, 80; see also Kelloway 1998, 82). Since our variables are likely to contain random and systematic errors, however, we reestimated the models assuming measurement error. There is no information in the literature about the reliability of the measures we used, but it seems reasonable to assume that socialist attributes have the lowest rehability, followed by leftist and religious cumulative power, then by social spending and foreign aid Four variants of the model were estimated, assuming a range of reliability from 0.5 to 0.7 for socialist attributes, from 0.6 to 0.7 for partisan variables, and from 0.7 to 0.9 for social spending and foreign aid. In each variant, for both years, it is impossible to evaluate the effect of measurement errors on the model because of negative residual variances and an R2 larger than one. Furthermore, the number of iterations necessary for the models to converge is too high for the solutions to be valid. No conclusion can be drawn about the potential effect of measurement errors on the models.

that the total effect of leftist cumulative power on foreign aid is higher in 1991.

Our model helps understand the role played by partisan orientation in shaping foreign aid policies. The level of a country's development assistance is significantly linked, through socialist welfare state attributes and government social spending, to the historical power of the Left but not to that of Christian-democratic parties. The effect of social-democratic parties is neither immediate nor prevailing nor direct. First, their effect appears only over time, when we use indicators of cumulative partisan power. Second, their direct effect is not as strong as that of welfare state attributes and social spending; in multiple regressions including these two variables, leftist cumulative power is not significant. Third, the paths that lead from partisan strength to foreign aid work through a country's welfare institutions and practices. These more distant but very real relationships explain the stress placed on parties by qualitative studies and the absence of confirmation in quantitative analyses.

TABLE 3. Total Effects of Hypothesized Predictors on Foreign Ald, 1980 and 1991

	1980	1991
Leftist cumulative power	.01*	.02**
Religious cumulative power	.01	.00
Socialist attributes	.09**	.09**
Social spending	.03**	.03**
At a . T-L1		OF: #1- <

Note: Table entries are unstandardized coefficients. *p < .05; **p <.01.

	Leftist Cumulative Power (1991)	Religious Cumulative Power (1991)	Socialist Attributes (1980)	Social Spending (1991)	Foreign Ald (Average for 1971–91)
Sweden	38.61	0.04	8	17.08	.80
Norway	33.88	2.04	8	14.87	.88
Austrla	28.63	16.96	2	15.27	.23
Denmark	25.39	0.35	8	17.05	.71
Finland	19.27	0.03	6	7.06	.33
United Kingdom	16.16	0	4	10.40	.37
Australla	15.77	0	4	8.28	.48
Belglum	14.13	24.48	4	22.07	.51
Germany	12.31	25.43	4	14.73	.40
Switzerland	11.65	13.33	4	12.17	.24
France	11.34	3.97	2	17.20	.45
Netherlands	9.70	29.04	6	24.88	.86
Italy	4.91	37.07	0	14.08	.22
Canada	0	0	4	10.16	.46
Japan	0	0	2	10.07	.27
United States	0	0	0	9.85	.23

FROM THE MODEL TO THE CASES

To examine more specifically the relationship between leftist cumulative power and foreign aid, it is useful to consider three groups of countries, categorized according to the strength of their social-democratic tradition. Table 4 presents these three categories, with each country's leftist and religious cumulative power and social spending for 1991, socialist attributes for 1980, and an average of foreign aid spending levels for 1971–91.

From 1946 to 1991, Austria, Denmark, Norway, and Sweden stand out as the only countries in which the Left has been in power for the equivalent of more than half the time. For the last three, the pattern is clear. Social democrats prevailed during most of the period; all three established typical socialist welfare institutions, maintained high levels of social spending, and have a strong record as among the most committed aid donors of the OECD. These countries, plus the Netherlands, are the only ones to reach the 0.7% of GNP target set by the international community.

Austria seems to be an odd case. Despite a strong social-democratic party in power for many years, it has a foreign aid record comparable to that of countries in which the Left is weak or absent, such as Italy, Japan, and the United States. The explanation lies in welfare state institutions. Austria's welfare state, established by conservative governments and left essentially untransformed by the Austrian Socialist Party (SPÖ), is typically conservative, with almost none of the universalist programs that characterize social-democratic institutions (Esping-Andersen 1990, 53, 60; Esping-Andersen and Korpi 1984, 194). Alone in power from 1970 to 1983 and then involved in coalition governments for most of the remaining years of the period, the SPO has pursued a moderate, catch-all strategy, more akin to that of centrists than to that of hegemonic social democrats (Gerlich 1987, 85–6; Kitschelt 1994, 178). In power for many years, the Austrian socialists governed in a conservative fashion; they did not establish social-democratic institutions and did not commit their country to a generous foreign aid policy.

In Australia, Belgium, Finland, France, Germany, the Netherlands, Switzerland, and the United Kingdom, the Left did not prevail over the 1946-91 period, but it played a role—whether in power alone or in coalition—for the equivalent of ten years or more (9.7) in the Netherlands). Most of the countries in this middle cluster have an average score for socialist welfare state attributes, which means that they have institutionalized universality in some of their social programs, such as health care. Accordingly, they tend to have average development assistance contributions. With a strong but not predominant Left, most of these countries integrated social-democratic elements into their welfare institutions and made substantial efforts to increase their aid budget. In this group, Switzerland and the Netherlands stand out for their atypical aid behavior.

Switzerland appears less generous than expected, given the cumulative power of its leftist parties and its average score for socialist welfare attributes. This result can be explained by the nature of the Swiss party system. The average cumulative score of the Swiss socialists results from their regular representation in coalition governments; this representation, however, is always contained within alliances that limit ideological conflicts (Kerr 1987, 119–21). In this context, explains Kerr (p. 140), "the Swiss parliament appears more to register decisions taken elsewhere—be it at the polls or in antechambers of the administration—than to shape coherent policy in response to recognizable party programmes." The socialist presence in government has not challenged the dominant conservative ideology of

the country (p. 182). Despite its average socialist attributes, Switzerland is a welfare laggard. One of the least redistributive countries of the OECD, it contributes relatively little to international aid (Castles and Mitchell 1992, 22; Katzenstein 1984, 111).

The Netherlands, in contrast, is more generous than expected. With the lowest leftist cumulative power score in the middle cluster, this country has a foreign aid contribution comparable to that of the Scandinavian countries, well above the 0.7% international objective. Again, explanations can be found in the party system. In the 1950s, social policy cemented the alliance between the Catholic Peoples' Party (KVP) and the social democrats (Partij van de Arbeid). Taking advantage of favorable economic conditions, these parties laid the foundations of a generous welfare state. These foundations were not challenged with the demise of the "Roman-red" coalition (van Kersbergen 1995, 132-3). On the contrary, in the 1960s, faced with electoral decline and confronted by radicalized Catholic trade unions, the KVP allied with the Protestant parties (Antirevolutionary Party and Christian Historical Union), and together they pursued social-democratic policies to preserve their power against the Left in a competitive and fragmented political space. To "bolster electoral support," the confessional parties "became the advocates of the adoption of universal programs that provide more generous benefits than anything the Labor party strove to realize" (Cox 1993, 169). Against the opposition of a Left concerned by costs, the religious parties, "whose ideology espouses a parochial version of the limited state," became "responsible for one of the biggest welfare expansions in the world" (pp. 218-9). In a country in which the Left was not very strong, socialist welfare state institutions were promoted by religious parties and, along with them, a political situation conducive to a remarkable expansion of foreign aid.

In the last group are countries in which the Left is weak or absent: Canada, Italy, Japan, and the United States. The results for the last three are straightforward. With a weak Left and almost no socialist welfare state attributes, these countries are predictably among the less generous aid donors. The case of Canada appears more puzzling, since it has average foreign aid levels, which themselves correspond to average socialist welfare state attributes. The situation is somewhat akin to that of the Netherlands, with a centrist party pushed by the Left to adopt reforms. From 1945 to 1975, when the contemporary Canadian welfare state was built, the New Democratic Party (NDP) was never in power at the federal level, but it remained a significant competitor for the dominant Liberal Party. The NDP was also in power at the provincial level, where some of the major social reforms originated. In this context, the Liberal Party institutionalized universal social programs and created a welfare state favorable to the development of foreign aid (Thérien and Noël 1994, 548).

With religious parties in these various countries, the situation appears quite different. The Christian democrats' commitment to redistribute income abroad remains well below that of the social democrats; when we control for social spending, religious parties even work against foreign aid. As van Kersbergen (1995, 204, 231) explains, Christian democrats built a distinct type of welfare state regime, which emphasizes societal accommodation and embodies a specific understanding of social justice. This welfare state, however, can vary significantly across countries, according to the competitive environment faced by Christian democrats. In the Netherlands, in a highly competitive situation, confessional political forces contributed to the development of a universalist welfare state and raised social spending; accordingly, they adopted a generous stance toward the Third World.

In Italy, in contrast, Christian-democratic dominance did not foster universalist social policies or foreign aid. This was not for lack of power: "No single party in any Western democracy has been able to become so politically dominant in the first decades after the Second World War as the Italian Democrazia Cristiana" (van Kersbergen 1995, 59). This political force did not permit many social policy innovations because reform-minded Christian and social democrats remained isolated within their parties and were unable to count on the strong communist movement to support gradual change. The result was a minimalist, fragmented welfare state, which was "more an effect of the incapacity to act and to reform than the result of intentional intervention" (van Kersbergen 1995, 162-7). Not surprisingly, the impulse behind development assistance also remained weak (Alessandrini 1984).

The paths that lead from leftist cumulative power to socialist welfare state attributes and social spending, and then to foreign aid, account for much of the cross-country variance in development assistance levels. Few exceptions can be singled out, and these can be explained by the peculiar logic of party competition in the different countries. In fact, these exceptions can help understand the interest and the limits of the variables considered and of the model we propose. While the cumulative partisan scores are useful, they are not faultless indicators of partisan power. First, adding up years of presence in coalition governments may exaggerate the real influence of a party that never governs alone and rarely prevails. The Swiss score, for example, overstates the strength of the Left in a predominantly conservative country. Second, the sum of years in power may also fail to represent the extent to which a party is or is not hegemonic. The difference between the Swedish Social Democrats and British Labour, for instance, is anchored in each party's ability to forge alliances with other political forces and to define the public policy debate. In Sweden, social democracy became hegemonic; in Britain, liberalism prevailed (Kitschelt 1994, 265; Pontusson 1988). Third, it is important to stress that there is never a perfect correspondence between partisan labels and political behavior (Kitschelt 1994, 108-9). The cases of the Netherlands and Canada remind us that different roads led to contemporary social and foreign aid policies and that, as strategic actors, political parties always retain

the capacity to go beyond the boundaries set by their own ideology.

CONCLUSION

The findings presented in this article help clarify how foreign aid is shaped by domestic politics. Our model confirms the primacy of welfare state socialist attributes and government social spending in the explanation of development assistance policies. At the same time, it shows, contrary to previous quantitative studies of the topic, that political parties do matter, not directly and in the short run, but indirectly and over the years. Our findings lend support to the intuition of qualitative studies that have long emphasized the role of partisan cleavages in the formulation of foreign aid policies. For scholars interested in parties, the model also can be useful, since it offers a new assessment of partisan effects. It confirms the empirical relevance of cumulative scores of partisanship and shows how social-democratic power operates through other political determinants.

Most international relations scholars agree that the question is no longer to determine whether domestic explanations should be integrated with international ones, but to establish exactly how this can be done (Moravscik 1993, 8). In our opinion, it is imperative to look closely at domestic conflicts and to do so with the help of the comparative politics literature. This study suggests that political parties, welfare institutions, and social spending play a significant role in the aid policy process, probably because parties and the welfare state

"render some ideas more politically influential than others" (Yee 1996, 93). As is explained in the theoretical model section, when parties hold power for a long period, they put forward specific conceptions of social justice that become established as central to a country's political debates. Foreign aid, in particular, appears strongly influenced by "the capacity of a society to accept and institutionalize nonmarket principles of income redistribution" (Noël and Thérien 1995, 549).

The exact processes through which this influence becomes effective and evolves can only be assessed with case studies. Our findings are consistent with a host of such studies, as well as with Lumsdaine's (1993, 179) observation: "The parties that supported aid were those concerned with issues of equality and alleviation of poverty, not those concerned with the free market or with military and strategic issues." The influence of these concerns is felt mainly in the long run, as parties establish their primacy in society and as their ideas become institutionalized. The partisan history of Scandinavia and Britain, for instance, has shaped these countries' public debates: "Liberalism and social democracy have been hegemonic ideological forces in the arena of political reasoning for close to a century. They have structured the political dialogue around questions of liberty and equality, individual property rights and the central government's power to intervene in the marketplace, social class and citizenship" (Kitschelt 1994, 265). We agree that such public debates matter, and we argue that they matter not only for domestic politics but also for foreign policy.

APPENDIX: DATA VALUES

TABLE A-1. Leftist and Religious Cumulative Power, Socialist Attributes, Social Spending, and Foreign Ald (1980 and 1991) Leftist Religious Cumulative Cumulative Socialist Power Power Attributes Social Spending Foreign Ald (1980)(1991)(1980)(1991)(1980)(1980)(1991)(1980)(1991)Sweden 29.61 38.61 0 0.04 8 17.08 21.06 .78 .90 Norway 28.50 33.88 1.22 2.04 8 14.87 21.05 .87 1.13 Austria 20.45 28.63 14.57 16.96 2 15.27 15.90 .23 .34 Denmark 23.72 25.39 n 0.35 8 17.05 19.89 .74 .96 Finland 14.20 19.27 0 0.03 6 7.06 19.67 .22 .78 United Kingdom 16.16 16.16 0 0 4 10.40 .35 11.67 .32 Australla 6.94 15.77 4 0 0 8.28 11.36 .48 .38 Belglum 11.98 14.13 18.82 24.48 4 22.07 21.90 .50 .41 4 Germany 10.89 12.31 17.57 25.43 14.73 13.06 .44 .40 Switzerland 8.62 11.65 4 10.26 13.33 .24 .36 12.17 13.87 France 2 3.09 11.34 3.97 3.97 17.20 20.08 .38 .54 Netherlands 8.36 9.70 22.54 29.04 6 24.88 24.73 .97 .88 Italy 2.60 4.91 29.92 37.07 0 14.08 18.54 .17 .30 0 Canada 0 0 0 4 10.16 15.23 .43 .45 Japan 0 0 0 2 Ω .32 10.07 10.91 .32

United States

0

0

0

0

0

9.85

11.04

.27

.20

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BOOK REVIEWS

Political Theory

Action and Contemplation: Studies in the Moral and Political Thought of Aristotle. Edited by Robert C. Bartlett and Susan D. Collins. Albany: State University of New York Press, 1999. 333p. \$24.95 paper.

George Klosko, University of Virginia

Inspired by the recent resurgence of interest in Aristotle among moral and political philosophers, this collection claims to address "the possibility of grounding moral and political action in some version of Aristotelian rationalism" (p. xi). The majority of authors "are united by their conviction that Aristotle, and hence reason, deserves a second hearing" (p. xiii). There are fourteen contributions, six of which were published in the 1980s and 1990s. They fall into two parts. In the first, five well-established, generally continental, scholars address the relevance of Aristotle; the second part is primarily exegetical, comprised of interpretations of aspects of Aristotle's moral and political thought.

The first set provides an overview of Aristotle's renewed relevance. Franco Volpi surveys Aristotle's influence, discussing an entire list of scholars, for the most part German. These range from Hans-Georg Gadamer, and his interest in practical reason, to Konrad Lorenz, who is interested in the biological basis of moral conduct. Volpi ultimately questions whether Aristotelian practical wisdom, removed from its metaphysical backdrop, can properly guide moral action. Hans Brunkhorst examines Hannah Arendt's claims concerning the relevance of classical republican models, criticizing her for insufficient attention to the West's Judeo-Christian heritage. In "Do We Need a Philosophical Ethics?" Ronald Beiner argues for the continuing relevance of Aristotelian contextual, practical reason, as articulated by Gadamer, against Jurgen Habermas's attempt to establish moral judgments on firm philosophical foundations. According to Beiner, the debate between these two thinkers "ultimately comes down to a question of the relative priority of theory and prudence" (p. 47). In his own brief contribution, Gadamer compares Aristotle's view on the logic of moral imperatives with the more transcendental views of Kant and Plato.

In the most ambitious contribution in this part, Richard Bodeus attempts to work out Aristotle's account of natural law as it relates to the positive law of existing regimes. Although well-known discussions of something resembling natural law are presented in the *Nucomachean Ethics* and *Rhetoric*, Bodeus argues for a view that dissociates Aristotle's "natural law" and nature. For Aristotle, he claims, nature does not provide a norm of right; rather, overriding positive law are provisions "which precisely assure the common interest in a correct regime" (p. 81).

Although of high quality, the contributions in Part 1 are far from establishing the applicability of either Aristotle or "reason" to contemporary issues. They present nothing approaching Alasdair MacIntyre's attempt to work out a defensible account of virtue, based on Aristotle's views, or Arendt's distinctive reformulation of the nature of political action. Although these authors recount attempts to work from Aristotelian perspectives, on the whole they commemorate the significance of this approach rather than demonstrate it.

In addition to contributions from the editors, the essays in

Part 2 are by David O'Connor, David Bolotin, Lorraine Pangle, Aristide Tessitore, Judith Swanson, Wayne Ambler, and Bernard Yack. These make for a mixed set. Some are highly interesting. For instance, Yack presents a stimulating account of the importance of conflict for Aristotle's understanding of political community. Scholars who approach Aristotle from a liberal perspective generally focus on Book I of Politics, looking for "a counterimage to liberalism's image of political society" (p. 289). Yack claims that the image in Book I should be interpreted in the light of the conflict-ridden accounts of political life in books III-VI. Doing so gives the lie, for example, to Arendt's celebration of the unique qualities of the Greek polis. (Yack's view is elaborated in The Problems of a Political Animal, 1992.) O'Connor explores Aristotle's rhetorical strategy in Politics. Against Aristotle's remark in Book I of the Nicomachean Ethics that an audience receptive to instruction in moral philosophy must be well brought up and so imbued with good habits, O'Connor marshals impressive evidence for a "morally minimalist" account of the audience, claiming that Aristotle actually addresses ambitious Athenian youth bent on political careers. Wayne Ambler pursues the unpromising task of comparing views of Thrasymachus and Aristotle on the role and responsibilities of political leaders, but he emerges with an interesting account of what Aristotle means by the "common advantage," which distinguishes good and corrupt political regimes.

Several authors try too hard to overturn established interpretations, often combing texts for hidden meanings. For instance, Aristide Tessitore presents a revisionist account of Aristotle's portrayal of Socrates. Although Aristotle dismisses the views of Socrates on akrasia (moral weakness) outright in Book VII of the Nicomachean Ethics and is similarly critical in other contexts, Tessitore holds that Aristotle is more sympathetic than is generally recognized, as a part of the latter's "larger effort to secure at least partial acceptance for the place and importance of philosophy in the city" (p. 220). There is much to criticize here, including Tessitore's focus on the Nicomachean Ethics and Politics, which ignores important evidence in the Eudemian Ethics and (possibly pseudo-Aristotelian) Magna Moralia, and no attempt to construct the overall intellectualist moral view that Aristotle ascribes to Socrates and of which he is highly critical. In her contribution, Judith Swanson thoughtfully examines the evidence concerning Aristotle's view of women in biological as well as political works, but then she unaccountably downplays the clear view of women's intellectual inferiority put forth in Politics: Aristotle's "dogmatism means to conceal an elusive teaching about the changeability of nature from those in his audience whose political ambitions might lead them to misconstrue and abuse that teaching" (p.

Despite the editors' claims, little in the volume directly addresses "the possibility of grounding moral and political action in some version of Aristotelian rationalism" (to quote the cover copy). Throughout, there is little attempt to apply Aristotle's thought to contemporary problems. Although the contributions are uneven, considered as a whole, the volume has a definite significance. Traditional scholars will doubtless argue against many particular claims, but they will benefit substantially from doing so. Against the weight of settled interpretations, the collection's accumulation of arguments and evidence conveys the richness and elusiveness of Aristotle's moral and political thought.

Community and Tradition: Conservative Perspectives on the American Experience. Edited by George W. Carey and Bruce Frohnen. Lanham, MD: Rowman & Littlefield, 1998. 206p. \$53.00 cloth, \$22.95 paper.

Community and Political Thought Today. Edited by Peter Augustine Lawler and Dale McConkey. Westport, CT: Praeger, 1998. 244p. \$59.95.

Steven Kautz, Emory University

The horizon of these volumes is familiar: "Today, the particular concern of most political thinkers is the erosion of social and political life, the decline of community. Community is threatened by individuals who are excessively self-conscious, who relate to others too exclusively in terms of egoistic calculation. The source of that preoccupation with calculating selfishness is early liberal political thought" (Community and Political Thought Today, p. xi). The authors of the essays collected in these volumes take as their point of departure the widespread lament among political theorists that the progress of the liberal enlightenment has engendered pervasive and intractable modes of political, social, and private discontent.

The "communitarian" indictment of liberal rationalism and liberal individualism is, of course, almost as old as liberalism itself. From the beginning, illiberal political philosophers have worried about the natural consequences of liberty: inequality and vice. But there is something odd about the most recent revival of the idea of community: its progressive or moderate—even, one might say, liberal—character. Radical assaults on liberal politics have on the whole been supplanted, lately, by efforts to tame the excesses of liberalism without rejecting it wholesale. We are all somehow liberals; we are all somehow communitarians. Many agree: Let us by all means have a more robust community (equality, virtue, tradition); but let us not abandon our rights (among the other securities afforded by liberal polities). The prevailing moderation is a far piece from the harsher critiques of liberalism once common on both Left and Right. Today's communitarianism lacks the moral rigor, the love of justice understood as equality, of the old Left; and it also lacks the moral rigor, the love of virtue understood to require unremitting self-sacrifice, of older republicanism. Praise of community today is most often praise of community that has made its peace with liberalism, including its rationalism and its individualism (see, e.g., Community and Tradition, p. 31).

On the other side of the divide, liberals have been moving toward the center, too. Liberal theorists are learning again to speak the language of virtue, of civil society, of equality, and of community. Many liberals have more or less abandoned the austere individualism, the praise of solitude and acquisitiveness, that was once thought to constitute the core of liberal moral psychology—the idea of the rugged individualist, who would take care of himself because he knew that no one else would much care to do the job. Through one sort of lens, in other words, political theory in the last decade is marked by a convergence of liberals and their critics toward a sort of kinder and gentler liberalism.

What distinguishes these admirable volumes is impatience with this sort of moderate communitarianism. As Wilfred M. McClay puts the point in his essay in Community and Political Thought Today: "The communitarianism we have been getting so far suffers from a fatal defect: it is much too closely bound to the very liberalism it would correct" (p. 101). The editors of Community and Tradition describe their volume as a search for a "fuller and deeper understanding of the true nature of community" (p. 14). Community and Tradition is the more openly partisan book: It offers a conservative vision

of community to counter the contemporary liberal communitarianism that, the editors say, "destroy[s] the possibility for real communities" (p. 14). Community and Political Thought Today is less partisan or programmatic, but many of the contributors agree that the gravity of the crisis facing contemporary liberal polities requires a more radical appreciation of the illiberal nature of communities than is characteristic of mainstream communitarian theorists. Both volumes are valuable contributions to the ongoing debates about the inadequacy of liberal individualism. Many of the essays will challenge those of us who are liberals or liberal communitarians; we will be obliged to think better about whether truly robust community is not almost inescapably illiberal, and about whether truly liberal community is not almost inescapably thin.

The eight essays in Community and Tradition present a more or less unified argument about the defects of progressive communitarianism, together with a sketch of a conservative vision of community. The authors generally embrace the insight of Robert Nisbet, an argument advanced in The Quest for Community (1953) and elsewhere, that conservative partisans of community must oppose the idea of a "national community" as a fraud (pp. 20-4). (This is the principal theme of Kenneth L. Grasso's introductory essay.) True community is intimate community: friendship, family, church. neighborhood, and perhaps voluntary associations of a certain kind. Yet, because such communities are notoriously conservative, many contemporary liberal partisans of the idea of community have been skeptical of proposals to invigorate local communities, fearing their natural exclusivity and intolerance. But this is an error: The idea of a national community is either frivolous or dangerous, either the language of the "soft tyranny" of the "nanny state" (pp. 12, 31-6) or merely "mood music" for an essentially bureaucratic liberal state (Community and Political Thought Today, p. 101). The national political community simply cannot satisfy our need for community—too impersonal, too few of the "essential bonds of shared thought, feeling, and experience of which true community is made" (Community and Tradition, p. 1). It can, however, "misdirect modern man's search for community" and otherwise undermine the intimate communities that are properly the object of the human longing for community (p. 23; and see pp. 41-53, 78-83). And so the contributors to the Carey and Frohnen volume call for a renewal of conservative efforts to cultivate "real communities," which "have invariably been illiberal" (p. 39) and which are founded on the functional autonomy of social institutions, decentralization of political power, hierarchy, and tradition (p. 26).

Community and Tradition also includes several useful historical essays on the relation between liberalism and communalism (republicanism and Protestantism) in early American political history. The essay by McClay in particular, "Mr. Emerson's Tombstone," is a small gem: beautifully written, and a masterly exposition of the transformation of the older Protestant idea of "constrained individuality," with its strong moral and communitarian dimensions, into the "boundless" Emersonian idea of the "autonomous self." which gives rise to a radically individualist "moral chaos," the "worship of the sovereign self and its sovereign appetites" (pp. 87-9, 96-7). This essay is a model of scholarly rigor and passionate advocacy. Finally, the volume contains a number of essays on the modern liberal communitarians Robert Bellah, Charles Taylor, and William Galston; Peter Augustine Lawler's thoughtful essay on Galston, especially, will be of interest to theorists of contemporary liberalism.

Community and Political Thought Today is a more diffuse volume, less partisan and programmatic. It lacks the unity of

viewpoint of Community and Tradition, and the contributors represent a wide range of disciplines, including sociology, political science, history, and political philosophy, as well as a remarkably wide range of philosophical orientations. This diversity has the usual advantages and disadvantages. But perhaps it would be fair to say that the fourteen contributors are united by a shared sense that our principal political problem today is the problem of community. Many of them share, as the editors say, "a moral revulsion against aggressive selfishness" as well as a concern that "the liberated individual may be in constant pursuit of happiness but is too rarely actually happy" (p. xi). "The concern of the authors in this book, in one way or another, is the perpetuation of the 'moral ecology' on which human liberty depends to flourish. They know that the chief threat to liberty today is not smothering community but rootless apathy" (p. xiv). And yet, many of the authors are also doubtful that mainstream communitarian thought can supply effective remedies for the "aggressive individualism" of modern liberal polities.

Thus, for example, several contributors to the Lawler and McConkey volume offer critical analyses of Robert Bellah's work and especially Habits of the Heart. Brad Lowell Stone's essay, "Universal Benevolence, Adjective Justice, and the Rousseauean Way," presents a thoughtful analysis of Bellah's "statist communitarianism" and especially his putative failure to respect "the familiar or communal realm of particularistic benevolence and the public realm of universalistic justice" (p. 96). Several essays discuss the communitarianism of Richard Rorty, of Tocqueville and Arendt, of Michael Sandel, and of the "modern Montesquieu," Raymond Aron (p. 233), among others. The essay by Daniel J. Mahoney, "The Experience of Totalitarianism and the Recovery of Nature: Reflections on Philosophy and Community in the Thought of Solzhenitsyn, Havel, and Strauss," has many virtues; above all, Mahoney offers a remarkable appreciation of the career and work of Solzhenitsyn, one that is not only fair-minded and respectful of his "liberal conservat[ism]" but also wary of the "nascent utopianism" of that dimension of Solzhenitsyn's thought which imprudently seeks to overcome too far the liberal 'separation of statecraft and soulcraft" (p. 219).

Both volumes make valuable contributions to the sometimes stale contemporary literature on liberalism and community.

Machiavelle's Romans: Liberty and Greatness in *The Discourses on Livy*. By J. Patrick Coby. Lanham, MD: Lexington Books, 1999. 267p. \$65.00 cloth, \$23.95 paper.

Anthony J. Parel, University of Calgary

The laudable aim of Coby's study is to rescue the *Discourses* from undeserved relative neglect in scholarly circles. The first part of the study (chaps. 2–6) gives a useful account of the *Discourses*, focusing on the Roman republic's internal structure, external policy, and "captains." The second part (chaps. 7–9) gives Coby's own interpretation of the significance of Machiavelli's achievements in the *Discourses*.

A basic issue that engages Coby's attention is the paradox of the two Machiavellis: the one who holds up ancient Rome as model for imitation and the one who wants to benefit future generations by opening up a new path in political philosophy. Most interpreters have focused on Machiavelli's novità rather than on his Romanità. Coby wants to introduce a balance between the two. His conclusion is that the old predominates, that Machiavelli is "more Roman than modern, and that impressions of newness derive from the fact that liberty and greatness, Machiavelli's twin objectives, occasion-

ally appear in the modern guise, as enlightened and unenlightened acquisitiveness respectively" (p. 11). Certainly, there is novelty in his ethical thought as well, but it is "less than is generally assumed" (ibid.).

Coby's argument revolves around three concepts: acquisitiveness, liberty, and greatness. The universal disposition to acquire and conquer is natural to individuals, social groups, and states, whether princely or republican. In the case of the Roman republic, acquisitiveness was manifested in the ways the republic's two component elements, the patricians and plebeians, related to each other and the body politic. Both were acquisitive, which they manifested in different ways.

The liberty of the Roman republic was the outcome of a state of affairs in which the patricians and plebeians were able to satisfy their respective humors without harming the health of the body politic. Liberty lasted as long as the humors behaved in a healthy way; it disappeared when they behaved in an unhealthy way, namely, when one humor sought its interests at the expense of the other and of the body politic as a whole.

Republican greatness was the outcome of a state of affairs in which the Romans were able to use their liberty and power to extend their domination by means of wars of conquest. The republic became great by extinguishing the liberty of others in peninsular Italy and the Mediterranean littoral. Republican greatness was no respector of other peoples' liberty

Seen in this light, Machiavelli's relationship to modernity appears highly problematic. According to Coby, Machiavelli's attitude toward acquisitiveness was ambiguous at best, an ambiguity which some recent interpreters are very eager to exploit. In Coby's wise judgment, however, Machiavelli's "intent is not the modern project of relieving man's estate but the ancients' ambition of achieving world empire" (p. 278). He is "an ancient, not a modern," insofar as he tried to ennoble acquisitiveness by lifting the sights of humans "from security and wealth to liberty and greatness" (p. 281). Coby's conclusion is that Machiavelli is "a Platonic timocrat lacking the philosopher's vision of the Good" (p. 284).

Under Coby's able guidance the reader gets a grand tour of contemporary Machiavelli scholarship and interpretations. The discussion of liberty and greatness (pp. 254–68) is especially stimulating. Coby is honest and courageous with respect to the interpretations he disputes. He puts paid to the civic humanist as well as to the esoteric reading of the Discourses. Even interpretations based on such notions as "the perpetual republic" and "impartial regime" come under heavy fire. All in all, the book makes the Discourses less mysterious, more intelligible, and more exciting than it is thought to be.

Given the many virtues of this book, it is all the more necessary to point out, however reluctantly, a few of its weaknesses, if only to help the reader make the best use of the book. The chief weakness arises from a misunderstanding of Machiavelli theory of humors (umon) and the related notions of "times" (i temps) and "the heavens" (i cieli). The theory of humors is one of Machiavelli's original contributions to political theory. In the Western canon, no one before or after him used this theory with such skill and explanatory finesse. He used it for a variety of purposes: to classify regimes (principato, libertà, licenzia), to assess their capacity for liberty and greatness, to explain why they rise and fall, and to identify the personality types of individuals, princes, and rulers, the understanding of which was so crucial for their fight against fortune. The theory had its roots in the Renaissance science of medicine, natural philosophy, and astrophys-

Unfortunately, Coby treats Rome's acquisitiveness, liberty, and greatness not in the light of Machiavelli's theory of humors but in the light of a modern theory of class and class conflict. Throughout the book, humors are presented in class terms (e.g., pp. 12, 13, 54, 56, 196, 197, 201, 243, 269). This is surprising in a scholar who is so admirably scrupulous in rendering Machiavelli's modi e ordini as modes and orders. The nadir is reached when the republic is assimilated to "a company divided into management and labor"; humoral conflicts to bad labor relations ("it is good if the workers are unsatisfied, quarrelsome, and suspicious; it is good if they hurl insults at their bosses, have work slowdowns, and go on strike"); the tribunate to labor unions; law to the means of institutionalizing labor unrest; liberty to the safeguards against exploitation; and power to the profits of the company (p. 28). How is this reading of the Discourses compatible with the judgment that Machiavelli had no intention of initiating the modern mode of acquisition? Space limitations prevent me from pointing out the equally unsatisfactory uses made of the notions of "times" and "the heavens."

Because Machiavelli's theory of humors is misunderstood, the book's final conclusion (that Machiavelli is a Platonic timocrat) is set within the framework of Plato's theory of thumos. Briefly, the class analysis of humors and the Platonic enframing of Machiavelli's Romanità diminish the value of an otherwise impressive book.

Eric Voegelin and the Foundations of Modern Political Science. By Barry Cooper. Columbia: University of Missouri Press, 1999. 478p. \$44.95.

The Politics of Truth and Other Untimely Essays: The Crisis of Civic Consciousness. By Ellis Sandoz. Columbia: University of Missouri Press, 1999. 250p. \$39.95.

James M. Rhodes, Marquette University

These two books are by students of the scholarship of Eric Voegelin. The first aspires to clarify the premises of his philosophy. The second aims to apply his insights to the analysis of contemporary political problems.

Cooper's book is an intellectual biography. It starts with Voegelin's attacks on the Nazi race idea in 1933 and leads up to his seminal theoretical works, *The New Science of Politics* (1952) and *Anamnesis* (1966). It undertakes a tremendously difficult task. Voegelin was a scholar whose insights encompassed millennia, whole civilizations, and all the major political, religious, philosophic, scientific, literary, and ideological currents within them. His arguments were built on an erudition so superior to that of his readers that only a few could evaluate his theses independently. Yet, they also were based on assumptions that seemed to be neither self-evidently true nor diligently defended.

Those who studied the enormous body of Voegelin's work from the *New Science* onward usually believed they could understand what he said, but no one ever published a full account of why he had said it. Cooper attempts to supply what has been missing. He examined Voegelin's vast correspondence collected at the Hoover Institution, his mountainous abandoned manuscript on the history of political ideas (1940–52), and numerous authors who influenced Voegelin. Cooper also made a heroic effort to survey the great mass of historical and literary materials that Voegelin mastered (e.g., everything in Toynbee; Mongol legal documents from the eleventh to fourteenth centuries). He traces the origins and evolution of Voegelin's postulates from the outset of his career to the works that brought him public acclaim. Cooper's book rewards careful consideration because it succeeds

in eliminating much of the bewilderment occasioned by the postulates of what really was Voegelin's middle period.

Voegelin often stated that his work had the same motivation as Plato's: The disorder of his time stirred up resistance in his soul. German National Socialism, the twentieth century's sorry record of ideological warfare, and the inadequate reactions of Western democracies and intellectuals to totalitarian movements drew him into a search for the grounds of right order. (In this context, Cooper tells the interesting story of Voegelin's narrow escape from the Nazis in Vienna.) Among those whom Voegelin blamed for the West's ineffective opposition to the totalitarians were the positivists, who denied the possibility of a science of the good. Cooper discusses Voegelin's debates with positivists. Arnold Brecht, particularly, challenged Voegelin to demonstrate the "validity of value judgments." Although Voegelin supposed that there could be a science of order, he always was unresponsive to these demands.

It never was generally clear why Voegelin acknowledged no duty to reply. Relying on little-known letters, Cooper shows that he had two reasons. He judged that Brecht was dogmatically closed to rational discourse, so an answer would have been useless. He also argued that Brecht's position was obsolete in view of the present state of science. Late developments in philosophical anthropology and the historicization of the problems of politics had superceded the values issue. Philosophical anthropology had discovered that the contemplative life of the Greek philosophers was the precondition of a theoretical view of man and that the origin of the bios theoretikos itself was a historical problem. That is, the theoretical life depended on the conditions conducive to its existence. Therefore, Brecht could be refuted only when younger scholars turned away from his position in practice and toward philosophic contemplation (pp. 85-7).

Voegelin's assertion that the origin of the bios theoretikos was historically contingent was another premise that he apparently failed to justify. This assumption appeared in another form in the first sentence of the New Science: "The existence of man in political society is historical existence; and a theory of politics, if it penetrates to principles, must at the same time be a theory of history" (p. 1). Although Voegelin's massive historical studies lent some credence to his principle, no one genuinely understood it or knew from whence it came. Cooper proves that Voegelin's studies of Bodin and Vico convinced him that there is an "unfolding of a pattern of meaning in time" (p. 338) (and, later, that "eternal being realizes itself in time," Anamness, 1978 ed., p. 116). Vico especially taught him that "a transindividual Providence . . . directs the generations of human beings along a line of meaning and that it also is present in the soul of the philosopher in such a way that it becomes intelligible through human contemplation" (p. 375). In this regard, Cooper takes pains to refute the charges of Leo Strauss that Voegelin was a historicist in Strauss's sense of the word. In what certainly will be a controversial accusation, Cooper, who normally is extremely sympathetic to Strauss, suggests that Strauss's indictment of Voegelin resorted to sophistical reasoning (p. 128)

In Anamnesis, Voegelin began to emphasize the philosophy of consciousness, arguing that a divine process of reality manifests itself in human consciousness. Again, readers were left to wonder why he thought so. Cooper shows that Voegelin's premises originated in Schelling's aphorisms on reason, which essentially maintained that "all is only of God, or of the All" (p. 389).

Cooper's meticulously researched, well-argued, and well-written book contains much more that cannot be mentioned

here. It usefully could be extended into a meditation on the adequacy of the foundations of Voegelin's science, a philosophic exercise that Cooper had to leave outside his scope in order to publish a volume of manageable size.

Ellis Sandoz's book is an anthology of occasional papers, some of which were published in journals such as *Modern Age*, others of which were presented at conferences in the Czech Republic on the requirements for establishing democracy there after 1988, and some of which appear here for the first time. All the essays are intended as philosophical lessons in civics. All are inspired by a vision of a university and a profession of political science "that would secure the coherence of the American community and instill the principles of free government in rising generations" (p. 122). All are beautifully written.

Although it is difficult to summarize the contents of a wide-ranging collection under a few headings, one could say that the Sandoz essays fall into six topical categories: the blessings of liberty; the grounding of free democratic government in eternal truth; the history of legal, philosophic, and religious discoveries and acknowledgments of truth; British and American law and civil religion as an embodiment of truth; the practical exigencies of establishing free government and transferring relevant aspects of the American model to Eastern Europe; and the contributions of Eric Voegelin to the science of the truths under consideration. If the essays have a common theme, it is that the line of recognition of eternal truth can be traced from the Bible and classical Greek philosophy to the Christian West; from there to the England of the Magna Carta, the common law jurists, Locke, and Sidney, and from there to the American colonists and founding fathers, not excluding Madison and Jefferson. Furthermore, Sandoz argues that the fall of Soviet-bloc communism has not eliminated threats to free government based on truth, because this evil has been replaced by a rampant, nihilistic deconstructionism that originated in Nietzsche.

Sandoz calls his essays "untimely" because he knows that most contemporary scholars reject his vision of the university and the profession of political science. Yet, in the spirit of Washington and in keeping with Jefferson's reasons for founding the University of Virginia, one should welcome his book as a call to the profession to renew the search for a well-grounded American civics. This is not to say that everyone should or will agree with everything that Sandoz says. Countless American political polemics are full of earnestly quoted proof texts purporting to show that our polity was created by Founders who abhorred the very idea of linking public order to eternal truth. Where there is so much smoke there probably is some fire. I myself am not quite so sanguine as Sandoz about the commitments of Locke and Jefferson to Christian principles of order. Nevertheless, it would be useful for scholars to inquire into means of resolving, or at least living harmoniously with, the American tension between eternal truth, on the one hand, and secular enlightenment, on the other.

Ethics of Consumption: The Good Life, Justice, and Global Stewardship. Edited by David A. Crocker and Toby Linden. Lanham, MD: Rowman & Littlefield, 1998. 585p. \$84.50 cloth, \$27.95 paper.

Leslie Paul Thiele, University of Florida

In this century alone, the world's population has quadrupled. Last October, the milestone of six billion human inhabitants

was reached. Mid-range estimates of future growth put the world population at nine billion by 2050. This growth constitutes a victory for modern medicane, agriculture, and economic development. Average life expectancy has risen 20 years since 1950. Yet, population growth has gone hand in hand with the increasing consumption of natural resources. The "carrying capacity" of the earth, some suggest, has already been surpassed. But the growth of human numbers is not the only cause of natural resource depletion and degradation. Overconsumption by the rich is a significant environmental problem. The wealthiest 25% of the world's population currently consumes 85% of the world's resources and produces 90% of its wastes. Indeed, the average U.S. resident consumes 40 or 50 times the resources of a person in a developing country and produces twice as much trash as the average European. Ethics of Consumption helps us understand and grapple with these national and global trends.

This book is the first of its kind, a compendium of contributions that address from both normative and empirical perspectives the causes and consequences of (over)consumption in an ecological age. The 27 chapters illuminate the life and times of *Homo consumens*. The authors explore the nature of consumption; its means of assessment and measurement; the necessity, costs, and benefits of its reduction and/or more equitable distribution; and its relation to a number of kindred topics, such as agricultural yields, religious commitments, economic development, psychological dispositions, and cultural values.

As in any edited volume, not all contributions are of equal merit. The editors might well have reduced by one-fourth the number of chapters. Even some very well-written and thought-provoking submissions (e.g., by Amartya Sen and Martha Nussbaum) might have been dropped owing to their indirect confrontation with the topic at hand. In turn, some issues were ignored that should not have been. A historical account of the growing scholarly, governmental, and popular concern with consumption would have been helpful. Notwith-standing these criticisms, the volume develops many important themes and is worth mining for illuminating facts and perspectives. I will limit my commentary to a small sample.

Following an introduction by the editors, Herman Daly leads off the volume with the argument that economic growth as it is commonly practiced is impossible to sustain in a finite ecosystem such as the earth. Even if it were possible, endless economic growth may not be worth the price. In "rich" nations, Daly argues, increasing production and consumption yield diminishing returns. Citizens of developed countries suffer ill health much more as a result of overconsumption and the stress of heightened production than of traditional forms of underconsumption or want. We have, for instance, become "goods-rich" but "time-poor." An index of sustainable economic welfare codeveloped by Daly indicates that welfare in the United States since the 1980s has become negatively correlated with increases in GNP. He concludes that the costs of growth are too high.

Mark Sagoff then criticizes Daly and other "ecological economists" who argue that unlimited growth is impossible in a closed system. They insist that we are running out of clean air, clean water, fertile land, and basic foodstuffs. A long-term view of economic utility demonstrates the need for environmental conservation. Sagoff charges that the utilitarian values of ecological economists do ill service to the environment in general and environmental ethics in particular. He argues that nature ought to be protected for moral and spiritual reasons alone. In turn, Sagoff scoffs at the notion that economic growth will necessarily be limited by the

exhaustion of natural resources. Technology will come to the rescue.

In response, Daly chides Sagoff for the rhetorical ploy of denying utilitarian concerns any merit in order to valorize a more spiritual path. In ecological affairs, Daly maintains, morality and prudence really do teach the same lesson—as long as one takes a sufficiently long-term and globally expansive view and does not underestimate the (social and ecological) costs of technological solutions. Daly might also have noted that Sagoff romanticizes the past when he suggests that "early environmentalists" such as Henry David Thoreau and John Muir saw only the divinity of nature and refused to dirty their hands and minds with utilitarian calculations. Anyone who has read Walden knows that Thoreau's defense of the simple life starts off with a chapter entitled "Economy," in which the benefits of living close to nature are shown to outweigh the costs. Similarly, Muir often engaged in utilitarian arguments with politicians and the public to supplement his better-known sermons.

James Nash's chapter, "On the Subversive Virtue: Frugality," suggests one means of personally unifying morality and prudence. Nash observes that frugality, once widely esteemed as a virtue, is not much in favor these days. It is notably absent, for instance, from William Bennett's bestseller compendium of traditional morality, The Book of Virtues. Frugality is a subversive virtue in a socioeconomic system that thrives on growth. Nash insists that frugality is not austerity. He notes that the Latin root of the word suggests fruitfulness. Nash defines frugality as a concern for being over having and for welfare over wealth.

Jerome Segal, in "Consumer Expenditures and the Growth of Need-Required Income," argues that welfare requires more wealth in contemporary society. U.S. per-capita consumption expenditures in constant dollars over the last three decades reveal a remarkable trend. The 1994 level is twice that of 1960 and three times that of 1929. This increase in spending, Segal holds, satisfies "fundamental needs" that previously went unmet or have been dramatically increased. Dual-income families, for example, have significantly more consumption expenditures (higher clothing needs, a second car, more prepared meals, and so on) than families with one housekeeping spouse.

Another cost of growth is examined in Robert Goodland's chapter, "The Case against Consumption of Grain-Fed Meat." Goodland cites well-known studies that demonstrate how harmful to health a diet heavy in meat can be. He also notes UN World Food Council calculations indicating that the global food supply would reach an adequate level if 15% of the cereals currently fed to livestock were employed in human diets. Goodland insists that people should be free to eat whatever they want. But if what they choose to eat has a large negative effect on the environment, then they should pay the full costs of their choice. He proposes that the environmental costs of meat production, such as topsoil loss, siltation, deforestation, and sewage disposal from feedlots, be figured into the price we pay for meat. He also proposes an end to the large-scale government subsidization of meat production in the United States. (Beef production consumes more than ten times the water of grain production, and this use of water is heavily subsidized.) Were meat to pay its own way, one could expect decreased consumption and improved health.

In a chapter entitled "Natural Resource Consumption: North and South," Allen Hammond furthers this line of argument by exploring full-cost accounting. He explains that resource use and degradation could be significantly reduced if the prices paid for goods and services adequately reflected their environmental costs. This could be achieved by any number of mechanisms, including green taxes targeted at polluting industries and the elimination of subsidies for natural resource depletion. In the United States, for instance, timber harvesting is publicly subsidized. The same might be said of automobile use, once the costs of road maintenance and construction and smog abatement are taken into account. Environmentalists have long argued that cars should be "taken off welfare." Public transportation would become much more economically viable were this accomplished.

David Luban believes that achieving a no-growth economy might require authoritarian rule. Austerity, he suggests, cannot be democratically sustained. Environmentally "safe growth" is proposed as a politically feasible alternative. Advocates for sustainable development have long supported economic growth that is stimulated by environmentally friendly services and technologies and does not increase the consumption of energy, raw materials, and habitat. Economic growth that serves to redistribute resources more equitably may also be environmentally beneficial. Nonetheless, Luban is right to note that the nature of public commitment to sustainability affects its political feasibility. The vast majority of Americans enthusiastically endorse environmental protection. Such attitudes, however, have not much altered patterns of consumption. "We talk green," Luban writes, "but live brown" (p. 127). This contradiction, he notes, is nicely illustrated by a Saturday-morning cartoon character, Captain Planet, who teams up with children from across the globe to fight pollution and save the earth. Captain Planet episodes, ironically, are sandwiched between multiple layers of advertisements coaxing the young audience to consume toys at a rate that would challenge the skills of any superhero.

In "Consuming Because Others Consume," Judith Lichtenberg argues that our early and ongoing love affair with conspicuous consumption has psychological roots. She explains that people consume goods to the extent they do in large part because others around them are engaged in similar levels of consumption. (That might explain why Americans spend three to four times more hours shopping each year than Western Europeans.) The upshot of Lichtenberg's thesis is that collective reductions in consumption will prove much less painful than isolated individual acts. The psychological benefits of consumption, in other words, are relative rather than absolute. Her argument feeds nicely into empirical studies of the relationship between wealth and felicity.

In "The Road Not Taken: Friendship, Consumerism and Happiness," Robert Lane reports on research demonstrating that happiness does not increase with rises in income above the poverty line. Across time, Lane observes, there may indeed be an inverse relationship. He notes that postwar generations in developed countries are at least three times more likely to suffer major depression than their grandparents' generation. In the United States, the likelihood of depression is ten times as high for current generations. To a significant degree, this increase in mental illness is caused by declines in social support and integration. Lane argues that the expansion of the market causes or exacerbates these declines by elevating instrumentalist and materialist values and exacting overtime labor. The market also erodes community directly, Lane proposes, because at high levels of income, money is increasingly spent on goods consumed individually rather than collectively. If Lane's argument is correct, psychological therapy would constitute one of the "fundamental needs" that increase our consumption expenditures in a growth-happy world.

Justice and the Environment: Conceptions of Environmental Sustainability and Dimensions of Social Justice. By Andrew Dobson. Oxford: Oxford University Press, 1999. 280p. \$55.00 cloth, \$29.95 paper.

Capitalism, Democracy, and Ecology: Departing from Marx. By Timothy W. Luke. Champaign: University of Illinois Press, 1999. 254p. \$49.95 cloth, \$19.95 paper.

Environmentalism for a New Millennium: The Challenge of Coevolution. By Leslie Paul Thiele. New York: Oxford University Press, 1999. 302p. \$35.00.

John S. Dryzek, University of Melbourne

On the face of it, these books have much in common. All three authors believe that social justice has to be addressed in contemporary political thinking about the environment, but they engage issues of environmental justice in very different ways. Leslie Thiele wants to accommodate it in an expanded environmental movement. Timothy Luke wants to purge that movement in favor of environmental justice. And Andrew Dobson cautions that connections between justice and environmentalism are often a lot more complex and tenuous than either Thiele or Luke would allow.

Thiele's intent is to identify and promote a "fourth wave" of environmentalism in the United States, whose emergence is dated to the late 1980s. This wave is more truly ecological than its predecessors, emphasizing the "dynamic interdependence" (p. 31) and coevolution of nature and humanity. Environmentalists should recognize their own place in this interdependent system and so celebrate the contribution of different kinds of thinking and action to the coevolutionary enterprise. This enterprise can embrace biocentric and anthropocentric thinking, spirituality and rationalism, local and national action, human welfare environmentalism and wilderness preservation, reformism and radicalism. This is an environmentalism without enemies, one that welcomes almost all comers in the coevolutionary quest, excluding only the antienvironmentalism of the ill-named wise use movement and its allies in the property rights movement. Even these merit congratulations from Thiele for their ability to mobilize rural communities, which environmentalists have failed to do.

Thiele's account is grounded in a (slightly foreshortened) history populated by three earlier waves of environmentalism. The first is a century old, combining Gifford Pinchot's conservation movement and John Muir's preservationism. The second, which began in the later 1960s, emphasized planetary limits to the growth of economies and populations. The third era began in the early 1980s and featured cooptation, not in any pejorative sense but in terms of the incorporation of environmentalism into the political mainstream. One might quibble with details of this history. Thiele accommodates seeming opposites in the first wave (Pinchot and Muir), third wave (the large national organizations and the grassroots antitoxics movement), and fourth wave (the large corporations in the Business Council for Sustainable Development and the preservationist radicals of Earth First!). And it is curious that he dates the beginning of cooptation to the exact moment when the Reagan administration attempted to expel environmentalism from the corridors of power. Still, this periodization makes some sense, especially as a history with practical intent. And the intent is commendable, although one might wonder about the prospects for popular embrace of coevolution in a country in which many people do not even believe in evolution.

Thiele's coevolutionary fourth wave is fed by three currents: sustainable development, with its focus on the long-term future; environmental justice and its expansion of

environmentalism to encompass the poor, ethnic minorities, and indigenous peoples disproportionately exposed to environmental hazards; and biocentrism, with its concern for the nonhuman world. All three currents enlarge our thinking: respectively, in time, across different categories of people, and across different species. Thiele enlarges our thinking about where environmentalism has gone and might go. Presumably, Luke and Dobson would argue that Thiele is too generous. Both of them supply an analytical razor: Luke to cut away kinds of environmentalism that he believes have got it wrong, and Dobson, more dispassionately, to enable us to take a hard look at alleged connections between social justice and environmental concern.

If contemporary environmentalism is conceived as a Rorschach test, then where Thiele sees a hundred flowers, Luke sees an infestation of weeds. Luke wants to cut them away and so render ecological protest fit to counter informational capitalism. He also claims to believe in coevolution between human and nonhuman life (p. 23) but is far more selective than Thiele when it comes to the kind of humanity that qualifies.

Luke develops a post-Marxist class analysis of contemporary society, according to which the world is now dominated by a "new class," a "professional-technical intelligentsia" (p. 50) that knows no boundaries, least of all ecological boundaries. The new class manipulates flows of cash and information alike. By writing this book Luke reveals his own membership in the new class, and by the content of the book he reveals himself a class traitor.

Some new class members claim to be environmentalists, but close examination reveals their environmentalism to be a way of further securing the power of their class; Luke condemns them as "ecodisciplinarians" serving the interests of "green governmentality." Here he takes his bearings from Foucault rather than Marx. About the only environmentalists who escape Luke's condemnation are the grassroots activists of the environmental justice movement, which, in another parallel with Thiele, Luke terms "fourth wave" environmentalism (p. 7). He wants to recruit these activists for an ecopopulist crusade against the new class. This is environmentalism as class war; departing from Marx, Luke believes that it is populist movements rather than the working class who now hold the key to history. What these movements can hope to accomplish in light of the massive forces arrayed against them remains unclear, especially given that, for any environmentalist opposition, the "real danger lies not in being ignored but rather in being accepted too literally and too soon" (p. 89). Closer attention to the real-world environmental justice movement would reveal that it, too, has often compromised.

In one of the best chapters, Luke deals with the unlikely figure of the Unabomber, treated as a "case study... in what new populist oppositional movements ought not to do" (p. 171), such as murder individual members of the new class. But perhaps the very fact that Luke needs to reach this far suggests there are few other straws for him to grasp in the real world of ecopopulist activism. Still, his book is a powerful critique, fully in the tradition of Marx as the uncompromising, ruthless, and comprehensive critic of capitalism, and it deserves to be widely read, not least by those whom Luke is likely to infuriate.

Thiele believes in an easy accommodation of sustainable development and environmental justice. Dobson believes that this kind of claim, central to the now-dominant discourse of sustainable development, merits close analytical attention. Taking its bearings from the landmark statement of the World Commission on Environment and Development (the

Brundtland Report) in 1987, this discourse assumes that social justice, especially in the form of global alleviation of poverty, is instrumental to sustainability. Dobson recognizes that social justice and sustainability are likely to remain important political goals for the foreseeable future and approves of both. His intent is to identify both synergies and conflicts across these two goals.

Dobson's task is not made any easier by the sheer proliferation of meanings of both sustainability and justice. This proliferation is tackled head-on in applying an "analytical and typological" approach to conceptual clarification; it rejects both a definitional approach that would stipulate a single essence for a concept and a discursive approach that would dwell on a concept's history. The idea, then, is to digest the relevant literature in pursuit of a small number of types with comprehensive coverage.

Sustainability proves quite tractable in these terms. Dobson identifies three conceptions of sustainability: Type A sustains "critical natural capital" for the sake of human welfare; type B sustains "irreversible nature" for the sake of both humans and nonhuman nature; and type C sustains nature for the sake of nature. It should be noted immediately that by narrowing the field to environmental sustainability, Dobson rules out the many conceptions of sustainable development that see some measure of human welfare (such as aggregate income, or even economic growth) as that which is to be sustained.

Dobson finds justice to be far less amenable to this sort of conceptual clarification. He begins by observing that theories of justice can be arrayed along three dimensions: substantive to impartial, consequential to procedural, and particular to universal. The usual suspects are surveyed: early and late Rawls, Gilligan, Nozick, Barry, Ackerman, Walzer, Miller, and so on. But theories of justice prove resistant to distillation into a small number of categories. Dobson then has no choice but to examine how aspects of theories of justice intersect with the three kinds of sustainability in "interesting" (p. 84) ways. The core chapters of the book are therefore organized by the three sustainability types, and aspects of the theories of justice are brought to bear as seems appropriate.

The exposition gets quite complicated, but some clear themes emerge. First, when it comes to nonhuman nature (notably in sustainability C, although also to an extent in B), the language of justice fails altogether. Those such as Luke who believe that environmentalism should be organized around a focus on, and movement for, environmental justice are missing this key point. Environmental justice is in the end about the distribution of environmental goods and risks across humans, while other kinds of environmentalism value goods that cannot be reached by the justice discourse.

Dobson has still worse news for advocates of sustainable development like Thiele, who believe in an easy accommodation between sustainability and justice. Brundtland and her followers argue that the alleviation of poverty will produce environmental benefits, because the poor will be released from the need to destroy critical natural capital to meet present needs. But it is at least as plausible that the rising consumption of the no-longer-poor will increase net levels of environmental stress. The issue is in the end an empirical one, and as Dobson observes correctly, the requisite empirical work has not been done.

But Dobson's news is not all bad for environmental justice. When it comes to sustaining both critical natural capital and irreversible nature (at least for the sake of humans), the language of justice can indeed be brought to bear, especially conceptions of justice that are substantive rather than impartial, consequential rather than procedural, and universal

rather than particular. Thus, libertarians, neutral liberals, feminist advocates of an ethic of care for particular others, and communitarians all fare badly. Dobson also believes that to be environmentally relevant at all, the boundaries of justice must be extended to encompass intergenerational justice, and it is on such intertemporal issues that the language of justice proves essential.

Dobson has done an impressive service to green political theory in working through the intricacies of relationships between sustainability and justice. His main points are important, especially where he explodes generalizations that have been accepted all too easily in the sustainability discourse, and his arguments are generally persuasive. Yet, he misses one crucial connection between environmental justice and all three types of sustainability. In mobilizing the poor against environmental risks, the environmental justice movement makes it harder for the industrial political economy to produce such risks to begin with. The rich have always made sure that toxic wastes are not dumped in their backyard. If the poor can also refuse, there is no place for these wastes to go. Thus, success on the part of the movement would also promote a reconfigured economy that would impose less stress on the world's ecosystems by not producing such wastes in the first place. This change could only be good for Dobson's A and B categories of sustainability, possibly even for his C category.

Luke (pp. 59-60) and Dobson (p. 11) both argue that political theory and environmental thinking rarely connect. But their observations are falsified by the very existence of these three fine and thought-provoking works—and the burgeoning literature on ecopolitical thinking to which they are welcome additions.

John Dewey: America's Philosopher of Democracy. By David Fott. Lanham, MD: Rowman & Littlefield, 1998. 166p. \$58.00 cloth, \$19.95 paper.

The Political Philosophy of John Dewey. By Terry Hoy. Westport, CT: Praeger, 1998. 141p. \$49.95.

Timothy Kaufman-Osborn, Whitman College

The three most important philosophers of the twentieth century, wrote Richard Rorty in Philosophy and the Mirror of Nature (1979), are Ludwig Wittgenstein, Martin Heidegger, and John Dewey. To many the appropriateness of conferring this accolade upon Wittgenstein and Heidegger may seem, at the very least, worth a decent argument. But Dewey? His inclusion in this trio appears vaguely embarrassing, if not outright incredible. He was waning in influence as early as the 1930s and was largely forgotten during the four decades preceding Rorty's reclamation effort, it seems not unreasonable to consign America's "philosopher of democracy" to the dustbin of intellectual history. Since 1979, however, animated in large part by a desire to rescue pragmatism from Rorty's idiosyncratic appropriation, scholars have repeatedly recycled Dewey's writings, in many cases with salutary results. Considered collectively, the contributions of Charles Anderson, Richard Bernstein, John Patrick Diggins, Steven Rockefeller, Alan Ryan, David Steiner, Cornell West, and Richard Westbrook have rendered Rorty's tribute less implausible. even if in the last analysis we remain unpersuaded that Dewey merits the status of resident alien amid the pantheon of this century's European luminaries.

The publication of these two new books on Dewey's political thought, each of which takes Rorty as its foil, indicates that this industry is still chugging along, if only in the sense that it continues to churn out ever more product.

Yet, as is often the case when markets become saturated, their appearance also may signal a decline in this enterprise's overall profit margin. Both works affirm Dewey's stature in the twentieth century, but neither gives us much reason to believe that he will remain prominent in the next.

Terry Hoy offers a concise and lucid exposition of Dewey's call for a reconstructed liberalism; as such, the book has much to recommend it, especially for those who have wondered how, for pedagogical purposes, to reduce Dewey's prolific political musings to digestible dimensions. That call, Hoy explains, was prompted by a recognition of "the contradictions and ambiguities of Enlightenment modernity" (p. 8), most particularly its conjunction of stunning technological accomplishment and remarkable economic productivity, on the one hand, and its relentless generation of misery, whether in the form of impoverishment or the standardization of everyday life, on the other. To account for this paradox, Dewey cited the subordination of classical liberalism's emancipatory import to the ideological imperatives of laissez-faire capitalism. Dewey's response, Hoy argues, was that of a radical Progressive, one who found the New Deal's reformist ambitions too modest and piecemeal but who was unwilling (leaving aside his awkward embrace of the Soviet Union in the 1920s) to endorse the cause of state socialism.

Although noteworthy for its careful presentation, Hoy's exposition will prove more or less familiar to those conversant with the main currents of Dewey scholarship. Its distinctive contribution, therefore, must be located in its contention that Dewey's political theory shows how we might begin to fashion a truly democratic socialism, one that will free liberalism from its capitalist fetters without falling prey to the evils of centralized planning. Because Hoy's imagined interlocutors are primarily philosophical rather than political in orientation, however, and because his chief concern is to show how Dewey is unlike Alasdair MacIntyre but much like John Rawls, the author never quite makes good on this promise.

Moreover, to one perhaps too well versed in the work of Foucault and his ilk, as am I, the cash value of Hoy's general calls for the revitalization of face-to-face communities, for the creation of truly progressive schools, for the extension of scientific inquiry into all realms of social life, and for state support of small-scale private enterprises cannot help but appear suspect. Do not such appeals ring hollow, perhaps even naive, when confronted by the apparently inexorable spread of surveillance technologies, the resurgence of religious fundamentalisms of various stripes, the merger mania that now defines global capitalism, the displacement of embodied communities by their digitalized counterparts, the proliferation of genocidal extermination campaigns, and so on? Is it possible that the paradox Dewey found resident within the Enlightenment has now been resolved, but in a way that mocks all appeals to the power of "organized social intelligence"?

Consistent with his claim that Dewey indicates how we might split the difference between laissez-faire liberalism and state socialism, Hoy claims that pragmatism also shows us how a renascent liberalism might secure the sort of grounding it requires in order to defend itself against its illiberal opponents, but without falling prey to either metaphysical foundationalism or postmodern relativism. Informing this contention is a rejection of Rorty's insistence that liberalism can do perfectly well without any footing more stable than that furnished by the contingent social practices constitutive of American democracy. To demarcate this ground, Hoy explicates and defends what he calls Dewey's "naturalistic humanism" (p. 9), a doctrine whose coherence ultimately

turns on the success of his effort to revive a nonteleological concept of human potentiality. Yet, that success is precisely what David Fott, in John Dewey: America's Philosopher of Democracy, calls into question. Hoy notes in his opening chapter that after World War II Dewey came under fire from Left and Right alike. The latter in particular contended that, because pragmatism could not conclusively escape the charge of "moral relativism and nihilism," it signaled "a spiritual decline of the West" (p. 4). Fott's book is the latest statement of that critique.

Although Fott explores many other concerns (e.g., the place of aesthetics in public life, the relationship between religion and politics, the liberal/communitarian debate), what really drives his inquiry is a single overarching question: "What sort of philosophical justification may be found for liberal democracy as a form of government" (p. 2)? As the godless harbinger of all that is wrong with what Fott indiscriminately labels "postmodernism," "deconstructionism," and "historicism," it is Rorty who must be defeated at all costs. It appears that is easily done. Rorty's entire philosophical edifice collapses, Fott insists, when one recognizes the simple logical fallacy at its core: If Rorty represents his rejection of all transcendent truth as itself a timeless truth, then his project is ultimately self-refuting. Having disposed of Rorty with this single stroke, Fott then turns to the question of whether Dewey should be similarly dispatched.

Fott's response is not so easily summarized, either because it is not altogether clear or because it is exceedingly subtle. Which of these assessments is more plausible turns, I suspect, on what one makes of claims such as the following: "On the question of foundationalism Dewey, as I shall show, seems to belong both to the philosophical tradition and to the sort of opposition to the tradition that is found in Rorty" (p. 63). On the one hand. Fott seeks "to rebut the widely noted interpretation of Dewey as opposed to all philosophical bases for politics" (p. 23). To do so, and much like Hoy, Fott explores Dewey's affirmation of the continuity of organism and environment, as well as the notion of growth that emerges from this affirmation, in order to show how metaphysical presuppositions undergird Dewey's conception of community and, by extension, his representation of democracy. On the other hand, Fott argues that Dewey's relativism, most troublingly apparent in his construction of science as a sort of creative art, invites "the view that he is an intellectual ancestor of postmodern political thought" (p. 116).

The political consequences that attend Dewey's failure to defend a more robust conception of reason include an inability to specify fixed limits to the scope of governmental action, which generates an inability to respond effectively to Tocqueville's concerns about majority tyranny, which in turn generates an inability to guarantee the sort of liberty that is necessary to the cultivation of independent thought and individual character. Just why Fott thinks these consequences of postmodern skepticism should be of concern is not evident, at least to me, since he also insists that most Americans "believe in God" and "affirm something like the inalienable human rights of the Declaration of Independence" (p. 117). If, indeed, the relativism so vigorously rejected by this book is unlikely "to gain widespread acceptance" (p. 117), and if, instead, the real danger to American politics is the outbreak of dogmatic forms of "political enthusiasm" (p. 118), then why does Fott close his book with a call to counter the hostility to revealed religion that has been encouraged by those secular cynics who now control America's public educational system?

However misguided his version of pragmatism, one thing Richard Rorty understands quite well is Dewey's claim that certain key philosophical dilemmas are not so much to be resolved as abandoned. If nothing else, these two books indicate our continuing failure to stop fretting about the twin perils of relativism and foundationalism. Fott condemns Dewey and Rorty alike for their inability to show how liberalism, absent some solid ground, can escape the snares of subjectivism, whereas Hoy lauds Dewey for his success in showing how liberalism can defend itself against rival traditions by rooting its claims in a rationally defensible conception of potentiality. It is my suspicion, contra both, that the specifically political promise of pragmatism will remain unfulfilled so long as its interpreters feel compelled to answer questions Dewey urged us simply to forget.

The Ticking Tenure Clock: An Academic Novel. By Blaire French. Albany: State University of New York Press, 1998. 256p. \$24.50.

Kathy E. Ferguson, University of Hawaii

Put together all the worst stories you have ever heard about academia, and you have the Department of Political Science at fictitious Patrick Henry University in Virginia, the setting for Blaire French's entertaining novel about a young professor's struggle toward self-definition. This department could be the inspiration for the infamous observation that academic politics are so vicious because the stakes are so low. The men with power are arrogant and dangerous. They twist the renewal criteria to ambush hapless untenured faculty and manipulate the exam process to sabotage outstanding graduate students and protect tender faculty egos. The men and women without power are generally anxious and weak, conforming with bitterness but little protest to the arbitrary rules governing their work. Departmental conversations consist of junior faculty (referred to as minnows by the main character) strategizing for favor vis-à-vis the senior faculty (whales) and jockeying for status among their own ranks, while the senior faculty brag about accomplishments, rest on ancient laurels, protect turf, and bully everyone else. The senior faculty pontificate; the junior faculty equivocate; the graduate students try, unsuccessfully, to stay out of the line of

Deeply invested in succeeding in this altogether nasty environment is Lydia Martin, a young American government professor who has all the markings of upward mobility. She published her dissertation as a successful first book on the environmental movement; she is an adequate teacher, she works all the time; she has no personal life. She is up for tenure. Lydia makes it a practice to "roll over" in any conflict with the big shots, promising herself independence after tenure; she often reassures herself that she is "willing to endure six years of submissiveness in return for a lifetime of freedom" (p. 17). She engages in constant calculations about the flow of power—is she above or below the hapless Victor, whose first book got mediocre reviews and whose second flounders in publishing purgatory? Is she sufficiently impressive to provoke awe from those beneath her and sufficiently unthreatening to be accepted by those above?

The most compelling thread of this novel is the chain of events that enables Lydia's transformation from the ranks of the ethically challenged into a likeable human being. Her "roll over" strategy slowly erodes in the face of massive injustices she witnesses toward those low in the departmental hierarchy, including herself. Lydia's emergence from the amoral world of "success at any price" to a more self-reflective and critical posture is a wonderful coming out fantasy. She develops a conscience; she stands up for the

graduate student underdogs; she admits a friendship with a young, naïve colleague; and she continues a line of research that her conservative senior colleagues find politically objectionable. Simultaneously, she writes the needed second book in record time, wins extensive praise within and beyond academic circles, becomes famous, and achieves tenure. In the end, Lydia has it all—except Charles, the handsome environmentalist who recoiled from her cold calculations of self-interest. Yet, it was Charles's revulsion that led Lydia to her epiphany of self-recognition, his rejection of her and the "roll over" rule that taught her its limits.

Despite its charms, there is not enough irony in this story to make the academic archetypes funny instead of shallow or to push through exaggeration to satire. Lydia's colleagues are uniformly one-dimensional, their ranks unrelieved by anyone who is the least bit interesting. Lydia's own epiphany leads her to question the value of utter conformity and selfinterested career calculations, but there is no larger political frame for these developments, and it is quite unclear where they will lead. Blaire French entertains us in this novel with an academic department we can love to hate, but in the end her character triumphs within a system that she has given us every reason to hold in contempt. There is little in this portrait that would let us understand why Lydia works so hard to join such a poisonous cesspool of a profession. She evidently takes no pleasure in her teaching, and in any event the undergraduates are portrayed as uninteresting and obsessed with grades. She likes getting good reviews but has no political commitments informing her research. The novel's subplot takes Lydia into the unfamiliar world of health food stores, radical environmentalists, and New Age spirituality as she studies a local animal rights group, where the people are more interesting, but the politics of engagement do not rub off on Lydia. While she looks forward to making more money after tenure, she has only disdain for the senior faculty whose ranks she seeks to join.

If Lydia's desires are mysterious, those of the graduate students are even more so: They are inexplicably motivated to work hard to join this profession, even when they are sandbagged during their exams, denied financial aid for perceived disloyalties, and subject to theft of their work by senior faculty. Unlike, for example, Gail Griffin's powerful portrayal of the charged complexities of academic life in Calling (1992), Blaire French sketches a flat organizational and interpersonal landscape, one devoid of the competing energies that could tell a more turbulent story.

The Rule of the Rich?: Adam Smith's Argument against Political Power. By Susan E. Gallagher. University Park: Pennsylvania State University Press, 1998. 141p. \$28.50.

Mark E. Yellin, North Carolina State University

The Rule of the Rich? is a provocative, interesting, and clearly written work that both challenges recent revisionist accounts of Adam Smith's political and economic thought and offers its own relatively radical reinterpretation of Smith's project in the Theory of Moral Sentiments and the Wealth of Nations. Gallagher calls attention to some less appreciated issues in Smith, most notably how he dealt with what can be called the problem of authority or determining who rules. This book is a return to standard works on Adam Smith, such as Joseph Cropsey's Polity and Economy (1957), in the way it emphasizes Smith's response to a rising commercial elite that displaced the aristocracy in eighteenth-century Britain. Gallagher's approach, however, is not Straussian, and her conclusions go well beyond those of previous Smith scholars.

What we get here is not simply an Adam Smith who emphasizes limited government and unencumbered markets, but an Adam Smith who rejects politics as such.

Gallagher's work builds on and is primarily a response to the Cambridge approach to the history of political thought most typically identified with J. G. A. Pocock. This approach emphasizes historical and linguistic contextualization as essential to proper interpretation. Methodologically, Gallagher is clearly sympathetic to this approach, but she is also quite critical of the way in which different Cambridge school scholars, such as John Robertson and Donald Winch, have argued for a very political Adam Smith.

In the "revisionist" Cambridge school interpretation, Smith is seen as responding to the classical republican or civic humanist critique of commercial society offered by opposition politicians such as Bolingbroke to the Walpole regime. Gallagher calls into question this revisionist scholarship, which argues that "Smith was an advocate of agrarian rather than industrial capitalism," engaged in "an effort to defend the British aristocracy against classical republican critics of commercial modernity; that he tried to promote civic virtue ...; and that he dealt with the moral defects of commercial society by placing his arguments within a jurisprudential framework" (p. 70). In place of such claims Gallagher offers an account of Smith in which he rejects any political role for the aristocracy and in which he rejects politics as such. Gallagher argues that for Smith the solution to the moral defects of commercial society could be found in allowing the unintended consequences of economic activity to play themselves out without any political interference, allowing "nature" to take its course. Gallagher is in effect revising the revisionists.

The key theme of the book is the place of a corrupt and incompetent aristocracy within a commercially dominated eighteenth-century British politics. Before turning to her discussion of Smith, Gallagher does a superb job of setting the stage with a thoughtful analysis of three key political thinkers who preceded Smith in considering the problem of aristocratic corruption. Indeed, one strength of this book is the way it is organized; a chapter each is spent on analyzing Mandeville, Bolingbroke, and David Hume and their responses to the crisis of authority. Gallagher initially presents a stark opposition between Mandeville and Bolingbroke. The former is presented as a withering critic of aristocratic pretension and hypocrisy, whereas the latter is portrayed as a vitriolic defender of aristocratic interest, using the republican language of virtue to condemn the moral corruption brought on by commerce. Hume occupies a kind of middle position. He recognizes the incompetence of the aristocracy but sees it as necessary for maintaining political order and stability in the new commercial age. By placing her account of Smith against the backdrop of these three thinkers, Gallagher allows us to see why Smith's rejection of a political role for the aristocracy would amount to a fundamental break with previous political thought.

The great flaw in this work is that the subtle, nuanced, and mostly accurate analysis that Gallagher gives us of Mandeville, Bolingbroke, and Hume does not quite extend to Smith. Gallagher deserves credit for examining the issue of authority in Smith, but her account overemphasizes the importance of certain passages, most notably those in which Smith highlights the perverse effects of government action, and she virtually omits any important discussion of the proper and positive role of government found in Book Five of the Wealth of Nations. In doing so, and in portraying Smith as a radically antipolitical thinker, Gallagher moves from a highly plausible

and convincing set of premises to an ultimately implausible

The author makes some very important and insightful points about Smith's view of the role of the aristocracy, but she does not adequately prove that Smith's views lead to the complete rejection of the aristocracy from politics, much less to a rejection of politics as such. One of her objectives is to distinguish Smith's view of the aristocracy from Hume's, but there is little actual evidence that they differed on this point. They had public disagreements on several important matters, such as how to deal with religious sects and the desirability of standing armies, but there is little difference in their thinking on the overall importance of political authority and the functional need for the aristocracy, whatever its defects.

To reject a political role for the aristocracy would be to reject the theory of the balanced constitution, that is, the commonplace view in eighteenth-century Britain that emphasized the need for three orders—commons, aristocracy, and monarchy—to pursue their respective roles and interests and as such thwart any one order from complete domination and tyranny. Smith clearly accepts this notion in the *Theory of Moral Sentuments* (VI.ii.2.9). Even if the aristocracy was seen as useless and even harmful, as Gallagher suggests, it still had a constitutional purpose to serve. This in itself presents a paradoxical problem that Gallagher does not address: What does one do with an order of society that is failing but is still necessary?

This points to an interpretation of Smith that is prosaic but probably more accurate. With the apparent failure of the aristocracy to continue to command respect and deference, a shift had to be made in the direction of a constitutional regime based more on institutional design than on social orders. The turn in emphasis to institutional structure for a commercial society is where both Cropsey in his work and Donald Winch in Adam Smith's Politics (1978) end up, although by very different routes. This suggests a truncated and limited notion of political life compared to the classical republican notions, but it is not an utter rejection of politics.

Gallagher's work could have been strengthened if it had also engaged two significant contemporary works, J.C.D. Clark's English Society 1688–1832 (1985) and Jerry Muller's Adam Smith in His Time and Ours (1993). It is particularly baffling that Gallagher did not take on Clark's work, which has recently been at the center of debates of eighteenthcentury historiography, since it could have served as an excellent foil for her argument. Clark maintains that there was no problem of the aristocracy in eighteenth-century Britain. In his view, aristocratic dominance and a culture of deference fit hand-in-glove with commercial development. Muller's work would have been useful because he sees Smith as someone with a very well-developed view of institutional design and its role in promoting prosperity, decency, and freedom. Muller even goes beyond Winch's work, seeing Smith as someone with not only a concern for politics but also a coherent and comprehensive political philosophy. If Gallagher had been able to show that Muller's claims about Smith's project were untenable, her interpretation truly would have broken new ground.

Despite the objections raised here, Gallagher's book is an important and insightful study that will be of interest to anyone who works on eighteenth-century British political thought or on the role of authority in commercial society. In addition, because of its readability and concise length, it is accessible to those with little background who would like to know more about Mandeville, Bolingbroke, Hume, and Smith.

In Defense of Natural Law. By Robert P. George. Oxford: Oxford University Press, 1999. 343p. \$65.00.

Stanley C. Brubaker, Colgate University

Robert P. George, McCormick Professor of Jurisprudence at Princeton, brings together a series of important essays linked by their common defense of natural law. The book consists of three parts: a direct defense against natural law's several critics; an indirect defense through engagement with alternative moral and political theories (those of Amy Gutmann, Joel Feinberg, Alasdair MacIntyre, Michael Perry, Richard Posner, Andrew Sullivan, and Dennis Thompson); and an illustrative defense, building on the author's Making Men Moral (1993), which shows natural law to be a formidable and attractive alternative to liberalism on such morally charged issues as gay marriage, pornography, abortion, religious freedom, and international law. On every front, George shows himself to be a champion of the first order. His arguments display analytic rigor, penetrating insight, honesty and graciousness toward his adversaries, moral seriousness, and rare moral courage.

The school of natural law that George defends (he modestly denies a major role in its development, p. 1) has been called the New Natural Law (NNL), although the title misleads if it implies novelty in the law's content. Instead, NNL advocates endeavor to articulate what has always been the natural law and to rescue it from what they believe is a legion of misguided exponents, in particular, those who have portrayed natural law in a manner that renders it vulnerable to the charge of "naturalistic fallacy," inferring a moral "ought" from a natural "is." Such fallacy includes the common interpretation of Aquinas's first principle of practical reason ("good is to be done and pursued, and evil is to be avoided") as a moral imperative ("ought") derived from speculative reason's identification of man's good with his natural end, or telos ("is"). In a seminal reinterpretation, the school's founder, Germain Grizez, argued that Aquinas makes no such derivation, only states a "pre-moral" principle of any coherent practical reasoning ("The First Principle of Practical Reason," Natural Law Forum 10 [1965]: 168-96).

Building on this interpretive insight, Grizez and later John Finnis (Natural Law and Natural Right, 1980), developed an understanding of nonhierarchical, incommensurable, and pre-moral "basic goods" (life, health, knowledge, play, aesthetic experience, friendship, practical reasonableness, and religion), whose goodness and ultimacy are not derived from prior speculative knowledge of nature; they are instead "self-evident" to one carefully reflecting on his reasons for action. Understanding these basic goods then allows for the derivation of NNL's "moral norms." Grizez and Finnis, according to George, thus "deny any rational basis of an identification of 'the natural' with 'the morally good'" (p. 60).

Although this independence from nature rescues NNL from the naturalistic fallacy, it also allows critics to pose troubling questions. In what sense is NNL natural? Can it dispense with the idea of nature as normative without rendering its basic goods and moral norms mere subjective preferences, social conventions, or structures of the mind? If one cannot derive an "ought" from an "is," can Grizez and Finnis derive an "ought" (moral norms) from a "good" (the basic goods)? Such are the concerns of the most penetrating portions of In Defense of Natural Law. In response to the last question, George candidly acknowledges that initial statements by Grizez and Finnis left the derivation of the moral norms "obscure" (p. 50), but he argues the problem has been subsequently resolved by their concept of "integral human"

fulfillment," a moral ideal that is not derived from the basic goods yet is somehow rendered visible in their light (p. 54). From this ideal, they derive the first principle of morality ("will those and only those possibilities whose willing is compatible with integral human fulfillment," p. 51), and hence the rest of the moral norms embraced by NNL.

If the ideal of "integral human fulfillment" permits a more strictly logical derivation of "ought" from "ought," what does it imply about the nature of nature in NNL? Despite misimpressions to the contrary in critiques of NNL by Russell Hittinger, Ralph McInerny, Henry Veatch, and Lloyd Weinreb, George holds that NNL advocates have never "denied that the basic human goods and moral norms have a groundin human nature" (p. 85, emphasis added). NNL simply holds that knowledge of the basic goods and moral norms is not derived from prior speculative knowledge of man's nature. In fact, George argues, as a matter of ontology as opposed to epistemology, human goods and moral norms presuppose the existence of human nature, whose perfection they envision. If human nature were other than it is, basic goods and moral norms would have to be other than they are (pp. 86-7).

If, however, moral norms are indeed grounded in nature—even though this nature may only be known as a presupposition of practical reasoning rather than more directly through speculative reasoning—then one is left wondering why NNL advocates, George included, deny that nature is normative (pp. 39, 60, 61, and 78). One must wonder as well if speculative knowledge of nature, especially the status of teleological understandings, would not at least affect the confidence with which one might affirm the propositions NNL holds self-evident.

Both the force of NNL and its ambivalence on nature are evident in George's treatment of human sexuality, a major concern of this work. George joins other NNL advocates in distancing himself from the teleological understanding of nature that led many to condemn "unnatural acts" as "contrary to the direction inscribed in the reproductive or procreative power" (p. 161). In place of such arguments from nature, he posits the intrinsic value of marriage, whose goodness is known self-evidently, defined "as a two-in-oneflesh communion" (p. 139) and as "the community formed by a man and a woman who publicly consent to share their whole lives, in a type of relationship oriented toward the begetting, nurturing, and education of children together" (p. 168). All sexual relations that are not actualizations of this two-in-one-flesh communion—sodomy (whether homosexual or heterosexual), fornication, masturbation, prostitutionnecessarily treat the body as an extrinsic instrument of the desiring "self" (pp. 6, 147-51, 167-81) and are thus "intrinsically morally bad" (p. 148).

The argument is carefully crafted, yet it replaces degrees and nuances of moral imperfection characteristic of old natural law and more so of natural right (see, e.g., Aristotle, Ethics) with absolute and categorical standards of condemnation. Moreover, its grounding ends up invoking nature in a way that is hard to reconcile with the initial dismissal of teleology. The "oneness" of the "one-flesh" community, for example, is traced ultimately, as George quotes Finnis, to "the behavior that unites biologically because it is the behavior which, as behavior, is suitable for generation" (p. 146). And this "openness to procreation" is described as the "natural fulfillment" of marriage (p. 168). Is this not natural teleology?

There is much to admire in these essays, but until we receive a less ambivalent account of nature, a defense of NNL, even in George's capable hands, remains incomplete.

Democratic Choice and Taxation: A Theoretical and Empirical Analysis. By Walter Hettich and Stanley L. Winer. New York: Cambridge University Press, 1999. 344p. \$59.95.

Robert C. Lowry, Michigan State University

In Democratic Choice and Taxation, Walter Hettich and Stanley Winer advance the argument that fiscal policies in democratic states should be analyzed as "equilibrium outcomes of a collective choice process that is constrained by political as well as economic forces" (p. 1). Although the authors characterize this as a "new approach to the study of taxation" (p. 284), their central premise is similar to that of University of Chicago economists studying regulatory policy (e.g., Sam Peltzman, "Toward a More General Theory of Regulation," Journal of Law and Economics 19 [August 1976]: 211-40) as well as to Alesina and Rosenthal's political economic model of macroeconomic policy (Alberto Alesina and Howard Rosenthal, Partisan Politics, Divided Government and the Economy, 1995). Nonetheless, Hettich and Winer do far more than just extend to tax policy the idea that policies are endogenous outcomes of political processes. They also provide a clear discussion of many issues that must be faced by scholars interested in modeling policy choices in a wide variety of substantive areas.

Hettich and Winer begin by considering the pros and cons of various models of collective decision making that might be used to study tax policy. They argue that the probabilistic voting model, in which every voter may vote for any party with some positive probability that depends on the economic consequences of each party's announced platform, is superior to a number of alternatives. The chief advantage of the probabilistic voting model is that it generates an equilibrium outcome in a multidimensional setting in which decision makers are accountable to voters. A weakness of the model is that it treats the actual decision-making process as a black box, a problem to which the authors must return in their empirical applications.

Having settled on a conceptual framework, Hettich and Winer develop a formal model in which political parties announce fiscal platforms to maximize their expected votes, subject to a budget constraint and a set of relationships specifying the effects of taxes on the private economy. According to the Representation Theorem originally due to Peter J. Coughlin and Shmuel Nitzan ("Electoral Outcomes with Probabilistic Voting and Nash Social Welfare Maxima," Journal of Public Economics 15 [February 1981]: 113-21), constrained maximization of the sum of voter utilities weighted by the sensitivity of each voter's support to changes in his or her welfare at the equilibrium yields the same platform as a noncooperative game between the parties. This equilibrium platform satisfies the condition that the marginal effect on political support per dollar of net tax revenue is the same across all taxpayers.

The result that political support is maximized when marginal effects are equal for all voters yields immediate insights, as it implies that policymakers should set a different effective tax rate for each homogeneous block of voters. In other words, simplified tax systems such as a flat tax are unlikely to be compatible with vigorous political competition. Hettich and Winer argue that the interesting analytical question is why the tax code is not even *more* complicated than we observe. Their answer involves administrative costs and the government's inability to observe taxpayers' true wage rates and effort.

A second implication of their model is that politically optimal tax rates neither satisfy the normative criteria of

horizontal equity nor match the policy prescriptions of optimal tax theory. The fault here lies with normative theories that rely on a benevolent social planner unconstrained by a collective choice process. One can nonetheless engage in normative analysis by focusing on barriers to political competition, lack of perfect information and coordination across jurisdictions in a federal system, and the extent to which political support for tax policies depends on factors unrelated to voters' economic welfare.

The authors then provide several empirical applications, including a general equilibrium model calibrated to compare U.S. tax policy in 1973 and 1983, as well as statistical analysis of the determinants of income tax provisions in American states and the choice between tariffs and debt financing in Canada from 1871 to 1913. The statistical analysis in particular highlights the limitations of the probabilistic voting model. Specifically, the political support function is stated in terms of individual voters, and the formal proof of the Representation Theorem does not account for the possibility of other forms of influence. Yet, a great deal of qualitative and historical evidence indicates that organized interest groups and institutions such as corporations have substantial influence on tax policy. Integrating these efforts into the theoretical model is not trivial, as it is necessary to consider not only the size of the interest group but also its ability to overcome collective action problems. Then there is the question of exactly how these organized groups obtain political influence above and beyond the influence that their individual members have as voters. Formal models that do not distinguish between different kinds of political support do not provide much guidance for developing empirical measures of interest group influence.

Hettich and Winer also recognize that the outcomes from a collective choice process may depend in part on design of the institutions in which policy decisions are made. Therefore, they compare tax policy in a parliamentary system (Canada) to that in a separation of powers system (the United States). This comparison is somewhat ad hoc, and the empirical tests they provide are not formal. Moreover, they do not attempt to relate this analysis to the probabilistic voting model, which takes institutions as fixed and transparent.

Despite these limitations, Democratic Choice and Taxation is an unusually comprehensive and straightforward treatment of the positive and normative theoretical and empirical issues confronting political economists who study policymaking in a democracy. Beyond their specific insights into the design of tax systems, Hettich and Winer provide food for thought for scholars interested in a wide range of fiscal, regulatory, and macroeconomic policies. Even the limitations of the probabilistic voting model provide a clear set of opportunities for further contributions to this scholarly enterprise.

Religious Toleration: "The Variety of Rites" from Cyrus to Defoe. Edited by John Christian Laursen. New York: St. Martin's, 1999. 252p. \$45.00.

Susan Mendus, University of York, UK

John Christian Laursen is a man with a mission. In fact, he is a man with two missions. The first is to expose the flaws in much modern writing on toleration; the second is to draw our attention to the wide range of toleration literature beyond the rather narrow canon beloved of modern political philosophers and political theorists. For Laursen, there are more things in heaven and earth than are dreamt of in John Locke's Letter on Toleration and John Stuart Mill's On

Liberty. There is Cyrus and Defoe, Bayle and Leibniz, Bartolome de las Casas and Juan Gines de Sepulveda. There is Pufendorf, Le Clerc, and Thomasius. There is Confucianism, Judaism, and Islam. In short, and as the subtitle of the volume notes, there is (and always has been) a "variety of rites." Yet, modern political philosophers and political theorists have largely ignored this variety or, at best, have paid it desultory and inadequate attention.

Laursen's volume aims to rectify this state of affairs. "It is" he tells us "the thrust of this book that much of the recent literature [on toleration] has been seriously flawed in one way or another. Errors of history are common... but there are also conceptual problems, such as distortions of ordinary language that have the effect of drawing the discussion away from what is really interesting about tolerance, which is its paradoxical nature" (p. x).

These are not peccadilloes. Given the centrality of toleration to modern political philosophy, and the eminence of the contributors to the debate (Thomas Nagel, John Rawls, T.M. Scanlon, Michael Walzer, Bernard Williams), the twin accusations of historical ignorance and conceptual confusion are of the utmost seriousness. Does the case for the prosecution succeed? The accusation of conceptual confusion is, in fact, confined to Laursen's own, and very brief, "Orientation" chapter, and it is far from compelling. Indeed, it is not always clear what Laursen takes conceptual confusion to be. Sometimes it is the claim that modern political philosophy is abstract and ahistorical ("most studies in recent decades have been written with geographical, cultural and chronological blinders"); at other times it is the claim that modern liberals present as philosophical principle what is in fact merely personal prejudice.

The first allegation is harsh. In recent years there have been strenuous efforts to contextualize political philosophy, both historical and modern. In the history of political thought, the work of Quentin Skinner in particular and of the Cambridge School in general has done much to emphasize the importance of context, while in modern political philosophy the writings of (among others) Alasdair MacIntyre, Charles Taylor, and James Tully have been largely devoted to exposing geographical, cultural, and chronological blinders. Their efforts have not been in vain, and indeed some would say that they have been altogether too successful in moving political philosophy away from its abstract and universalizing tendencies. Whatever the truth on that, Laursen's "blinders" allegation seems like a bad case of crying "Fire!" in Noah's flood.

The second allegation is also questionable. It is true, of course, that many writers are prone to present personal bias under the guise of general principle, and liberal political philosophers are no exception, but Laursen's chosen example is somewhat disingenuous. He attacks Gordon Graham for concluding that some kinds of Islamic fundamentalism ought not to be accorded the freedom and respect of other religious views. What Graham in fact concludes, however, is that in some circumstances toleration may have to be traded off against other values, such as social cohesion and public order. If the toleration of religious belief, for example, threatens social order, then toleration may have to be sacrificed. So Graham is not (as Laursen implies) advocating intolerance of Islamic fundamentalists. He is raising a general question about the relationship between the value of toleration and other important values. Unless we are prepared to say "let toleration prevail though the heavens fall" (an unedifying and possibly incoherent pronouncement), this is a question we must all address, and it is not necessarily a mere prejudice to conclude that toleration will have limits.

In his eagerness to draw our attention to interesting but neglected works in toleration, Laursen overstates the case for the prosecution. We can agree that discussions of toleration would be better, and better informed, if they took account of a wider range of texts. And that is enough. We do not need to go on to imply that modern political philosophers wear their ignorance of these texts as a badge of honor, or that they have some principled objection to including them in their reflections. For myself, I would dearly love to include them in my reflections, but the days of my age are short, and that is why, despite the reservations voiced above, I am grateful to Laursen for making available a range of work on thinkers and events of whom I was, I confess, wholly ignorant.

To cite just two examples. Zhang Longxi's article on the Chinese tradition of toleration offers a highly informative account of Jewish communities in Henan Province from the eighth century onward, while Nabil Matar's piece, "The Toleration of Muslims in Renaissance England," traces the reception of the Islamic faith in fifteenth- and sixteenth-century England. Ironically, given the title of this book, both authors draw attention to the way in which commitment to religious toleration frequently has been constrained by political and economic considerations. Jews in China were tolerated so long as they did not challenge the supremacy of the secular ruler, and Muslims were accommodated partly because their influence was important for trade.

A principle of religious toleration has rarely, if ever, been allowed to trump the interests of the politically powerful, and the resounding message of this collection is that, through the infinite variety of text and context, when all the nuance and subtlety of different times and places have been accommodated, there remains a single problem of toleration: explaining how it can be right to permit what is wrong. This collection contributes much to our understanding of that question, even if it does not provide an answer to it.

The Condition of England Question: Carlyle, Mill, Engels. By Michael Levin. London: Macmillan, 1998. 194p. \$65.00. Morality and the Market in Victorian Britain. By G. R. Searle. Oxford: Clarendon, 1998. 300p. \$85.00.

G. W. Smith, University of Lancaster, England

Writing in 1866 James Fitzjames Stephen declared that the unyielding commitment displayed by the English to the principles of free trade revealed them to be, contrary to the common view, a thoroughly logical rather than a merely pragmatic people. Even so, by the time of the death of Queen Victoria in 1901 laissez-faire had been under intense pressure in Britain for decades, one reason being increasing foreign industrial and commercial competition, another the growth of antiindividualist moral and political ideas associated with socialism of various descriptions. In Morality and the Market in Victorian Britain G. R. Searle takes the high point of the doctrine to be the radical policy of tariff reform represented by the repeal of the Corn Laws in 1846, and "Victorian" means for him the period between 1830 and 1870. Certainly, by the latter date the reputation of the market as being both economically efficient and "natural" was in serious decline and did not rise to similar heights again in Britain until the advent of Thatcherism in the 1980s. Searle's survey embraces a broad range of activities and institutions, and he is convincing in showing how, even at the height of laissez-faire, Victorians were prepared to recognize and contend anxiously with profound moral problems arising from the tectonic pressures exerted upon customary and valued ways of thinking and doing by the apparently relentless advance of commercial society. Logical or not, Searle shows that they were never doctrinaire.

The author presents a wide range of detailed examples to illustrate the Victorian response to the permeation of market ideas, including the question of the moral standing of capitalism and of profits (frauds, joint liability, entrepreneurialism, business integrity), slavery, problems of poverty and pauperism, the effect on domesticity and women (political economy and the "woman question," contract and marriage, prostitution), the principles and practice of charity-giving, the liquor question and temperance reform, gambling and betting, militarism and the just war, the ethical standing of professions (advertising, professional competition, and so on). God versus mammon is, as one would expect, a major theme: the injunctions of the Sermon on the Mount versus buying cheap and selling dear, and the duties of charity versus the merciless imposition upon the destitute of the "lesser eligibility" principle of the political economy-inspired Poor Law.

Searle well brings out the ethical parameters within which Victorians wrestled with these contending values, particularly the moral easement afforded by the fact that the economic principles in terms of which the claims of mammon were defended were themselves highly moralized anyway via notions central to the tradition of political economy from Adam Smith to J.S. Mill, especially the notion of self as "character" and the associated edifying view of the connection between market competition and the Smilesian cardinal virtues of individual independence and self-reliance. In these terms even the predicament of the anguished cleric (cited by Searle), torn between the precepts of the evangel and the injunctions of political economy on the question of whether bibles should be given away or sold to the heathen at full market price, is perhaps (just) understandable. Indeed a vein of fascinating incidental information runs through Searle's entire account. How many readers know that Sir Robert Peel, inventor of the English police force, secretly and illegally sanctioned free trade in corpses (for the medical dissection business) eighteen years before he legislated free trade in com?

Apart from its strictly historical value, however, this book will, or ought to be, of particular interest to economists, political scientists, and political theorists, among whom there is perhaps still a tendency to theorize questions of markets and morals in sublime neglect of time, place, and circumstance. The message needs to be heard with some caution, however. Searle's political subtext is that the Victorian attempt to set moral limits to the Carlylean "cash nexus," far from being negative in its effect, contributed to the long-term stability of the market in Britain, something that the author believes needs now to be essayed again in post-Thatcherite Britain. The problem is that Victorian experience can offer but limited help here. Although the intellectual currency of the Victorian markets and morals debate was largely that minted by the great political economists themselves—Smith, Malthus, Ricardo, and Mill-it was inevitably, given the context, carried on in the form in which their economic ideas actually sedimented themselves in the popular consciousness, via national political debates over the Corn Laws and the like, by way of popularizations of the principles of political economy by Harriet Martineau and Mrs. Marcet, and through novels critical of hard-nosed "Manchester" liberalism by the likes of Charles Dickens and Mrs. Gaskell. Consequently, the discourse is saturated with Victorian cultural assumptions long ago discarded, and in any event the moral values inscribed in the conceptual framework of classical political economy—character, the place of labor in wealth creation, merit and reward in the market, and so on—no longer carry, especially after Hayek, that kind of authority. Moreover, after 1870, collectivists of various stripes entered the ring to challenge the entire notion of the legitimacy of individualistic market relations. These, too, are now equally obsolete. But the combined result is that the inheritance of thinking on morality and markets is currently, at least in the British context, so profoundly problematic that Victorian ideas appear to have lost virtually all moral purchase.

Thomas Carlyle coined the phrase "The Condition of England Question" in his prescient tract for the times, "Chartism" (1839). The 1840s was a decade of profound social and political difficulty for Britain: deep economic depression, Irish famine, unsettling revolution in Europe, and unprecedented mass agitation (Chartism) in Britain. In what is basically a teaching text, Michael Levin has the idea of using the responses of Carlyle, Engels, and J. S. Mill to these events as a way into, and as a useful touchstone for, their respective social theories. All three writers regarded the repeal of the Corn Laws as a political climacteric, representing a decisive step in the shift of power from the aristocracy to the middle classes. Beyond that there was little in common. For the semisecular prophet Carlyle, English troubles were but a symptom of a human, especially aristocratic, moral failing, and the solution was "great men" and firm government. For the German revolutionary Engels, only the abolition of the entire class system and the end of worker exploitation would answer. Mill was much less apocalyptic than either, looking instead to a gradual amelioration of conditions arising from government carried on with proper respect for principles of political economy.

Carlyle comes away best from Levin's "snapshot" technique as, unlike the others, his position never really developed; his voice simply became louder and more irritable. And it works well enough for Engels, although his analysis of England's woes in his 1844 The Condition of the Working Class in England is basically inchoate, awaiting the benefit of the theoretical stiffening soon to be supplied by Marx. As the most open-minded and reflective of the three, Mill suffers most. Levin relies upon Mill's journalism on the Irish famine, his review of the "Claims of Labour," and the first edition (1848) of Principles of Political Economy. The latter is surely the major piece of evidence, but in the course of its seven editions Mill's view of the causes of poverty, and of the role of the working class in countering the problem, evolved from one of passivism to significant active participation. These reservations aside, Levin presents a well-judged mixture of history and theory in a book that teachers of nineteenthcentury political ideas will welcome.

Early Modern Skepticism and the Origins of Toleration. Edited by Alan Levine. Lanham, MD: Lexington Books, 1999. 282p. \$60.00 cloth, \$22.95 paper.

Cary J. Nederman, University of Arizona

Until quite recently, the history of religious toleration in Europe was a dreary narrative in the literature of political theory. As the story is recounted, the monolithic character of Latin Christianity rendered tolerance a theoretical as well as practical impossibility. With the Reformation came an unprecedented opportunity for the emergence of new structures capable of supporting tolerant practices; and the rise of liberalism in the seventeenth century can be credited with supplying the conceptual principles on which the pragmatic gains achieved during the Reformation could be grounded.

The story pits a heroic few—John Locke, J.S. Mill, and some of their sympathizers—against a vast conspiratorial network of "authoritarians" who would suppress freedom at any cost. This tedious comic book version of the modernizing Enlightenment tale remains popular among those unfamiliar with the cutting edge of scholarship, but advances in the historical literature call it into serious question as an interpretation of the record.

Many of the contributors to this collection make further substantial advances in the scholarly dissent against the conventional narrative of toleration. In doing so, they are guided by the dual goals espoused by the editor in the Introduction: to advance an historically plausible account of the unfolding of tolerant thought while also exploring the contemporary relevance of these ideas for current theories of toleration. At their best, the chapters meet these aspirations very well indeed. Maryanne Cline Horowitz's masterful presentation of the "free thinkers," who, under the influence of Bodin, wrote in the era immediately following the Edict of Nantes, demonstrates the breadth and power of the intellectual arsenal arrayed against the enemies of toleration. Although many of the figures she surveys are unknown to all but the most specialized historians of political thought, their ideas more than stand up when compared to English-speaking early modern advocates of toleration. Similar cases are made for the historical importance as well as relevance of several other authors or traditions commonly ignored in the literature on tolerance in sixteenth- and seventeenth-century Europe. Deserving special mention are Steven B. Smith on Spinoza, Alan Craig Houston on the Levellers, Kenneth Weinstein on Bayle, and Patrick Riley on the philosophes.

The theme that is supposed to hold together this diverse array is the relationship between toleration and another emerging trend in philosophy of the early modern period, skepticism. I am less convinced than Levine, however, that the volume fully accomplishes this aim. Levine's own essay on Montaigne is a paradigm for how the themes may fruitfully be joined. But too often skepticism is highlighted to the detriment of toleration, or vice versa. It is hardly feasible to equate "skepticism" with something like "chastened reason," as Joshua Mitchell does in order to make the case for a tolerant dimension in the writings of Calvin and Luther. And how comfortable can we be with Descartes's skepticism as a basis for toleration when even his advocate, Michael Allen Gillespie, admits that blasphemy, atheism, polytheism, and any questioning of God's existence do not merit the protection of Cartesian tolerance, since they all violate one or another tenet of manifest reason? At the other extreme, some essays—such as those by Houston, Nathan Tarcov on Locke, and Diana Schaub on Montesquieu—barely touch upon skepticism at all, focusing instead upon toleration.

Part of the problem may be a failure in the volume to define the meaning of skepticism very precisely. For some contributors, it seems loosely to designate any position of doubt about human epistemic capabilities; for others, it seems to pertain to a narrow and rigorous set of doctrines. It is certainly true that early modern skepticism embraced a range of positions, but this does not exclude the possibility of identifying some common standard of what renders an argument minimally skeptical. If such a question had been posed, of course, the volume as published might have a somewhat altered table of contents.

Close attention to the detailed arguments of original texts is a hallmark of all the contributions. Unfortunately, there is less consistency in careful examination of the secondary scholarship. No mention is made of Preston King's classic

1976 book (recently reprinted), Toleration, with its powerful attempt to connect the conceptual principles of skepticism to those of tolerance. Reference to Richard Tuck's classic essay, "Skepticism and Toleration in the Seventeenth Century," which offers significant historical evidence against King's thesis, is also noticeably absent from any of the chapters. Those familiar with the literature on skepticism will be surprised to find no citation of J.C. Laursen's 1992 study, The Politics of Skepticism in the Ancients, Montaigne, Hume and Kant.

Given the large number of scholars who have usefully investigated the topic of Lockean toleration, Tarcov's complete refusal to engage (even to recognize) these commentators detracts from one's ability to evaluate the worth of his own contribution. And those who follow recent research on early modern theories of tolerance may wonder why Gary Remer's several articles and his 1996 volume, Humanusm and the Rhetoric of Toleration, merit no acknowledgment. More generally, the contributors seem unaware of the large body of literature on the historical development of tolerance that has appeared during the past decade or so in continental Europe, including work by Klaus Schreiner, Mario Turchetti, Simone Zurbuchen, and Antonio Rotondò.

Finally, the typographical errors are excessive in a book whose contributions are of such high quality. It is incumbent upon publishers to demonstrate the same sort of concern for the presentation of volumes under their imprint as authors and editors show in the impeccability of their research. Nonetheless, Levine and his contributors must be congratulated for producing a uniformly learned and elegant set of inquiries into the foundations of early modern toleration.

The Noblest Minds: Fame, Honor, and the American Founding. Edited by Peter McNamara. Lanham, MD: Rowman & Littlefield, 1999. 256p. \$60.00 cloth, \$22.95 paper.

George W. Carey, Georgetown University

In Federalist 72, Hamilton characterized "the love of fame" as "the ruling passion of the noblest minds." Some 35 years ago, the noted historian Douglass Adair, in an effort to show that the motivations of the Founders extended well beyond narrow and selfish economic interests, built upon Hamilton's observation. He advanced the proposition that the Founders, or most of them at any rate, were motivated by "the love of fame," a passion that reconciled self-interest with the pursuit of noble goals that advanced the common good. This collection of essays is devoted to a close examination of Adair's thesis. Each of the seven contributions that comprise the heart of this volume (Part 2), although they differ in emphasis and scope, deals with a major figure of the founding period and addresses "three basic questions: what the subject of the essay thought of fame, what role the love of fame played in his life, and how and in what way he won fame" (p. vii). What emerges is a picture of the Founders' motivations far too complex to be understood simply in terms of "the love of fame."

Paul Rahe, whose "Fame, Founders, and the Idea of Founding in the Eighteenth Century" constitutes the whole of Part 1, significantly undercuts Adair's contention by noting that the Founders, unlike "the ancients," never "wholeheartedly embraced the ethos of honor and fame" (p. 26). James Wilson, he remarks, in stressing the "primacy of private life" (p. 28) in the first of his Lectures on Law, expressed "the reigning opinions" of the founding era (p. 29). Rahe concludes that "the Founding Fathers...never gave full rein to their longing for fame," although they did seek "a measure of

greatness as statesmen and, in some cases, also as generals" (p. 29).

James Ceasar's "Fame and The Federalist" (Part 3), raises still another concern with Adair's thesis. The author acknowledges that "it is...clear that a desire for fame was an important spur to action for many of the Founders," he comments that Adair's "desire to trump his colleagues's" narrow and base depiction of the Founders' motivations may have "led him too quickly to discount the motive of justice and pure patriotism" (p. 192). Indeed, Ceasar observes, the authors of The Federalist acquired fame for their efforts, although the "love of fame...was not the chief motive of writing the book" (p. 194).

In one fashion or another, the majority of the essays in Part 2 deal at length with the question of whether love of fame can adequately account for the Founders' behavior and accomplishments. John Adams's understanding of motivation, on C. Bradley Thompson's showing, perhaps comes closest to conforming with that of Adair. Adams, as Thompson informs us, believed "benevolent fame is connected to higher principles that the honorable man seeks for selfish reasons" (p. 77). In contrast, Robert Faulkner points out that John Marshall, greatly influenced by the example of George Washington, took his bearings from his "sense of patriotic duty" (p. 177). At a more elevated level, Marshall's heroes, Faulkner notes, were those who resembled Aristotle's "great-souled man" (p. 178), that is, one whose "soul inclines to actions and activities worthy of the superiority of his soul," to a "loftiness or dignity of outlook" (p. 179). Such a view, Faulkner believes, stands outside "a psychology of fame-seeking" (p. 178). In the same vein, Lorraine and Thomas Pangle write that "the love of fame is not adequate as an account of Washington's motives as he understood those motives" (p. 62). His principal motivation, they maintain, was "virtue or true honor," based on a "sublime self-love . . . a love of one's soul that one longs to make as fine and as splendid as possible, by claiming for oneself the most worthy actions" (p. 70).

Peter McNamara, in his essay on Alexander Hamilton, takes care to note that Adair put both Hamilton and Benjamin Franklin in a special category because both sought to rise from their "lowly circumstances" so that "their high ambition" could not be attributed to the "Revolutionary situation" (p. 142). Acknowledging that "the love of fame was the ruling passion of Hamilton's remarkable and controversial life" (p. 141), McNamara notes a complex relationship among fame, ambition, and virtue in Hamilton's thinking and behavior. For instance, Hamilton regarded "the love of fame" as a "peculiar kind of ambition," as a "laudable ambition" or one "governed by principle," such as those dictated by "justice" and "natural rights" (pp. 151-2). Franklin, according to Steven Forde, pursued a different path to fame than Hamilton or, for that matter, than any of the Founders. Although he grew up in an age "in which the notion of fame described by Douglass Adair had become a reigning ideal" (p. 39), he "repudiated neoclassical notions of heroism" because he anticipated "that a new type of individual and new type of fame were foreordained for the coming democratic age" (p. 40).

Lance Banning's discussion of Madison focuses on how "modern scholarship has skewed our memory of Madison's career in ways that might have seemed both curious and painful to himself or his contemporary allies" (p. 124), only superficially touching upon Adair's thesis. Likewise, David Mayer chastises modern scholars for their failure to preserve or to convey accurately "Jefferson's ideas about government" (p. 97). Mayer sets forth salient aspects of Jefferson's thought

and major accomplishments, but his essay does not deal with the intricacies surrounding motivation.

The final essay, by Thomas Pangle, uses the lives of Washington and Winston Churchill as points of departure, to explore "a series of deep puzzles concerning the nature and status of honor" (p. 207), such as how it can be "just or reasonable for the few unselfish and public spirited men of honor to expend their lives, in the role of servants to the predominantly selfish or privately interested many" (p. 209). Among his more significant conclusions is that modern liberal theory is incapable of comprehending what motivates such "men of honor" (p. 218).

This is not a closely knit collection. The essays on Madison and Jefferson are largely of a different order than the others, and some contributions overlap to a considerable degree in their treatment of individual Founders. Nevertheless, on the whole, the authors point up the shortcomings and complexities of using "the love of fame" in the manner suggested by Adair. This collection would serve as excellent outside reading for courses in American political theory or for those that focus on the founding period.

The Political Institution of Private Property. By Itai Sened. New York: Cambridge University Press, 1997. 204p. \$54.95.

Jeffrey S. Hill, Northeastern Illinois University

A friend of mine once characterized rational choice theory as "slash and burn political science," arguing it tries to answer questions as if they had never been asked before, ignoring or dismissing well-established bodies of literature. Nowhere is that less true than in Itai Sened's analysis of the origins of property and other individual rights. Sened develops his own theory by combining rational choice analysis with a careful and complex review of several theoretical approaches. He examines in detail the work of classical political theorists (e.g., Hobbes, Locke, Rousseau, and Hume), classical and neoclassical economic theorists (e.g., Stigler, Coase, Demsetz, and North), and game theory. Along the way, he develops a new understanding of the origins of rights, produces several nonobvious implications, and supports his argument with historical illustrations and an empirical analysis of a special kind of property right: airport landing and take-off time slots.

Sened begins with a review of classical political theory, rejecting Locke's thesis that governments are created to protect preexisting rights. While he prefers the more Hobbesian view that rights can only exist where there is a strong, stable government, he argues it is incomplete. Why would any such government grant rights? There is no moral imperative to grant rights, and doing so would have the effect of restricting what the government can do.

Sened finds only a partial answer to his question in classical and neoclassical economic theories. Many of them deal with it, at least implicitly, but most of them assume governments exist to protect the property rights necessary for an efficient market. This is not a sufficient answer. Creating rights for some places a burden on others, who must now forgo or pay for what they previously enjoyed for free. Rights also create work for the government, since they must be enforced if they are to be effective. We still do not know why a government would provide a right that constrains people (including itself) from using things and that requires additional effort to protect the right. Borrowing a quote from North, the question becomes why a state uses "its coercive forces to grant

and protect private property and related individual rights rather than to rob its citizens of their assets" (p. 50).

For his answer to this question, Sened uses a game theoretic approach. The creation of rights cannot be explained by the Invisible Hand, benevolence, or altruism. Instead, using the basic rationality assumption, he suggests governments create rights because it is in their interest. More specifically, governments create rights in return for political and economic support from their citizens.

The political support motivation can be seen in the creation of the Bill of Rights. These amendments were originally rejected at the Constitutional Convention. "They were added to the Constitution to make it attractive enough to the opponents of the Constitution" (p. 30, emphasis in original). The Bill of Rights may indeed be an example of the wisdom of the Founders, but it was added to buy support. This example shows how Sened's approach provides an explanation for the granting of rights, but, as the author points out, it also shows an assumption that "governments pursue their own interests does not imply that they are insensitive to the interests of their constituencies" (p. 30). A better illustration of this point is seen in one small section of the Magna Carta, which granted widows the right to refuse the king's order to remarry. This right was not granted out of any sense of altruism or respect for women's rights. It was demanded by the barons who challenged King John, since monarchs had previously used remarriage to rob the barons of their property. King John, in turn, granted this restriction upon himself in order to gain the support of his rebellious barons.

Sened uses a similar approach to explain the need for economic support. Without property rights, no production takes place, because the benefits of production are not protected. Only when government creates rights to protect producers will markets develop. But where is the incentive for government to do this? The answer is that the increased wealth created by the market becomes a source of economic support for the government. The conditions necessary for markets are not created out of respect for economic efficiency. They are created to provide tax revenue. For the same reason, governments will not always establish property rights. Since there is a cost to enforce these rights, they will only be granted when the expected benefits of taxes and political support exceed this cost, that is, when the gain in revenue is greater than the cost of enforcement, and when the gain in political support from rights holders is greater than the loss in political support from "duty bearers" (i.e., those obligated to respect the rights). Since governments cannot be certain when these conditions will exist, they will look for information. It is supplied by interest groups and political entrepreneurs, who make government aware of potential sources of support that can be obtained in return for the granting of a right desired by the group.

Some may see this as a trivial argument, whereas others may see it as simply an extension of economic arguments for limited government. In both cases they would be wrong. Sened's contribution provides a subtle twist on the more popular economic arguments on taxes. Taxes are not seen as an intrusion into a previously existing market but as the reason markets can exist. Not all taxes are necessarily beneficial, but without them government would have no reason to provide the protections needed for a market. Similarly, Sened's model suggests interest groups and political entrepreneurs are not threats to the efficiency of the system but instead are necessary solutions to uncertainty. Although the intervention of these groups may lead to economic inefficiencies, their participation is vital if the rights are to exist at all. "Without political intervention, there is little chance property

rights, so crucial for the price mechanism to work..., would ever be established or maintained" (p. 117). Viewed this way, interest groups are more than simply necessary; they are part of "the game of politics...[that] makes both governments and their constituents better off" (p. 117).

Perhaps most important, the analysis provides hope for the creation of rights in places where they still do not exist. Sened shows rights are not the result of benevolent governments. A selfish government is just as likely to produce institutions protective of rights, since it benefits from their creation. This is hopeful because selfish governments cannot otherwise be trusted to be benevolent, and benevolent governments cannot be trusted to exist.

The strengths of this book are such that I hesitate to mention any shortcomings, but there are occasional side comments that require either explication or elimination. For example, Sened explains that heavy pockets of crime exist because the cost of protecting rights exceeds the political and economic benefits. There may be some merit to the argument, but crime is more complex than this suggests. It is not an important part of Sened's larger argument and could easily be dropped or ignored.

In general, the book is a valuable addition to the literature. Its arguments are dense, but the presentation is clear, and the chain of reasoning is continually summarized and restated as it is developed. Readers unfamiliar with game theory will have difficulty following the technical parts of the argument, and those unfamiliar with political theory may have difficulty following the nuanced review of classical political theorists, but all should stick with it, since the remaining parts of the book are worth the effort. For these reasons, the volume is most appropriate for a motivated, advanced class of undergraduate or graduate students. These groups, as well as other political scientists, will find here an excellent illustration of formal, analytic theory as a tool of political science, not just as a subfield of the discipline. The book leads to subtly different views and new answers to established questions in political science.

Democratic Justice. By Ian Shapiro. New Haven, CT, and London: Yale University Press, 1999. 333p. \$29.95.

David Miller, Nuffield College, Oxford

This well-written and engaging book sits at the intersection of political theory and public policy. It lays down and applies principles to determine who should be allowed to make decisions of different sorts if justice is to be promoted. Broadly speaking, Shapiro takes a pluralist position. This is so in at least the following three senses: He believes that decision-making power should be dispersed in a democratic society, so that different kinds of decisions are taken in different places by different people; he believes that the authority structure should also vary from one site to the next, depending on the kind of decision being taken, the relative competence of the various parties to it, the costs of exit, and so on; and he believes that power should be divided within each of these decision arenas, with minorities having channels through which they can express their opposition to the actions of the principal agents.

The general provenance of these ideas, from Madison to Dahl and beyond, will be familiar to readers of the APSR. Perhaps what is most original in Shapiro's book is less his general understanding of a pluralist democracy than his application of that idea to areas of life often regarded as not political at all. Indeed, Shapiro says rather little about democratizing the state itself. Instead, he looks at four main

questions: authority over children, and especially the proper division of powers as between parents and public officials; the legal framework of marriage (responding here especially to feminist concerns about the inequalities of power and resources that pervade the traditional institution of marriage); the control of work, under which heading Shapiro investigates a range of proposals for democratizing the employment relationship; and responsibilities for the elderly, including questions about the distribution of medical resources and decisions about terminating life.

Shapiro's approach, which in effect asks what would it mean to govern each of these areas of life by the democratic principles laid down at the beginning of the book, is fresh and instructive. It has to be said that it does not yield any radically new policy proposals. This is partly because Shapiro observes Rousseau's injunction to take people as they are and laws as they might be, and so he builds a good deal of contextual constraint into the picture. I felt this particularly acutely in the chapter on work. After sketching in the main alternatives to the conventional capitalist organization of work—ESOPs, worker-owned firms, codetermination, and so forth-and identifying difficulties with each proposal, Shapiro concludes somewhat pessimistically (or realistically if you prefer) that the most we can hope for is legal regulation of employment and effective representation of workers' interests by trade unions. "Whatever the disadvantages of unions, to date no alternative form of industrial organization has come along that holds out any hope of doing better" (p. 189). This is surely too gloomy. Without descending into utopia, political theorists should start by delineating the best forms of social and political life, even if they cannot presently see how to get there starting from here.

Although the main focus of the book is on making decisions democratically, Shapiro is also interested in the justice of the decisions made (hence the title of the book). His general view of the relationship between democracy and justice is surely correct: These are two distinct values, and it is wrong to think that laws or policies must necessarily be just simply because they are democratically enacted; yet, the linking of the two in the popular mind is no accident. The theorist's task, according to Shapiro, is to discover forms of democracy that are likely to promote justice and principles of justice that are suitable for implementation by democratic means. It is not so clear, however, how Shapiro wants to place his project in relation to theories of justice of the more conventional kind, such as that of Rawls, which attempt to specify the socially just distribution of rights, liberties, resources, and other such goods. At times, he appears to see his democratic theory as complementing such accounts, by describing the institutions and procedures needed to realize an independently defined ideal of social justice. At other times, he appears to regard his theory as displacing such accounts, in the sense that justice is to be defined as whatever outcomes properly democratic decision procedures will produce. In this latter vein, he attempts to get some mileage out of the fact that theorists of justice-Rawls, Dworkin, Walzer, and so forth-notoriously offer competing accounts of what justice means. The argument here is: Since we can never agree about what justice, substantively, requires, we must instead fall back on democratic procedures as the means to resolve our practical conflicts in a just manner.

But this argument is too quick. It does not follow from the fact that theorists disagree about justice that there is no right answer to be found, any more than it follows from the fact that theorists disagree about democracy that there is no best account of what makes decision procedures democratic. Just as Shapiro is at his strongest when thinking creatively about

how difficult decisions ought to be made, he is at his most vulnerable when handling substantive questions of justice (see, e.g., his too-rapid dismissal of Dworkin's account of just health care, pp. 212–5). Indeed, I would say that what Shapiro needs in order to complete his argument about justice-promoting democracy is precisely a more fleshed out account of social justice itself. This gap manifests itself in a number of places but especially perhaps in the unexplicated idea of basic interests, which plays a key role in the discussion of the government of children in chapter 4 and in the discussion of the organization of work in chapter 6.

Theorists interested primarily in justice may, then, come away from the book feeling a little frustrated. In contrast, democratic theorists and students of public policy will enjoy, and learn from, Shapiro's well-informed and sensitive discussion of a set of policy issues that increasingly concern citizens in liberal democracies.

The Promise of Green Politics: Environmentalism and the Public Sphere. By Douglas Torgerson. Durham, NC: Duke University Press, 1999. 218p. \$49.95 cloth, \$17.95 paper.

John M. Meyer, Humboldt State University

There is now a blossoming of literature that explores the relationship between environmentalism and political thought. Long overshadowed by environmental ethicists, on the one hand, and environmental policy analysts, on the other, this body of writing is distinguished by its explicit engagement with concerns familiar to political theorists. New books on "green" political thought offer in-depth treatment of such themes as pluralism, social justice, rationality, virtue, and property rights. Douglas Torgerson makes a significant contribution to this new literature; his book raises important questions about the conceptions of politics and about the relationship of means to ends within contemporary environmentalist politics.

Instrumental rationality and its embodiment in "the administrative mind" are the primary subjects of critique in this book. At their core is a tragic seriousness that Torgerson finds central to the project of control and domination of nature from which greens vehemently dissent. As an alternative, he points to manifestations of the comic and the "carnivalesque" in environmentalism: early German Green Party representatives in the Bundestag who deliberately flouted standards of dress and decorum, irreverent Greenpeace banners hung from dams and nuclear power plants, the festival that was Earth Day (p. 93). At moments such as these, we might come to see political action as fun (p. 155). Where others have seen these qualities of environmentalist political action as either irrelevant or problematic (e.g., Robert Goodin, Green Political Theory, 1992), Torgerson presents them as constituting a vital alternative that highlights our human fragilities and deflates the pretenses of the administrative mind (p. 87). They are manifestations of what he calls a "performative green politics valued for its own sake" (p. 154), a concept that relies heavily upon Hannah Arendt's distinctive account of the intrinsic value of action within the sphere of the political.

There is much to admire in this book. I commend Torgerson for attending to the importance of our conceptions of politics in relation to environmentalist critiques. He is also persuasive in his call to move beyond often sterile debates that seek to establish the "true" foundation for environmentalist action. In contrast to many thinkers who see deep divisions among those professing environmental concern (i.e., "deep" versus "shallow" ecology; "ecocentrism" versus

"anthropocentrism"), Torgerson advocates an embrace of the "inescapably broad and diverse nature of the green movement," all varieties of which are a part of what he terms the "green public sphere" (p. 25). It is the allure of instrumentalist thinking and strategizing, he argues, that leads us to the mistaken belief that these divisions are of central importance. Finally, Torgerson's engagement with the literature on policymaking—in addition to that of environmental politics and contemporary political theory—enables him to write insightfully about the administrative practices that he critiques and promotes a dialogue with potential to enrich all three fields.

Despite these strengths, two central ambiguities seem unresolved. The first is the relation of Torgerson's green public sphere to the politics of the state. As a point of comparison, John Dryzek-identified by Torgerson as a significant influence upon his ideas—explicitly presents democratization in the public sphere or civil society as an alternative project to the democratization of the state (Dryzek, Democracy in Capitalist Times, 1996). Yet, it is not clear that Torgerson does or can embrace this view. His focus upon policy processes would be incoherent in the absence of a critical engagement with state authority. Conversely, it would be wrong to conflate the aim or function of a green public sphere with the takeover of state power. Given Torgerson's salutary and provocative discussion of "politics," a greater effort to explicate this relationship could have offered further insight.

The second ambiguity regards the status of instrumentalism for green politics. Although Torgerson emphasizes a noninstrumentalist stance, he is also quick to note the irony here: "Even though a key element of the green critique of modern culture is to reject instrumentalism as a prevailing orientation, desperate green concerns markedly reinforce an instrumentalist conception of politics" (p. x). The resulting tension is acknowledged throughout Torgerson's analysis. Nonetheless, the implications are never quite worked through. Although he challenges instrumentalist approaches to both politics and environmentalism, he often equivocates when it comes to defending his alternative conception. Even near the end of the book, self-critical questions threaten to overwhelm Torgerson's advocacy of a noninstrumental politics: "the very idea . . . seems bizarre—virtually a contradiction in terms, a denial of all that makes green politics important" (p. 154). Part of his answer to this virtual contradiction is to balance a noninstrumentalist ("performative") conception of politics with two others (termed "functional" and "constitutive" politics) that have a more instrumentalist flavor. Yet, this does not fully resolve the difficulties that he admits are present in his most distinctive argument.

In articulating his "performative politics," perhaps Torgerson has drawn the wrong lesson from Arendt. Careful to respect the distinctiveness of her conception of the political, he places much emphasis upon both its expressive dimension—in contrast to an administrative mindset—and its exclusion of what Arendt terms "the social question" as part of its radically noninstrumentalist character, in contrast to an emphasis upon political outcomes. The consequence, however, is that it is difficult to see how this conception of politics can be labeled in any sense green. Greenpeace bannerhanging, for example, is quite clearly expressive, but it is not devoid of an intended effect. Does this locate it within an instrumental or an intrinsic realm of politics? If the answer is the former, then the purity of intrinsically rewarding political action seems preserved only by emptying that politics of all substance or content, a consequence hardly consistent with environmental concerns. Yet, to suggest that the bannerhangers are pursuing the intrinsic rewards of acting politically is to ignore or deny the deep commitment to achieving the environmental goals that they espouse. This seems equally problematic and unconvincing.

Torgerson acknowledges at other points that he takes liberties with Arendt's thought, but his close adherence to her formulation of the political here contributes to this unworkable dichotomy between the instrumental and intrinsic value of politics. Hanna Pitkin's reconstruction of Arendtian politics might offer assistance here. Pitkin ("Justice: On Relating Public and Private," Political Theory 9 [August 1981]: 327-52) argues convincingly that while Arendt appears to protect her conception of the political from virtually all extrinsic concerns, in fact "no account of politics or the public can be right that wholly empties them of substantive content, of what is at stake." She concludes that it is not truly the entry of social and economic (and, we should add, environmental) questions that threaten Arendtian politics but, rather, "failing to transform [them] in political activity, letting [them] enter in the wrong 'spirit'" (p. 346). If Pitkin is right, and I think that she is, then we should be able to embrace the green critique of instrumentalism that Torgerson elaborates and at the same time honor the fact that even the most creative or expressive forms of environmental activism—if at all meaningful—have a purpose or an end outside themselves.

Law and Disagreement. By Jeremy Waldron. Oxford and New York: Oxford University Press, 1999. 332p. \$65.00.

Donald A. Downs, University of Wisconsin

I once attended a lecture in which a famous law professor declared that the time had arrived to abandon critiques of judicial activism in the name of rights because "we now have available the definitive public philosophy upon which constitutional adjudication may rest, that of John Rawls." In Law and Disagreement, Jeremy Waldron provides serious arguments against every tenet of this smug thesis.

Waldron argues that such general theories of justice and rights as those of Rawls and Ronald Dworkin are flawed because they disrespect politics, which consists, first and foremost, of disagreement over fundamental principles. Rawls's notion of "public reason" holds that although we might disagree over particular conceptions of the details of justice, we nonetheless "still agree in accepting a conception's more general features" (Rawls, Political Liberalism, 1993, p. 226). Waldron refuses to take a bite of this apple: "We should pause to consider how remarkable this view is. In the world we know, people definitely disagree—and disagree radically—about justice. Moreover, their disagreement is not just about details but about fundamentals" (p. 153). The inescapable fact of disagreement means we must engage in politics, which involves making a community out of the plurality of perspectives that constitutes the human world. "Politics exists, in Arendt's words, because 'not man but men inhabit the earth and form a world between them" (p. 112).

Waldron's theory points to renewed respect for democratic politics and its ultimate product, legislation. Yet, Waldron is not dreamy about the nature of democratic politics. He has little patience for what he calls the "dewy-eyed" school of "deliberative democracy," which stresses conversation and the achievement of consensus as the essence of democratic governance (pp. 91–3). Rather, he is interested in establishing a hard-headed defense of democratic institutions (especially legislatures) based on the authority they earn by the very act of passing binding, formal law in the context of disagreement.

In the first of the book's three parts, Waldron constructs an original "jurisprudence of legislation," which he presents as an antidote to the "discomfort with democracy" that is contemporary legal theory's "dirty little secret" (p. 8). Legal philosophers are "intoxicated" with courts and the charms of constitutional adjudication. But the condition of fundamental disagreement does not stop magically short of the judicial door. Judges disagree among themselves just as much as citizens and legislatures and "make their decisions, too, in the courtroom by majority voting" (p. 15). A "genuinely democratic jurisprudence" must take legislatures seriously.

Legislative enactments are entitled to special respect for several reasons. As Joseph Raz has argued (The Authority of Law, 1979), it is often advisable on pragmatic grounds to follow decisions made by some other body than to have to figure out what to do by oneself; legislative procedures and vote counting are ultimately the best methods for coordinating the conflicts that comprise any polity. (Waldron discusses Condorcet's theorem that the probability of reaching a valid judgment increases with each additional vote, so long as each voter is more likely to be right than wrong, and Aristotle's argument that judgment of a collective body can be superior to that of the individual because more viewpoints are likely to be entertained.) Finally, respect for legal enactment is "the tribute we should pay to the achievement of concerted, co-operative, coordinated, or collective action in the [pluralistic circumstances of modern life" (p. 101).

This understanding leads Waldron to endorse a position similar to Justice Antonin Scalia's textualism (A Matter of Interpretation, 1997). He rejects a jurisprudence based on original legislative intent because there is no single legislative intent except in rare cases. The authority of law is "its emergence, under specified procedures, as a 'unum' out of a plurality of ideas, concerns, and proposals, in circumstances where we recognize a need for one decision made together, not many decisions made by each of us alone" (p. 144).

Part 2, "Disagreement in Principle," provides the epistemological foundation of the argument. Examining Rawls's notion of public reason and the model of legal "integrity" Dworkin presents in Law's Empire (1986), Waldron maintains that "moral objectivity" is a quixotic quest because of the conspicuous diversity of views about justice and the fact that moral realists (unlike natural scientists) have provided no method by which we can settle fundamental moral disputes (pp. 176–80). To be sure, we can reason together and reach tentative moral conclusions. This is what legislatures do, and, Waldron attempts to show, there is precious little evidence that courts do it any better.

In Part 3, Waldron concedes Dworkin's claim in Freedom's Law (1996) that some rights are essential to democratic legitimacy and that appealing to majority decision making to settle the status of such rights is "question-begging." Yet, he maintains that appealing to judicial review is just as questionbegging and is improperly paternalistic, to boot. Constitutional "precommitment" to nondemocratic means of resolving disputes about rights (i.e., judicial review) violates democratic authority and the principle of disagreement. Dworkin and other advocates of judicial review look to courts to find "right answers" about rights, but such arguments ignore Hobbes's fundamental teaching that "any theory that makes authority depend on the goodness of political outcomes is self-defeating, for it is precisely because people disagree about the goodness of outcomes that they need to set up and recognize an authority" (p. 245). The only right that Waldron would bind by constitutional precommitment (the "right of rights") is the right of equal participation, "whose exercise seems peculiarly appropriate in situations

where reasonable rights-bearers disagree about what rights they have" (p. 232).

Waldron would have strengthened his argument had he provided a more meticulous comparison of judicial and legislative protection of rights. The fact that courts encounter the same disagreement as legislatures does not necessarily mean that they are not better over the long haul in protecting rights that matter to democracy. And although Waldron's theory is a needed antidote to the cynicism about democracy that he rightly deplores, at times he seems to veer too far in the opposite direction. American constitutionalism has traditionally been at its best when it embraces a creative tension between trust and distrust of democracy. Along these lines, what are the implications of essentially doing away with a practice by a co-equal branch of government that has become an established part of America's long tradition of constitutional law? In historical context, does Waldron's position ironically embody the same type of theoretical imposition for which he admonishes his foes?

Regardless, Law and Disagreement is a thoughtful, powerful book that promises to contribute to the tension between trust and distrust in the larger marketplace of ideas. Defenders of judicial review (let alone judicial activism) have met a worthy adversary they may not ignore.

Constitutional Construction: Divided Powers and Constitutional Meaning. By Keith E. Whittington. Cambridge, MA: Harvard University Press, 1999. 295p. \$49.50.

Ronald Kahn, Oberlin College

This is a superb, pathbreaking book that demonstrates the dual nature of constitutional change. Through a subtle analysis of congressional-presidential politics, Whittington convincingly argues that the process by which constitutional meaning is defined is not solely the purview of the Supreme Court and lesser courts. He shows that the Constitution gains meaning as a result of the politics of construction engaged in by political actors seeking political and policy objectives. For most legal scholars the Constitution is viewed as a binding set of rules that can be externally enforced by courts against government actors through the process of constitutional interpretation. Whittington brings to light a "second" nature, "the Constitution that penetrates politics itself, shaping it from the inside and altering the outcomes" (p. 1). This is the Constitution that guides and constrains political actors in the process of policymaking as opposed to constitutional interpretation. In the process of "construction," the Constitution is viewed as dependent on political actors, who formulate authoritative constitutional requirements and enforce fundamental settlements.

To demonstrate the process of construction and compare it to constitutional interpretation, Whittington offers four detailed and well-researched case studies. The first examines the debates surrounding the impeachment of Justice Samuel Chase in 1804–5. This chapter shows how the politics of construction "reshaped our dominant conceptions of the appropriate use of the judicial power in American politics" (p. 17). Whittington cogently argues that this constitutional construction "allowed the removal of judges for more than narrowly criminal conduct, but not for simple political expediency" (p. 65). It was an "essentially political standard, but protected federal officers from removal by impeachment for mere technical errors in the conduct of their office, for private political sentiments, or simply for the purpose of creating vacancies" (p. 65).

The second case involves the nullification crisis of 1832-33,

in which conflict about three different versions of federalism erupted over the continuation of the protective tariff of 1816. "The crisis features the states as significant actors... setting the boundaries of government power as well as dealing with the distribution of institutional functions.... The crisis highlights the interaction of policy interests and constitutional principle in constructions, and the use of statutory instruments to consolidate the new understandings" (p. 17).

The third case study is the impeachment of President Andrew Johnson in 1868, sparked by his refusal to honor congressional commitments after the Civil War. "The episode highlights the unintended consequences of constructive activity and the compromised nature of possible outcomes. In this instance, Congress reaffirmed earlier understandings of the impeachment power, crushed Johnson's efforts to strengthen the postbellum presidency, and put the spoils system on the defensive" (p. 18). As in the other cases, Whittington argues that "many of the [political] principles advocated bore only a tenuous connection to the constitutional text, and textual authority was distinctly secondary in advancing and defending those principles" (p. 156).

The final case explores conflicts between President Richard Nixon and a Democratic Congress in the mid-1970s over the construction of presidential and congressional leadership in terms of budgeting, war powers, and intelligence. Whittington argues that the accepted constructions of the day discouraged the emergence of a general challenge to the modern presidency. "Though Nixon and Ford were unable to sustain an expansion of presidential power or to prevent an undercutting of their inherited powers, they did contribute to the ultimate shape of the constructions that were established. . . . Congress was able to establish new understandings of its responsibilities in budgeting, the conduct of foreign policy, and intelligence gathering, as well as to establish the institutional supports to realize those responsibilities" (p. 206).

Whittington argues that the case studies demonstrate the following: (1) Nonjudicial actors engage in constitutional deliberation; (2) the role of the Supreme Court is situated within a context of competing claims to constitutional authority; (3) political actors elaborate constitutional meanings in ways that are inconsistent with standard jurisprudential meanings; (4) political actors emphasize external political principles, policy concerns, and political interests, while "the text" is used as "cover" to influence the construction process; (5) political will and partisanship, not judicial doctrine or objectivity, sustain construction-building; (6) defenders of constructions that seek to perpetuate the status quo argue that their constructions are legally required by the Constitution, whereas those who advocate change stress the contin-

gent nature of constitutional provisions; both sides seek to "identify themselves with the true and required meaning of the Constitution" (p. 213); (7) since political constructions matter to constitutional outcomes, they should not be "excluded from constitutional theory and relegated to mere political science" (p. 213).

This book deals mainly with how constitutional meaning is influenced by politics. To read only this analysis would lead one to overemphasize the role of politics compared to court interpretation on the meaning of the Constitution and the powers of its institutions. A fuller account would explore the conditions under which the politics of construction, as compared to judicial decision making, defines the powers of the presidency and Congress. Under what conditions does the politics of construction, directed by political actors acting instrumentally, influence constitutional meaning? Under what conditions does the legal model, in which Supreme Court justices do not primarily or only act instrumentally, inform the changing meaning of the Constitution, in this case the power of Congress and the presidency? Unless we begin to ask whether the politics of construction is a pattern played out in all areas of constitutional meaning, we may draw false conclusions about the role of political constructions compared to judicial interpretation of the Constitution.

For example, while many scholars argue that politics, not Supreme Court action in Brown v. Board of Education (1954) rid our nation of invidious race classifications, it is important to note that significant social change in this area only began to occur after the Court forced a restructuring of the debate about segregation and racial equality by its actions. In an effort to show how political constructions are used by political actors to secure policy gains, Whittington fails to analyze how Court interpretation influences the overall construction of political debate itself, thereby closing certain avenues of change and opening others to political actors. Perhaps a more balanced presentation of the process through which institutional power is determined would view the politics of construction and the Constitution as binding rules in a more synergistic relationship, with Supreme Court and lesser court decisions and the politics of construction influencing each other, in varying degrees in different doctrinal areas, especially those involving individual rights. Like all paradigmatic, pathbreaking scholarship, this book raises important theoretical issues and subjects for future empirical study. It is must reading for a wide range of scholars of American institutions and political development, law and courts, history, and American political thought, especially those with an interest in historical institutionalism.

American Politics

Neighborhoods, Family, and Political Behavior in Urban America. By Yvette M. Alex-Assensoh. New York: Garland, 1998. 197p. \$50.00.

Kent E. Portney, Tufts University

Over the last decade or so, scholars of political behavior have increasingly turned their attention to the role of "contexts"—characteristics of local areas where people live—to help explain and understand political phenomena previously examined only at national or cross-national levels. At the same time, scholars of urban politics have become particularly

attentive to the role of urban contexts in determining a wide array of problems typically associated with urban areas and inner cities—persistent and concentrated poverty and the emergence of an urban underclass prominent among them. Yvette Alex-Assensoh has written a fine little book that deserves to be viewed as an important contribution to these literatures. I will try to explain why.

The book initially addresses the literature and debates on the causes of concentrated poverty in inner cities, but its primary purpose is to examine "the political consequences of today's urban context, including concentrated poverty neighborhoods and single-parent household structures, on the political behavior of white and black inner-city residents" (p. 4). Alex-Assensoh approaches this issue by contrasting three competing models that purport to explain urban political behavior. After reviewing the standard socioeconomic status model of political participation, rational choice approaches, and various contextual theories, she argues that existing models either ignore contexts altogether or inadequately explore them. Her central thesis is that neighborhood social context, by which she specifically means living in neighborhoods with concentrated poverty, living in predominantly African American neighborhoods, and living in families with single parents, are important determinants, even more important than race, of social and political isolation, electoral and nonelectoral political participation, and "negative political orientations."

Alex-Assensoh examines this thesis by developing a quasiexperimental design in which she selected four neighborhoods in Columbus, Ohio, for in-depth analysis. They were chosen in order to vary the concentration of poverty and racial composition: one was predominantly black (91%) with concentrated poverty (49% of the residents living below the poverty line); another was predominantly black (90%) with less poverty (21%); a third was predominantly nonblack (16% black) with high poverty (49%); and the fourth was predominantly nonblack (4% black) with low poverty (20%). Thus, the racial and poverty context is operationalized as residence in one of these four neighborhoods. She then conducted telephone interviews with samples of between 201 and 432 residents randomly selected from each area. Clearly, the comparison of the selected neighborhoods facilitated the effort to isolate the influences of concentrated poverty and racial composition from each other and from other variables also thought to influence urban political behavior.

Initial analyses focus on bivariate relationships between neighborhood context and a variety of political and social attitudes and behaviors. Subsequent analyses center on evaluating multivariate path models that attempt to sort out the direct and indirect influences of a number of variables. Many of the findings are contrary to much contemporary wisdom and will not be uncontroversial. Alex-Assensoh found, for example, that in the two neighborhoods with a high concentration of poverty, blacks are not more likely than whites to exhibit aberrant social behaviors, and whites are more likely than blacks to exhibit social isolation. She also found that political participation of residents in high-poverty areas is relatively low, but there is a clear tendency for whites and blacks in these neighborhoods to engage in different kinds of activities: Whites contact political officials more frequently than do blacks; blacks are more likely to attend in-person functions, such as community meetings, to try to address and resolve local problems. Family context—whether living in a one- or two-parent household-certainly influences economic marginalization as well as a propensity to vote in elections, but it tends not to influence overall political attitudes or orientation toward politics.

Sorting out the influences of specific "contexts" turns out to be complex. Living in an area of concentrated poverty, or in a predominantly black neighborhood, or in a single-parent family does not produce consistently negative political consequences. It is clear, however, that the context variables often play significant roles in influencing political attitudes and behavior. According to Alex-Assensoh, "context matters," even if it seems to matter differently for whites and blacks.

The would-be skeptic can find fault with some issues. Foremost among these is a general lack of precision when describing the research results. To her credit, the author tries to tackle the formidable challenge of distinguishing neigh-

borhood poverty status and racial composition from personal poverty status and race. Yet, it is often difficult (or impossible) to know whether Alex-Assensoh is describing people who live in poor neighborhoods, or all poor people, or poor people who live in poor neighborhoods. Similarly, it is difficult to know whether she is describing black people, or all people who live in predominantly black neighborhoods, or black people who live in predominantly black neighborhoods. This underscores the apparent lack of a thorough editorial review.

A second problem is more substantive, owing to the fact that there is no discussion of what the concept of "context" means. The reader is left to wonder why the author would treat a household characteristic—family structure—as a context, somehow comparable to the conception of context as a neighborhood characteristic. If family structure is a context, then so should be family poverty status or many other household traits. In short, the family structure portion does not seem to fit in conceptually. Furthermore, in the analysis of family structure, the descriptive language often refers to people "in single-parent family structures" with no discussion of who they are. One assumes that the person is the single parent (perhaps a female), but there are other possibilities, and we should not have to guess.

Nevertheless, from both a substantive and a methodological point of view, this book deserves to take its place among the evolving literature on the political consequences of urban contexts.

The Puzzle of Judicial Behavior. By Lawrence Baum. Ann Arbor: University of Michigan Press, 1998. 215p. \$42.50 cloth, \$18.95 paper.

Sandra L. Wood, University of North Texas

Lawrence Baum provides a much-needed reflection on the state of the discipline. This book surveys the scope of our current information about how judges go about the task of making decisions and determines that, frankly, we do not know much, and what we do know may be subject to criticism and future correction.

Baum uses the framework of goals as a means to explain judicial behavior for two reasons. First, a goal-based framework "encompasses rather well the major issues in the scholarship of judicial behavior" because scholars of courts are often trying to explain "what justice seeks to explain and how they go about trying to achieve it" (p. 11). Second, such a framework "requires systematic consideration of what people are trying to accomplish and directs attention to the processes that determine their goal orientations" (p. 11). Baum's focus on goals serves him well. It allows him to provide insight into the major theoretical argument in the judicial subfield: the relative influence of law, attitudes, and strategy.

Three chapters explore the nature of goals in three ways. First, Baum considers the extent to which "judges act on the basis of goals related to the content of legal policy rather than the array of other possible goals" (p. 21). He suggests a pyramid metaphor, as judges on lower courts are often depicted by scholars as holding and acting on a wide range of goals (policy concerns, reelection, public opinion, promotion to higher office), whereas those at higher levels are usually depicted as concerned primarily with legal policy. The emphasis on goals highlights differences among judges at various levels and the diverse goals they may hold.

Second, Baum considers the mix of legal and policy goals of judges. He takes seriously the notion that legal concepts

may matter to justices and provides a nicely balanced discussion of the probable balance between legal and policy goals. Particularly helpful in this chapter is the discussion of the difference in goals between those on various courts. Assumptions about the Supreme Court, the focus of most judicial research, may not hold true for the myriad other courts.

Third, Baum looks at strategic versus nonstrategic behavior as a means of achieving goals. His ability to show the nature of competing goals may indicate why strategic voting does not dominate court behavior. Baum's treatment of the balance between sincere and strategic voting (pp. 94–5) is particularly well illuminated by his consideration of goals, in particular goals beyond seeking right policy. Strategic voting may be an attractive alternative, especially in intracourt negotiations, but it may be that other goals, such as the approval of certain groups or the inner satisfaction of voting honestly, may push a justice toward sincere voting.

One weakness of the book is that, although it attempts to examine the state of the field and to get various segments to talk and interact with one another more fully, it fails to provide the tools to do so. Baum discusses each area of law with the familiarity and skill of an expert. Indeed, he provides helpful insights for those who have expertise in the area. Yet, Baum's greatest flaw is not providing enough information on the many areas discussed. Since most political scientists tend to be specialists, even within the judicial subfield, it may be that his objective (as I see it) is subverted by these rather superficial descriptions. Baum tries to point out links, common ground, and similarities among various parts of the field and speculates about future links that should be forged. But his book does not provide enough grist for the mill. He summarizes each study very briefly (if at all, and merely refers to some). This is adequate if one has great familiarity with the literature, but most of us do not know all the works cited extremely well and may know little about the empirical work in some areas. Baum does not provide the basics needed to make the connections. For example, in order for scholars of the lower courts and the Supreme Court to forge links, there has to be more crossover between them, but Baum does not give enough information to help make those connections.

Overall, Baum strikes his own balance between his goal of sharing what we know about judicial behavior and what he thinks we need to learn. Although he tends to focus on future research needs at the expense of explaining the current state of knowledge, he provides judicial scholars with a goal of their own: Develop our scholarship more broadly so that we may attempt to unravel the puzzle of judicial behavior.

The New Liberalism: The Rising Power of Citizen Groups. By Jeffrey M. Berry. Washington, DC: Brookings, 1999. 192p. \$24.95.

Kay Lehman Schlozman, Boston College

Sometime during the late years of the Reagan administration a colleague in the discipline remarked: "I have been teaching about political alienation for many years. Now I know what it is to be truly politically aliented." Early in 1995, when Newt Gingrich was behaving as if he had just become president, if not king, I wondered to myself: "If she was alienated then, how must she feel these days?" I can now get in touch with her and say: "If you are still alienated, read Jeff Berry's excellent new book. It actually has some good news for liberals about politics in Washington."

Berry documents the increase in the number and activity of citizen groups in the last generation and demonstrates that—thanks to seed money from foundations, ongoing support

from upper-middle-class members, and extensive and favorable coverage in the media—they command substantial political resources. These groups are, by and large, liberal: Of the five core values represented by citizen groups—equality and rights, environmentalism, consumer protection, good government, and family values—only the last is associated with conservatives in contemporary national politics. The presence of these citizen groups on the national political scene has resulted in a transformation of the domestic policy agenda in Congress such that the material issues dominant in the early 1960s have receded in importance relative to postmaterial issues. That is, issues such as disability rights, school prayer, and toxic waste disposal that have some noneconomic dimension now occupy much greater space on the congressional policy agenda than do old-fashioned economic issues, such as cotton price supports, job training, or trucking regulation. Not only have citizen groups become the principal antagonists to the business organizations that have traditionally figured so prominently in pressure politics, but also they often win in legislative policy conflicts.

An especially interesting subtheme of Berry's analysis is the ways in which conservative and liberal citizen groups differ. Conservative groups are fewer in number and more visible as individual groups; their leaders are more readily identifiable as public figures. They have put more eggs in the basket of mobilizing a clientele that is not particularly affluent—generating huge sums that are, sometimes, devoted to more fundraising. They have dedicated considerable resources to raising and disbursing campaign funds and have put less energy toward traditional lobbying strategies. Consequently, they have been less successful than citizens groups on the Left, especially environmental groups, in shaping legislative outcomes.

This is a very important book both substantively and methodologically. With respect to substance, Berry adds a significant element to our understanding of the meaning of changes in American politics over the past three decades. In light of such trends as the electoral successes of an increasingly conservative Republican Party, the emergence of the Christian Right, and the domination of the judiciary by Reagan and Bush appointees, the era has appropriately been characterized as one in which conservatives have rewritten the terms of political debate in America and have scored massive policy victories. What Berry makes clear is that this characterization simply does not do justice to the reality of policymaking in Congress. Nevertheless, the configuration of the "new liberalism" includes two developments that receive less attention in Berry's argument: the erosion of the white working-class base of the Democratic Party and the attenuation of union membership and power. The net result has been the eclipse of concern with the economic needs of those less well off. In short, within the story he tells are sobering lessons for liberals.

With respect to methods, Berry comes very close to unraveling the principal conundrum confronted by those who seek to do systematic work on interest groups: specifying a universe of political conflicts from which to sample in order to assess whether organized interests make any difference. In a prodigious data collection effort, Berry gathered and coded information on all 205 domestic policy issues about which Congress held hearings in any of three separate years—1963, 1979, and 1991. Moreover, he also monitored media coverage of those issues—listening to television news broadcasts and reading op-ed pieces in major newspapers in 295 days of 1995. Thus, although he sampled years, Berry examines the universe of legislative issues within those years.

My only quibble with this first-rate book is that what is on

the pages claims too little and what is on the cover claims too much. Berry is so modest in his prose that he does not trumpet sufficiently loudly the novelty or the magnitude of the data or the importance of his findings in revising what we think we know about American politics. I would urge linguistic inflation in the prose and would suggest deflating the subtitle to: "Congress and the Power of Citizens Groups." As Berry knows, and elaborates at length in his nuanced conclusion, he would have told a different story had he focused not on Congress but on any of several other arenas of American politics—for example, state and local politics or the judiciary—where conservatives have scored greater successes. In short, I urge students of American politics, whether or not they feel alienated, to place this book on their "must read" shelf.

The Unions and the Democrats: An Enduring Alliance. By Taylor E. Dark. Ithaca, NY: ILR Press/Cornell University Press, 1999. 233p. \$37.50.

Stephen Amberg, The University of Texas at San Antonio

The coalition between unions and the Democratic Party is stronger than ever, according to Taylor Dark. Pronouncements made over the last twenty years that the New Deal has ended are seriously mistaken. Democrats in Congress and the White House work cooperatively daily with leaders of organized labor. The right-wing reconstruction of American politics since Reagan has been unable to dislodge entirely the power of the AFL-CIO in economic policymaking and its influence in electoral politics. Dark makes these surprising claims in a valuably argumentative and well-written analysis that combines rational choice and historical institutional approaches. He focuses on the bargaining relationships between the leaders of organized labor and the Democratic Party from the 1960s to the 1990s within the context of the institutional structures of American unionism and the larger political regime. As with all such exercises, critics can challenge the parameters for bargaining as well as the characterization of the substance of the relationship. What will withstand such challenges and what is enduring is the ongoing and growing exchange of resources between the leadership cadre of both organizations.

Dark connects his analysis to large parts of the American and comparative literatures on labor to make an original contribution to our understanding of contemporary politics. The relationship between organized labor and the Democratic Party has long been problematic for political scientists conceptually and practically. The peculiarities of American unionism are at the heart of debates about American exceptionalism, the New Deal, the welfare state, interest group liberalism, and the transformations of the party system since the 1960s.

Dark conceives unions as interest groups of an especially potent type, namely, those with the ability to shut down significant parts of the economy, and his study focuses on how the leaders of the AFL-CIO plus the dozen most active federation member unions have adapted to changes in the political, economic, and ideological environment in the last 35 years. Dark also roots his analysis in comparative debates about the governing of capitalist societies, among the most notable projects of which was John Goldthorpe's Order and Conflict in Contemporary Capitalism (1984), which recently has been reassessed by Herbert Kitschelt et al. in Continuity and Change in Contemporary Capitalism (1999). Although Dark does not make a comparative analysis, his stress on

institutional structure and party alignments parallels one line of development in that literature.

Major variables in the 1980s debate that recur in *The Unions and the Democrats* are the importance of centralization or fragmentation of bargaining capacity, the significance of nationally specific political institutions, and the shifting character of policy problems, from growth to inflation, after the 1960s. Many countries employed corporatist techniques with some success, one outcome of which was growing union membership. On each variable, the American value predicted failure to employ corporatism and weak unions. What Dark points out, however, is that these are properly matters of degree rather than binary alternatives: American unions lost much of their position in private industry, but organization increased in the public sector, and the political role of organized labor in the 1990s is virtually unchanged from 50 years ago.

The heart of Dark's explanation is the claim that union leaders and elected officeholders are rational actors who seek to maintain their leadership position, in part by exchanging valuable resources with one another to protect themselves from opponents. He draws on rational choice analyses of bargaining regimes, elections, and the legislative process to elaborate alternative models of union-party relationships, which he calls "centralized pluralism" and "fragmented pluralism." The ability to establish and sustain exchange depends on the degree of centralization in the AFL-CIO Executive Council and the party unity of the Democrats in Washington. Both the labor federation and the federal government exhibit famous constraints on centralization and unity. Nonetheless, leadership strategies can mitigate the fragmenting qualities of institutions, in part by the leaders of the respective organizations helping each other with their internal problems. In the mid-1960s—Dark's starting point-George Meany and Lyndon Johnson were able to assert substantial control over their respective organizations in order to carry on high-level negotiations over a wide range of subjects. This centralization helped provoke a "crisis of representation" in the union federation and in the Democratic Party in the late 1960s and 1970s as insurgents challenged the leaders. The crisis resulted in the fragmentation of bargaining capacity, growing distrust, and a breakdown of exchange relationships. Yet, Dark marshals evidence to support his claim that in the 1980s much of the bargaining capacity was rebuilt by Lane Kirkland and Jim Wright when the latter became Speaker of the House in 1987 and has continued with their successors. The book is substantially devoted to presenting fascinating case studies of union-Democratic leadership relationships and of legislative cooperation and conflict involving organized labor.

The close attention to bargaining over legislative and bureaucratic policies is achieved at a cost, which may be high or low depending on what is most important to explain. Dark notes early on that the first purpose of unions is to bargain with employers, but he almost completely ignores labormanagement relations. This lacuna allows him to skim the problem of declining union membership (a relative decline after 1955 and an absolute decline from the mid-1970s until 1998) and faltering collective bargaining policy. Treating unions as economic interest groups is not unusual, and the period on which Dark focuses provides ample support for the claim, but there is a reason we also use the locution labor movement, which is not used for employers. Indeed, the inequality of the bargaining position of employees and employers is basic to labor-management relations as well as to the political bargaining on which Dark focuses. When Dark cites "disruptions of economic production" as one of the

"bargaining chips" of unions that politicians seek "to either avoid or obtain" (pp. 36-7), we might ask when a politician has ever wanted economic disruption.

A distinguishing quality of the New Deal was that the Democratic Party promoted collective bargaining and made it and the unions, which were its embodiment, part of its project for governing the economy. In short, the New Deal was about restructuring the employee-employer relationship. That began to change in the 1960s, just when Dark picks up the narrative. It is no longer true that the Democratic Party promotes unionization and collective bargaining as a primary element of governance, although the majority will protect the status quo of union rights. One might reconceive the labor-Democratic alliance by focusing on the "crises of representation" as part of a restructuring motive. That might help make sense of Dark's final speculation about whether the AFL-CIO can go on much longer cementing its exchange relationship with the Democrats when the party will not help it with its first purpose.

Oh, Say, Can You See: The Semiotics of the Military in HawaPl. By Kathy E. Ferguson and Phyllis Turnbull. Minneapolis: University of Minnesota Press, 1999. 270p. \$49.95 cloth, \$19.95 paper.

Beth Bailey, University of New Mexico

On many levels, this is a book about what we see and why we see it. Investigating the way that the military is "hidden in plain sight" in Hawaii, University of Hawaii colleagues Kathy Ferguson and Phyllis Turnbull argue that narratives (often visual) of naturalization and reassurance work to render the extensive military presence in the islands both natural and seemingly inevitable.

This is a theoretically sophisticated and scholarly work of activism, full of metadiscursive claims about the larger purposes of the authors. They mean not only to analyze the "particular expressions of militarization" in Hawaii but also to provide a "more general critique of American narratives of war and the state" (p. xv). Seeking ultimately to undermine the constrictions that militarization places on democratic citizenship, they craft analyses intended to "deconstruct the apparent seamlessness of these militarized spaces and their power/knowledge techniques...so as to contribute to contesting their hegemony and opening greater spaces for resistance and counterrationalities" (p. 178).

Ferguson and Turnbull analyze four sites of representation: the Fort DeRussy Army Museum, Punchbowl National Cemetery, the Arizona Memorial, and the iconic-pedagogical presence of Senator Daniel Inouye. Employing a methodological tool box that ranges from postcolonial and psychoanlaytic feminist theory to Foucault, the authors have crafted intellectually vigorous essays devoted to revealing the narrative strategies employed in and by these sites.

For example, "Remembering and Forgetting at Punchbowl National Cemetery" offers a reading of Punchbowl, visited by five million people each year, as a "site for the production of the stories the state tells you about why young men die in war" (p. 108). In a tour de force, Ferguson and Turnbull progress from descriptions of the land itself, and its prior meaning to native Hawaiians, through nuanced spacial analyses of cemetery layouts; the influence of Hegelian tropes of history, nature; death, and the state; the experiences of Micronesian islanders and the Japanese during World War II; and gendered narrative strategies of "masculine reason, dark feminine otherness, filial loyalty, and maternal sacrifice" (p. 131).

One of the more compelling assertions of this book is that Hawaii continually challenges the militarized order through elements of excess—the excess of its geographical and physical spaces, of the bodies of its people, of its language, of its complex ethnic/racial identities and relationships. In many ways, Ferguson and Turnbull likewise contest the militarized order through forms of excess. They choose an excess composed of provocative and sometimes extravagant claims, of widely assorted theoretical formulations, of narrative strategy that rejects the carefully argued and empirically demonstrated for intriguing and elipitcal essays, rich with discursions and challenging juxtapositions. The authors intend to disrupt our ways of seeing, not only through their analysis but also in the very structures of their work.

Ultimately, however, a closer integration of theoretical and empirical might have allowed the authors to craft a stronger case. This work is very clearly about the semiotics of the military in Hawaii, not its political economy or its role in U.S. foreign policy. Yet, the military is oddly lacking from the landscape that the authors argue it dominates. We get only an aggregate figure for military presence—no acknowledgment of the different branches of service, of the various ways in which different military forces construct their "heavy" presence on the islands. There is no discussion of why the military is in Hawaii, except perhaps to help overthrow the Hawailan monarch in the 1890s. One can question the militarization of both the United States and Hawaii and still pay attention to the logics and material circumstances that have produced the militarization. The "erotics" of "war talk" and "national security discourse," while fascinating, needs to be paired with a more specific history and analysis of the military presence in Hawaii. Ferguson and Turnbull demonstrate an impressive knowledge of and sensitivity to native Hawaiian history and culture, which lends their analyses weight and credibility. That same credibility does not adhere to their discussions of the military, because they demonstrate so little knowledge of-or interest in-its history, culture, and complex roles in the islands.

That positioning pervades the book. As do many other works that employ cultural studies methods, this one looks with what used to be called a relativistic perspective on such practices as human sacrifice ("nothing anonymous or impersonal about it"), intra-Pacific conquest, and the intensely hierarchical structures and practices of precolonial Hawaiian society (including gender hierarchies and exclusions, quoting, in their notes, Lilikala Kame'eleihiwa's claim: "As a Hawaiian woman, I can frankly say that I would not mind if I never ate with a man. I can think of many more interesting things to do with men than to eat with them") (pp. 111, 220). Yet, the attempts of military historians and Park Service employees to create representations in which "the enemy is not routinely demonized [and] 'we' are not uniformly virtuous" (p. 45) is briefly acknowledged but treated with something between skepticism and disdain. Of course, the authors' larger point is a good one: the politics of an exhibit is not in its fairness or accuracy but in "its constitution of the ground on which to decide what counts as fact" (p. 45).

Ultimately, although the authors call for plurality and openness in our public life, they deny the legitimacy of all but a few voices. One strength of this book is its great willingness to see differently and to embrace such visions as Sam Ka'ai's gently disruptive spatial imaginary of the mainland United States as an ocean of land containing "strange islands" such as Houston, but I was frequently disturbed by the total lack of willingness to acknowledge with any modicum of respect the experience of those who might be construed as the victims of militarization or the failures of democratic citizenship. What

about the young men who fought and died on Tarawa or Okinawa? The 19-year-old soldier in Honolulu for R&R, fresh from the jungles of Vietnam? The pimply-faced adolescent recruit and his younger wife, bewildered by the not-especially-welcoming reception they received on Oahu? What about the tourist, the lower-middle-class 60-year-old white woman who worked hard all her life and saved for years for her trip to "Paradise"? All are implicated in the militarization of Hawaii, but all have presences that complicate the semiotic realm. Despite the brilliance of some of their analysis, Ferguson and Turnbull too often reject the understandings that would allow them to do more than preach to the choir.

The American Dream in Black and White: The Clarence Thomas Hearings. By Jane Flax. Ithaca, NY: Cornell University Press, 1998. 170p. \$27.50 cloth, \$15.95 paper.

Shulamit Reinharz, Brandeis University

Jane Flax is a psychoanalytically trained political scientist at Howard University, a white woman in a predominantly black university. This would be of no significance were it not for the fact that this book is about race and gender as they intersect with power. The case she analyzes is the second hearing of Clarence Thomas when he was being considered for the Supreme Court. The first hearing focused on his competence as a judge, the second—unwittingly—on his character.

As everyone knows, Anita Hill, a former employee of Thomas, was contacted during a background check. Material she gave in confidence about being sexually harassed was leaked to the press, and she was called to Washington to testify. This encounter was televised for three days in mid-October, 1991, and mesmerized the nation. The drama consisted of four parties: Hill, Thomas, the senators (all of whom were white males), and the viewing public. At the end of the day, Hill's testimony was not believed or considered important. Her character was impugned, and Thomas became a Supreme Court justice.

All nomination proceedings for the Court are potentially dramatic and politically significant. In this case, however, additional history was made because the term "sexual harassment" entered our vocabulary. Since then, discussion of this problem continues daily. This hearing became such a watershed in gender relations in the workplace that when the Clinton-Lewinsky affair was at its height, there were newspaper references to "before Anita Hill" and "after Anita Hill." As memory fades, the hearings are remembered as much as the Anita Hill hearings as they are the Clarence Thomas hearings, because Hill's stature and credibility were challenged as much, if not more, than were Thomas's. Moreover, the public may have forgotten that the televised soap opera was hearings, not a trial. White women identified with Hill; black men identified with Thomas, as did black women and white men. There was no way, given this dichotomization, that Hill's words would carry the day.

Anita Hill's life was spoiled, to a large extent, by these events. Hill claims that only now, after becoming a visiting professor of Women's Studies at Brandeis University and then a professor in the Heller School of Social Policy, has she found a home where she can once again work objectively on the issues that concern her. When she took a position at the University of Oklahoma Law School, her welcome was uneven, at best, and endangering at times. Her book, Speaking Truth to Power (1997), is an autobiography that includes detailed information about her experience at the hearings. In

public gatherings I have attended, she unabashedly labels Thomas a perjurer.

After so much has been written about the Thomas hearings, is there anything left to say? Jane Flax has shown that there is. Rather than focus on who was telling the truth, Flax discusses the language itself, to see what it tells us about American society more generally. We see the grandstanding of the senators, the twists and spins of Thomas and his supporters, the attempt to ignore race, and the squeamishness in the face of sexuality. Flax argues that the hearings compelled the American public to confront directly our contradictions. We were compelled to recognize that the law refers to an "abstract individual" who does not exist. "The senators were uneasy because they were temporarily forced to recognize their own race/gender position" (p. 7). For Flax, abstract individualism is not a philosophical or political term. It is the manic defense we use to deny the existence of race/gender domination in the United States. As long as we resort to that defense, this domination will remain, she argues.

The book is worthy of assigning in classes (which I did in my course at Brandeis on theories of social psychology), but it is also marred. It is filled with typos, repeated words, and other trivia that should have been caught by the publisher. These errors are unfortunate when a book is used in class, because they lower the standards to which students might strive. That is exactly what my students told me, when they noted how disappointed they were in this sloppiness. In addition, some of the errors occur in quotations of Thomas's speech, which leaves the mistaken impression that he speaks incorrectly. A second problem is the repetitiveness in the book and in Flax's argumentation. I also believe the book would have been more effective and persuasive if the author had included long passages of the transcript. When fragments alone are presented, the context is missing, and the argument hinges on a few words. This may reflect Flax's training as a psychoanalyst, where a very few words can stand for a whole world of experience. Finally, although Flax clearly understands a great deal about American society, history, and politics, her interpretations are psychoanalytic. She claims, for example, that our defenses, rather than vested economic interests, keep domination in place.

Narratives, in Flax's view, play a powerful role in creating social order. For example, it is her contention that "we" cannot incorporate the history of race relations into "our" identity, because to do so would destabilize the foundation of our beliefs. Although I concur that it is important to investigate the psychodynamics of social life, her argument is both tautological and does not account for change. It also does not account for the fact that many people are doing precisely what she claims cannot be done: Many do have a critical view of the country's self-representation and understand that detachment from pervasive myths is also what America is all about. For this reason, there were many people in the United States, although not a majority, who believed then, and still believe, that a different outcome for the hearings would have been just.

Congress and the Presidency: Institutional Politics in a Separated System. By Michael Foley and John E. Owens. Manchester: Manchester University Press, 1996. 432p. \$74.95 cloth, \$24.95 paper.

Steven A. Shull, University of New Orleans

This well-written and informative book by these far-away observers offers useful insights in what I consider to be a valuable undergraduate text. Few other such texts on presi-

dential-congressional relations are available. Congress and the Presidency is much more encompassing (by more than 150 pages) than similar volumes on this topic, and it is quite comprehensive in some coverage, less so in other respects.

Foley and Owens organize their volume by institution, with separate large sections on Congress and then the presidency. This takes up about two-thirds of the book, followed by a more interesting but brief section comparing the two institutions. The authors present much more material on Congress (100 pages more) than on the presidency, even though they consider them coequal institutions. They cite only some of the emerging literature using the tandem institutions approach. Comparing the two throughout the book would have been preferable.

There is relatively little that is new in the first four chapters on Congress, which deal with institutional change, electoral connections, organization, political parties, and decision making. Chapter 2 provides the best treatment yet in a book of this type on institutional development in Congress. Interesting historical material and nice longitudinal figures appear, and the authors conclude the chapter with four eras of institutional change in Congress. As with other early assessments, however, they overplay the lasting effects of the Republican takeover of both chambers in 1995. Chapter 3 provides the standard information on elections, representation, recruitment, and constituent relations. Chapter 4 has even less new information on organization (especially committees and lawmaking), but it does provide useful comparisons of chamber differences. Party, leadership, and decision making are covered competently in chapter 5.

The section on the presidency has three chapters: organization, public presidency, and decision making. In chapter 6, Foley and Owens provide a quite interesting discussion of accumulation of precedents from Washington through Wilson, with a fusion of them all by Franklin Roosevelt, but their treatments for the stabilization of civilian federal employment and growth of budget outlays are simplistic. The former is partly due to contracting out and the latter should at least be presented in constant dollars (p. 198). There are some inaccuracies (e.g., Clinton did not issue an executive order on gays in the military, p. 201). Elections are adequately covered in chapter 7, although there is the rather odd statement that presidential election campaigns "attract a greater volume of voters than they did in the past . . . and they lead to a popular mandate" (p. 227). The remainder of the chapter covers the media, and very little attention is given to interest groups and the voluminous literature on public opinion. Chapter 8 is a very brief discussion of presidential decision making. Congress and the Presidency recognizes broad theoretical issues, such as the Lowi and Barber typologies, but does not cite empirical use of either authors' work in this chapter.

After nearly three hundred pages, we arrive at presidential-congressional relationships. Chapter 9 is on legislating together, but we learn of no scholars other than Peterson who use this approach. An overview of presidential influence on legislative support is provided, but it is weak on explaining this measure and its relation to CQ's legislative success (p. 302). Here appears the first real mention of popular approval, but only citations showing its influence are given, rather than those discounting it as important for the president's legislative relations. The authors incorrectly state that approval ratings cannot distinguish by policy areas, but Gallup makes just the distinction the authors favor (economic and foreign policy, p. 310). Chapter 10 provides a very elegant interpretation of separation of powers, but chapter 11 is a rather disjointed discussion of policymaking controversies. Nevertheless, the authors make the important point that the two actors share power in almost all major functions of governing, from lawmaking to oversight. Chapter 12 is a bit disappointing, but at least Foley and Owens seem to believe that Congress and the presidency are coequals. They too easily see gridlock in presidential-congressional relations, whereas it is actually a much rarer occurrence than they acknowledge.

Congress and the Presidency makes the important point that these two branches of government genuinely share power in the policymaking process. Although this argument is now more common than previously, president-centered and Congress-centered approaches still abound. Sometimes the authors rely too much on textual rather than scholarly sources, and they occasionally make claims without evidence. For example, I doubt that cabinet secretaries "often remind presidents" (p. 203). The authors suggest there is limited presidential control of the bureaucracy, but an abundant scholarly literature argues for considerable top-down control; they should at least acknowledge this alternate view. Also, some of the sources used are quite dated.

The volume provides more tables and figures than any comparable book. Although some are not the latest values (e.g., pp. 206, 246), most graphics in the book are quite good. Because some of the tables and discussion are rather difficult to follow, the volume will be tough sledding for unprepared or marginal undergraduates. Much of the book is descriptive, but portions will prove demanding for all but the best college juniors and seniors. Some tables are unclear or contain errors (e.g., p. 160), and others have no sources provided (e.g., p. 164). The occasional comparisons with the British political system are useful.

For foreign observers, Foley and Owens know these two institutions rather well. They offer insightful commentary on most elements of presidential-congressional relations but leave other aspects largely untouched. The volume tells us nothing about the presidential role in the prefloor stage of Congress or institutional competition for agenda setting. We are given virtually no feel for presidential liaison and how Congress reacts to such efforts. Still, the authors get most of it right; there is a separated system of largely equal power sharing in most elements of presidential-congressional relations. All in all, *Congress and the Presidency* is a worthy source for students of American government, if less so for scholars looking for new information about these two institutions.

Spinning Wheels: The Politics of Urban School Reform. By Frederick M. Hess. Washington, DC: Brookings, 1999. 210p. \$39.95 cloth, \$16.95 paper.

Melissa J. Marschall, University of South Carolina

Frederick Hess seeks both evidence and answers to a troubling puzzle in urban education: the adoption, replacement, and readoption of reforms that continue to have no noticeable effect on the deplorable performance of our nation's urban school systems. He focuses on not only the pace of reform but also the start-and-stop, chaotic nature in which it is pursued. A primary objective in Spinning Wheels is to develop a political-institutional explanation for why urban school reform has frequently disappointed, and why these disappointments have not produced material changes in the practice of reform. Using comparative empirical analysis to document insights from various case studies, Hess finds that political incentives and the very structure of urban schooling—the short tenure and limited administrative capacity of superintendents, the lack of expertise and political experience of school boards, and the unwillingness of any educational stakeholder to engage in supervising the implementation and ongoing evaluation of educational reform programs—create an environment in which difficult and controversial reforms are avoided and the constant pursuit of reform for its own sake dominates.

A truly new and exciting feature of Spinning Wheels is the data. Using a stratified random sample of 57 urban school districts, Hess conducted 325 structured interviews with six "fixed position" respondents (e.g., school administrators, school board members, teachers' union heads) in each site in order to investigate what he terms "Third-Wave" reforms (five specific measures focusing on curriculum and pedagogy) during 1992-95. The cross-sectional design and relatively large sample provide a rare opportunity to go beyond case studies and empirically test hypotheses about how variation in the administrative, political, and contextual arrangements of urban school districts affect the pace, scope, process, and outcomes of specific reform initiatives. Hess should be lauded for this ambitious undertaking; as any researcher in urban education knows, school board members, district administrators, and the others in Hess's sample are not easy to track down and frequently are either too busy or too wary of outsiders to submit to lengthy interviews.

Hess also should be commended for his serious and careful attention to theory building. He skillfully synthesizes the vast literature on educational research and practice and weaves it together with political science theories in such areas as leadership, agenda setting, diffusion of innovation, and policy implementation. Consequently, the political-institutional framework he develops to understand reform in urban education is more generalizable. Those interested in other areas of especially urban public policy will likely find *Spinning Wheels* helpful not only for understanding the reform process but also for designing their own empirical studies of reform.

There is much to like about Spinning Wheels, but a number of shortcomings seriously undermine the author's ability to test the theoretical argument he develops. What I found most disappointing was Hess's underutilization of the data and his tendency to rely on anecdotal evidence rather than empirical findings that would more convincingly (and appropriately) substantiate his argument and conclusions. For example, in chapter 7, which explores the effectiveness of urban school reform, Hess presents a careful literature review highlighting the factors (e.g., time, resources, commitment, and leadership stability) that contribute to policy effectiveness. Rather than operationalize and test the effects of these factors, however, he analyzes the effectiveness of education reform by simply comparing districts with different levels of reform activity. Based on this comparison, he concludes that "the evidence strongly suggests that more active districts implement reform less thoroughly and that their reforms fare relatively poorly" (p. 160). Given that the data include many of the relevant measures, Hess could have rather easily tested some of the hypotheses implicit in his theoretical framework and provided presumably a much stronger case for his political-institutional argument and conclusions.

The vast majority of the empirical analyses are merely descriptive, but Hess does specify and test one set of causal models in chapter 6. Specifically, he seeks to explain four aspects of urban education reform: the number of reforms proposed during 1992–95, the amount of attention each reform attracted, the amount of controversy attending reform in each district, and the community's rating of school performance (all measures were based on responses to survey questions). Again, rather than formally test components of his main theoretical argument (which emphasizes political and institutional factors), Hess models these aspects of

reform as a function of strictly contextual variables (primarily measures of the school district's socioeconomic status, spending per pupil, and public school enrollment). Thus, his findings indicate, for example, that the pace of reform could be altered simply by increasing the rate of high school graduation among the district populace or by reducing the poverty rate. Yet, the argument Hess wants to make (and actually makes) is that the pace of reform is related to the institutional arrangements of urban public schools—the tenure of superintendents, the inexperience of school boards, and so on. In short, there is a disconnection between the theoretical argument and conclusions Hess draws and the nature and results of the analyses he conducts.

Another issue is Hess's conceptualization and subsequent measurement of reform. Although the theoretical framework attempts to explain urban education reform in general, Hess limits his analysis to what seems to me a narrow and less consequential set of reforms ("Third Wave" reforms include day and time measures, curriculum, evaluation, professional development, and site-based management). He justifies his selection of cases by the fact that these reforms "were just gathering steam in the early 1990s" (p. 25). What about such reforms as school choice, vouchers, contracting out, charter schools, and accountability measures? Such "governance" reforms featured prominently in most other analyses of education reform in the 1990s, and a considerable proportion of the districts in Hess's sample had such measures in place or were considering implementing them during the period of his study.

One problem with focusing solely on Hess's particular set of reforms is that they are largely procedural and administrative and are thus less likely either to alter existing institutional arrangements or to cause immediate and dramatic changes in educational outcomes. Consequently, the feeling that the flurry of reform activity has yielded so little change in urban school districts is likely to be enhanced when looking only at these "Third Wave" reforms. Perhaps more problematic is the fact that the set of policy recommendations Hess promotes for "getting unstuck" from the "spinning wheels of reform" includes a number of the very governance reforms that were excluded from analysis.

Empirical social scientists may at times be frustrated with the methodological shortcomings of *Spinning Wheels*, but the book is extremely accessible to practitioners and lay persons who are simply interested in urban education policy. Hess's carefully articulated political-institutional framework makes an important contribution to our understanding of the causes, extent, and consequences of policy churn and provides food for thought with regard to designing further studies of urban education reform.

Perception and Prejudice: Race and Politics in the United States. Edited by Jon Hurwitz and Mark Peffley. New Haven, CT: Yale University Press, 1998. 272p. \$30.00.

Katherine Tate, University of California, Irvine

This collection of articles on the racial views of white Americans is based on a 1991 national telephone survey funded by the National Science Foundation. Directed by Paul Sniderman and Thomas Plazza at the University of California, Berkeley, the project team included the volume's editors, Jon Hurwitz and Mark Peffley, as well as other scholars across several disciplines. This survey stands apart from mainstream political surveys, such as the University of Michigan's National Election Study, because of its unique experimental design, which permits researchers to address ques-

tions that previous scholarship based on standard surveys could not.

The first two chapters, by Peffley and Hurwitz and by Sniderman, Piazza, and Harvey, describe somewhat redundantly the context of the project and the questions addressed. The contributors enter into the quite contentious debate over the continuing significance or growing insignificance of prejudice and racial hostility in the policy attitudes of whites. Two camps exist. In rather simplified terms, one side claims that whites remain racially hostile to blacks, an attitude that drives their opposition to policies that might benefit blacks. The other side argues that racism has declined and that white opposition to such policies is based on other beliefs, including a principled objection to big government. Because many of the authors are relatively new to the race controversy, they have no longstanding position to defend in the debate, so they are free to take a fresh look at the issues.

James H. Kuklinski and Michael D. Cobb begin their research on the racial attitudes of white southerners with a clean slate. Rather than replicate and analyze the standard but still contested measures of racial prejudice, they set up an experiment to determine to what degree black behavior can make white southerners angry, specifically, black families moving into the neighborhood and interracial dating between black and white teenagers. They juxtapose the reported anger in response to these acts to the anger professed by the white southern respondents to higher taxes, million-dollar salaries for professional athletes, and environmental pollution by large corporations. An open-ended question format provides an indirect measure of racial anger that yields interesting results. For example, when asked indirectly about black families moving in, a significantly higher proportion of white southerners expressed anger-42% in 1991, compared to 7% when asked directly. Although Kuklinski and Cobb have reasons to doubt the reliability of their indirect measure of racial prejudice, they have created a new technique that may help circumvent the social desirability bias inherent in racial opinion surveys.

Kathleen Knight's piece is the least technically sophisticated contribution, with no complicated experimental design, but it is quite innovative. Knight asked respondents to perform a deceptively simple exercise: In your own words explain why racial inequality exists in the United States. Analyzing responses to this open-ended question, she found that no whites claimed the genetic inferiority of blacks, but about 20% said inequality was rooted in the individual dispositions of blacks. According to this group, blacks simply do not try hard enough to succeed. Half the respondents, however, gave responses that linked inequality to external factors, including segregation and racial bigotry. Another 20% gave answers that included both internal and external forces.

The remaining contributions address aspects of the race controversy that have been examined previously, albeit with new data and a higher level of methodological sophistication. Peffley and Hurwitz return to the question of the degree to which whites negatively stereotype blacks and how such stereotypes influence their attitudes about public welfare. Edward G. Carmines and Geoffrey C. Layman follow through on the earlier work of Sniderman and Piazza in establishing that racial prejudice among Democrats has a greater effect on their policy views than does the racial prejudice expressed by Republicans. Laura Stoker carefully establishes that how affirmative action programs are described can powerfully affect the degree to which whites will oppose them. Martin Gilens shows what others have alleged: White attitudes toward welfare policy and policies for the

poor are highly racially charged. Support for antipoverty programs in the United States depends on white attitudes toward blacks generally.

How important, then, is race to American politics? While the editors reject the new racism thesis, *Perception and Prejudice* strengthens the claims of those who argue that race is strongly implicated in the political attitudes of whites today. As the Peffley and Hurwitz chapter makes clear, about one-third of whites agree with statements that blacks are lazy and are not determined to succeed. A majority also believe that blacks are aggressive and lack discipline. For the researchers involved in the Berkeley race surveys, it is not simply a question of whether race matters, but when, why, and how it matters. The way in which white attitudes about blacks are processed and enter into their politics, however, is quite complicated.

The Tourist City. Edited by Dennis R. Judd and Susan S. Fainstein. New Haven, CT: Yale University Press, 1999. 340p. \$40.00 cloth, \$17.95 paper.

Michael A. Pagano, Miami University

Diversity is one hallmark of a federal system. Conventional wisdom suggests that state and local governments in federal polities experiment with a variety of policies and services, serve the diverse needs of their citizens, and create places of work and play that presumably reflect this diversity. Or is conventional wisdom on diversity reflected and strengthened in the world's growth industry, tourism? The Tourist City provides a theoretical perspective and an abundant evidential base that tourist cities, rather than reflect the uniqueness of place in time or history, replicate one another. Through what the editors and some of the contributors refer to as the "commodification of tourism," cities attempt to attract visitors by increasingly creating "sameness" or a "Fordism" of the tourist industry. This commodification is not new, but the articles illustrate how businesses have found successful formulas for promoting a place's historical or cultural identity and replicate the success in other places that do not have such an identity. It no longer is the case that one needs to travel (or "virtually" travel) to places of historical, architectural, or natural significance, because those experiences can be re-created nearby. Investors and multinational corporations seek to provide consumers the products that match not their unique individual needs but their similar, common needs.

Las Vegas, as Robert Parker shows, now is home to 42nd Street, the pyramids, or the French Quarter to the tourist-consumer. Why travel to those places when the "real" experience can be had in a single location? Likewise, festival marketplaces all tend to look alike, cater to the same clientele, and offer similar products. Norfolk or Boston, it seems not to matter much where one goes, because the experience is the same. Fordism, according to Judd's perceptive analysis in an excellent chapter, means that cities promote "tourist bubbles," or islands within a city that are segregated from the rest of the community, and they tend to resemble one another.

Most of the essays are case studies. Richard Foglesong discusses the evolution of Orlando as a tourist mecca, and Arie Shachar and Noam Shoval examine the allure of religious sites as destinations (Jerusalem). Daniel Hiernaux-Nicolas explores the history of the tourist development of Cancun, and Lily Hoffman and Jiri Musil examine the postcommunist tourist explosion of Prague. The principal theme of the studies is "tourism" more than "cities." Indeed,

several chapters offer theoretical insights into the tourism industry and government promotion of tourism, such as Charles Euchner's study on sports and the chapter by Sabina Deitrick, Robert A. Beauregard, and Chery Zarlenga Kerchis on riverboat gambling.

Anyone who has traveled to any theme park or visited any city's tourist bubble can hardly deny the sameness of the experiences (e.g., one can experience a rain forest without ever leaving the desert). The sameness may create consternation among those who want the real thing and who value diversity. Despite the strong case that tourism has been "commodified" (the study by Saskia Sassen and Frank Roost on the "global" entertainment industry is fascinating in that regard), the reader is not told why the consumer would want to pay for it. Keeping construction (and franchise) costs low by mass-producing sameness certainly explains the allure of tourist sites for investors, but why should hundreds of thousands of consumers choose to travel hundreds of miles to have the same experience that they could have had by traveling a thousand miles? Do tourists value the diversity of culture, history, and entertainment, or is it the universal experience that they seek? A study of tourist tastes and consumption patterns would be an important complement to this book.

Scholars will find several of the chapters important in understanding the direction and vision that cities have adopted in the past few decades, not only in promoting tourism, but also, and more important, in accommodating Fordism. The underlying theme of the book is that diversity is threatened, even in federal systems, which value diversity. The commodification of the tourist industry, so well documented in *The Tourist City*, may be only symptomatic of larger concerns about community values, localism, and diversity.

Pivotal Politics: A Theory of U.S. Lawmaking. By Keith Krehbiel. Chicago and London: University of Chicago Press, 1998. 258p. \$50.00 cloth, \$17.00 paper.

Eric Schickler, University of California, Berkeley

The conventional wisdom that divided government causes policy gridlock has proven difficult to dislodge. Nonetheless, Keith Krehbiel not only offers a powerful critique of this idea but also provides an alternative explanation for gridlock: the need for supermajority support in order to pass laws in the U.S. system. By combining innovative yet accessible theorizing with rigorous empirical tests, Krehbiel has made a major contribution to the literatures on divided government, legislative-executive relations, and the policy process more generally.

Krehbiel begins with the observation that there is a shortage of good basic theories of U.S. lawmaking that incorporate both presidential and congressional decision making. Existing frameworks fail to account for two basic facts of lawmaking in America: Gridlock is common but not constant, and coalitions are regularly bipartisan and greater than simple majorities. Krehbiel develops a simple formal model that yields predictions consistent with these basic facts and accounts for variations in gridlock level and coalition size.

The model includes a president and n legislators in a unicameral legislature, each with an ideal point along a unidimensional policy space and single-peaked preferences. Earlier formal models of legislatures focused on the median voter as the pivotal decision maker, but Krehbiel incorporates two procedures that temper simple majority decision making: the president's right to veto legislation (which is

subject to a two-thirds override vote by the legislature) and the right of unlimited debate in the Senate (which generally can only be overcome with a three-fifths cloture vote). The pivotal voter in veto override when the president is, for example, to the left of the median voter is the legislator whose ideal point and all ideal points to her right make up exactly or just more than two-thirds of the legislature. The support of this veto pivot then guarantees override. Analogous reasoning is used to identify the filibuster pivot. The model generates a gridlock interval: a set of status quo policies that cannot be defeated, even though the median voter in the legislature (and thus a majority of legislators) favors a new policy. This interval is a function not only of the floor median but also of the president's ideal point, the veto pivot, the filibuster pivot, and the status quo. The size of the gridlock interval changes in predictable ways as these pivots shift with infusions of new members and other exogenous

After laying out this theoretical foundation, Krehbiel conducts a series of empirical tests to assess the pivotal politics model. For example, drawing upon David Mayhew's (Divided We Govern, 1991) coding of important legislative enactments, Krehbiel shows that changes in the size of the gridlock interval have a substantial effect on changes in legislative productivity, whereas changes in regime (from divided to unified party government or vice versa) have little or no effect. In separate chapters, Krehbiel also assesses how well the model explains the size of enacting coalitions in Congress and vote switching in cloture and veto-override situations. In these chapters, competing explanations, such as party government theory, are also assessed and generally found wanting.

After subjecting the model to these tests, Krehbiel shifts gears and uses pivotal politics as a measurement model. Assuming the framework captures an important aspect of lawmaking, Krehbiel then asks what implications follow for measuring such concepts as presidential power and party government. In one of the most innovative chapters, Krehbiel derives a new measure for presidential power based on the president's success in persuading pivotal legislators in veto-override battles. This measure is not without its limitations (which Krehbiel explicitly acknowledges), but it provides presidential scholars with a theoretically defensible quantitative indicator for one of that subfield's core concepts.

More generally, one of the most unusual and welcome aspects of this book is that Krehbiel includes at the conclusion of each chapter, a caveats section which highlights the limitations of the analysis. As one might expect, the tests of the pivotal politics model vary in persuasiveness because of the vagaries of measurement, of deriving predictions from competing theories, and of model estimation with limited cases. But by highlighting rather than hiding from these difficulties, Krehbiel not only makes it easier for the reader to assess the results but also points to likely directions for future work.

As Krehbiel notes, on the theoretical side, extending the model to include uncertainty and dynamic play would likely improve the accuracy of its predictions. In particular, in its pure form the model predicts no filibusters and no vetoes. Krehbiel's empirical chapters show that when such off-the-equilibrium-path behavior occurs, subsequent events are nonetheless consistent with the model's predictions. Yet, incorporating uncertainty and dynamics—along with position-taking concerns—would likely yield additional insights into the lawmaking process. The assumption of a unicameral legislature also should be relaxed in future work: Differences

in House and Senate preferences are a plausible source of policy gridlock and perhaps even supermajority coalition size.

The modeling assumption that many readers will find most troubling is that the lawmaking game is essentially nonpartisan. Krehbiel defends this assumption by citing evidence that parties in government are weak in the United States and by noting that if the assumption is fundamentally flawed, then the predictions of the model should not stand up empirically (pp. 26-8). In addition, by testing the model against party government models, Krehbiel challenges the idea that a focus on parties will necessarily lead to improved explanatory power. In some cases, however, these tests are open to contrary interpretations that are more friendly to party government theory. For example, in the vote switching analysis, Krehbiel finds that presidents are less successful in winning over members in their own party in veto situations when there is unified party government than when there is divided party control. Since majority party leaders are presumably allied with the president when there is unified control, Krehbiel argues that this evidence is contrary to a majority party government model. Yet, it appears from Krehbiel's data that when the president vetoes legislation under unified party government, most of his party's members tend to vote against his position on both initial enactment and the override. In such a circumstance, if majority party leaders are agents of majority party legislators (rather than agents of the president), then presumably they would be working against the president rather than alongside him. As such, the president's weakness in persuading his party members when he vetoes legislation under unified party control might be taken as evidence of an independent, but strong, majority party.

Such differences in interpretation by no means detract from Krehbiel's accomplishments in this landmark book. He has developed a parsimonious model for the lawmaking process, tested its implications against a wealth of data, and obtained results that offer considerable, if not unequivocal, support for the model. The challenge now is to extend the model to include uncertainty, dynamics, and bicameralism and for critics of the nonpartisanship assumption to show that a partisan model can yield predictions of similar precision and empirical validity.

Chalk Lines: The Politics of Work in a Managed University. Edited by Randy Martin. Durham, NC, and London: Duke University Press. 303p. \$17.95 paper.

Robert Weissberg, University of Illinois-Urbana

Even the most insouciant tenured professor living high in the most comfortable Ivory Tower will surely notice tribulations in today's university. Unease is everywhere. Many departments continue to manufacture unneeded Ph.D.'s, while other disciplines cannot attract sufficient students to fill waiting jobs. Campus bureaucracies swell at alarming rates, while the pusillanimous faculty mumbles about paltry salaries and being "managed" as factory workers to enhance some nebulous productivity. Increasingly, instruction depends on badly paid roving academic "gypsies," while a handful of academic superstars, several of whom enthusiastically bite the generous hand that feeds them, receive extraordinary financial remuneration. Every year tuition increases outstrip inflation. There are even reports of "labor actions" by self-styled proletarian graduate students. Meanwhile, university governance is being dominated by "professional administrators" whose singular charm is inoffensiveness and a graciousness toward every incessant demand. No doubt, this list of unease is easily expanded.

Chalk Lines is a collection of essays that seemingly address these and related issues. The subject is timely. Make no mistake, the health of today's campus is not merely faculty lounge small talk. The matter is serious. Legislators may conceivably tire of funding units of dubious intellectual merit or subsidizing assaults on Western civilization. The secondary public education exodus may soon affect higher education—private industry may find it cost effective to train its work force rather than pay inflated salaries to illiterate college graduates. A "counteruniversity" of think tanks and corporate research centers (often labeled "campuses") is rapidly emerging. Talented undergraduates may abandon altogether the life of the mind. Indeed, just as corporate America underwent drastic downsizing during the 1980s, higher education may be next.

Of the twelve Chalk Line essays, eight originally appeared in a special issue of that Temple of Postmodernism, Social Text. Four are original contributions. It is a very mixed bag, and, measured by the insights-per-page standard, those genuinely concerned with university life will be largely disappointed. This is a book directed to the postmodern faithful. Several contributions are little more than strident, almost cliché-like defenses of labor radicalism applied to university settings. Harney and Moten, in "Doing Academic Work," opine that "most professors in the United States are part of the service sector proletariat in this country" (p. 155). Notably absent are any hard-headed economic analyses of fiscal constraints and the daunting conflicting missions of higher education today. Turgid prose often disguises simplistic, polemical arguments. An unashamed sense of entitlement is pervasive. In his opening remarks, Randy Martin asserts: "The attack on students takes many forms: workfare, tuition increases, scholarship cuts, increased class sizes, and, when they dare to challenge these, the stiff arm of the law" (p. 24). For these academic activists, administrative refusal to grant full-time status to adjuncts, provide lavish health care benefits, or hire all the unemployed English Ph.D.s only proves the evils of capitalism. That this fashionable cultural Left ideology is part of the problem, not part of the solution, is never considered.

Another recurring theme is how modern universities shun their obligations to impoverished minorities. Again, this failure can have only one explanation: evil capitalism. Every attempt, whether remedial coursework or tougher admission standards, is somehow twisted into smoking gun evidence of malfeasance. Multimillion-dollar outreach programs are but disingenuous ruses. The underlying common expectation is that a well-paying academic job and/or worthy degree guaranteeing a well-paying career are sacred rights, and one acquires these benefits via political mobilization. Significantly, nothing positive is offered to accomplish these lofty although debatable goals; to paraphrase the Nike slogan, "Universities should just do it!"

These analytical efforts are overwhelmingly permeated by an affection for the postmodern vocabulary (nearly everything is a "text") and a hearty disdain for business. The style is rhetorical not empirical. Achieving this denunciation of business can take some prodigious twisting of logic. For example, Gary Rhodes and Sheila Slaughter observe what appears to be a paradox: the burgeoning of women's studies and cultural studies in the face of conservative ascendancy in the university. How is this oddity explainable? An unseen devious hand is at work, to wit: "To the extent that these programs become disconnected from larger social movements and critiques of the economic order, or to the extent

that they are unintelligible except to the initiated, these spaces may actually serve to reinforce the existing order and draw strength away from other progressive programs and projects" (p. 59). In other words, business destroys by caving in to the cultural Left!

America's technological preeminence and the long lines of those from abroad seeking university entry are given scant attention. If any palpable accomplishment receives passing mention, this is (again) evidence of business evil-doing. Relentlessly, often in sentences that defy decoding, the reader is informed that America suffers untold grievous economic crises, and the universities have become cruel exploitive instruments while minorities and women are purposely shortchanged. If university-supplied technology has made life easier, this, too, is a plot to heighten social hierarchy. Useful bits and pieces of empirical data occasionally pop up, but these are mere decorations. For example, Stanley Aronowitz asserts that in 1996 30% of the world population (one billion people) was either unemployed or underemployed (p. 212) but how this number was ascertained or calculated is a mystery. Truth is merely proclaimed.

Much of this invective is dismissable, but the persevering reader can uncover some items of merit. Currie and Vidovich's essay on the rise of corporate managerialism in the United States and Australia is one such gem, although its underlying explanatory framework cannot rise above the usual postmodern account. Based on interviews in six universities, the authors find that academic decision making has "become more top-down, bureaucratic, centralized, autocratic and managerial" (p. 122). This nefarious intrusion even dictates how knowledge itself is created (although this is never demonstrated). A particularly pernicious trend is transforming lower level administrators into instruments of higher authority—the department chair as lackey foreman. As many of us have long suspected, this bureaucratic shift was accomplished by burgeoning faculty-administrator salary gaps. Anxious powerlessness among faculty is often the upshot of this power shift. The predictable culprit is the infusion of the business ethos into university life, especially at the higher levels. Nevertheless, the authors conclude on a slightly upbeat note by suggesting that if this managerialism can be dislodged, true collegial decision making can still thrive.

It is said that a pickpocket sees only pockets. Similarly, those embracing the postmodern faith see only economic crisis and capitalist evil. This is most unfortunate, for the dilemmas confronting university life are serious, and conscripting the disgruntled into some grand cultural-political assault invites catastrophe. When ostensible tangible advice is given, it is often of a confused sort. Christopher Newfield (p. 71) advises that the solution to the increasingly businessdominated academy's woes is to increase the university's influence on the business culture. But what contingent of scholars should be sent to corporate headquarters? After all, there are untold departments that might seek this invitation, including those in schools of business or disciplines more sympathetic to business than Newfield's fellow postmodern scholars. We deserve better. The craven power-hungry administrator is not a text, and it is a romantic fantasy to mobilize the toiling adjuncts to storm the Winter Palace. Serious analysis is not performed when the usual suspect business—is unceasingly rounded up. Our insouciant Ivory Tower dweller is uneasy for good reason, but, alas, Chalk Lines will not furnish the necessary useful insight.

The Governor as Party Leader: Campaigning and Governing. By Sarah McCally Morehouse. Ann Arbor: University of Michigan Press, 1998. 322p. \$47.50.

Gerald Benjamin, SUNY New Paltz

In the 1960s, upon entering his Florida hotel room with his family to start a vacation in the depths of winter, an upstate New York Republican county leader found waiting a message from a close political aide to Governor. Rockefeller. "Call immediately," it said. He did. "The governor wants you here," he was told. "Your Assemblyman won't support the budget." The leader protested; "I can handle it by phone." The response was: "Not good enough. Get on a plane."

Leaving his family behind, the county leader flew back to New York City and then drove for hours through the snow to Albany. He and the assemblyman had a talk. The leader reported to the governor's aide that all was straightened out, "I'll go back to Florida now," he said. "No you won't," was the reply. "The governor wants you here until all the votes he needs from your man are delivered." So the leader stayed in Albany for four days, until the deal was done. New York passed its budget on time. Only then was the leader released to return to his foreshortened Florida vacation.

That was party government. A single party controlled the governorship and the legislature. The governor commanded a disciplined party. Party leaders got members elected and did not hesitate to call in the chips when necessary. Those were better days, Sara McCally Morehouse would say, for she is an unreconstructed believer in the importance of political parties for democratic governance. "Only cohesive parties," she believes, both seek to "gather the greatest numbers and intensities and interests in order to govern" and have the capacity to "keep their promises, and ... [thus]... be held accountable." Because of this, she says, "political parties are basic to our survival under a humane governmental system" (p. 281).

This volume, designed to confirm the author's belief in the importance of parties in the states through systematic comparative empirical analysis of their role in electing governors and gaining support for them once elected, is the capstone of Morehouse's long and distinguished career as a scholar in the field. It is based primarily on data for the 1982 election in ten states—five in which parties are empowered by law or practice, five in which they are not-and on legislators' votes in 1983 on governors' program bills in each of these states. The strong party states were Colorado, Connecticut, Illinois, Minnesota, and New York; those in which party leaders had little influence were California, Kansas, Arizona, Tennessee, and Texas. Gathering the data was a prodigious task. Quantitative material for 1982, analyzed with regression analysis, was augmented by interviews with gubernatorial aspirants (or their campaign directors). Visits to state nominating conventions in 1994, and a questionnaire administered to convention delegates in that year, allowed some updating and assessment of changes over time.

The author argues, first, that in states which preserve or permit a key role for party organizations in the nominating process for governor—through formal or informal preprimary endorsement of one of the aspirants—that role is likely to be crucial to the outcome of the nominating process. Second, support of the governor's program by his or her legislative copartisans in states in which the nominating process empowers party organizations is greater than in those in which it does not. Morehouse is especially interested in demonstrating that the practices which strengthen state party organizations make big money less important in the gubernatorial nominating process.

Morehouse confirms the overwhelming importance of incumbency, with primaries (where they occur) used to "tune up" the governor's reelection machinery. Absent an incumbent, campaign money is the key factor in gaining the gubernatorial nomination in nonparty endorsing states, but "in the pre-primary endorsing states, party organization dominates money," and endorsed candidates are likely to be nominated (p. 186). Moreover, persistent intraparty factionalism based on ideology, geography, degree of urbanization, and personality is most evident in states with weak party systems.

In the postelection year, legislative support from the governor's party for his or her program is shown to be greater in states in which the nominating structure empowers the party organization. The analysis is complicated, however, by divided party control of state government during 1983 in seven of the ten states. In such circumstances, as the author notes, governors must go across the legislative aisle to have their program adopted, and they risk alienating members of their own party when they do so.

At the individual level of analysis, Morehouse finds little relationship between the governor's electoral strength in a legislator's district and support of the governor's program. Yet, by correlating socioeconomic data with legislative support of the governor's program for a subset of two strongand two weak-party states (Minnesota and Illinois; California and Texas), she determines that "where party cannot explain voting loyalty, district demands can help provide the explanation" (p. 244).

The follow-up in 1994 suggested to Morehouse that money had gained importance in the nominating process for governor over time. Moreover, there was an increased predisposition for other aspirants to challenge party-endorsed candidates for governor, a tendency that was, she states, probably irreversible as state parties brought fewer resources to the table.

But this research may overstate from the first the value of the party endorsement in strong-party states. For New York, to cite one example, Morehouse downplays the importance of money in influencing the Republican nomination for governor in 1982, but it was not only the measurable amount—how much was actually raised by the potential aspirants—that was key. As important was the calculation, less measurable, by Republican leaders (then the out party) of how the campaign might most easily be financed. Fund raising was hard, these political veterans knew; why not let Lew Lehrman pay the tab, as Nelson Rockefeller had done in their salad days?

Also in New York, Morehouse misses that it was a long-established norm in the state's Democratic Party to arrange for each serious candidate to receive one-quarter of the convention votes on one of several ballots, so that a primary could occur without anyone having to petition for a ballot position under state law. That is, party harmony was maintained by leaders at the convention by making it easier for a challenger at the convention to enter the primary against the party's endorsee. And of course, as the author does note, it has been common in New York for Democratic Party endorsees for governor to lose primary fights for the nomination to challengers, as was the case in 1982.

Students of state government know that there is a cyclical rhythm to the legislative process, defined by the fixed terms for governor and members of each house. The toughest business tends to get done in the year in which all the key players are most distant from their next election. In contrast, the process is most contentious and bills are harder to pass in years in which all the players must leave the capital and face

the electorate at the same time. Morehouse does not consider election cycles in her discussions of party strength and legislative support for the governor's program. One hesitates to be critical of such a carefully constructed analysis of data so painstakingly collected, but this is an important limitation to a single-year analysis.

There are instances in which the author reaches beyond her evidence, as when she comments regarding the rarity of primary battles in Vermont: "Ruralism is an inhospitable environment for political conflict" (p. 41). Some generalizations are clearly at odds with recent experience, as when she suggests that "state parties cannot provide the new technology" of political campaigns (p. 182). In general, however, the limitations of this valuable study come down to the issue, well known to researchers, of what may be best learned from careful, large-scale, quantitatively based comparative analysis and what may be best learned from in-depth study of single systems. Even if detailed knowledge of one state causes the lessons of Sarah Morehouse's work to be modified in the particular, they remain most important in general in informing our understand of the essential role, actual and potential, of party organizations in state politics.

Those were better days? I would agree. New York, with divided partisan control, has not had a timely budget in fifteen years.

Sports, Jobs, and Taxes: The Economic Impact of Sports
Teams and Stadiums. Edited by Roger G. Noll and
Andrew Zimbalist. Washington, DC: Brookings, 1997.
508p. \$49.95 cloth, \$22.95 paper.

John T. Williams, Indiana University

The verdict is now clearly in. The resources used by local and state governments to subsidize professional sports teams, usually to help build and renovate stadiums, do not produce much economic benefit to the host communities. This book does much to discredit claims to the contrary. The introductory and closing chapters by Noll and Zimbalist do a fine job of stating the problem and addressing conclusions derived from the findings in the book. In between, chapters analyze specific cases of subsidies (Twin Cities, Baltimore, Cincinnati, Chicago, Cleveland, San Francisco, spring training, and the minors) and the economic effect of sports facilities. In all, I recommend this book to anyone—whether academic, government official, or interested individual—who wants to understand the economic and, to a lesser extent, political foundations of the recent craze in sports subsidies.

The chapters examine the political economy of professional sports leagues and teams. Most focus on the details of particular cases. Why did a city decide to subsidize a team? There are political reasons, but much of the analysis centers on the faulty economic foundations of policy analyses. For example, most politicians in favor of subsidization cite the major reason, or rationalization, as economic development. When a serious policy analyst tackles this issue, it becomes clear that many of the benefits of professional sports teams are consumptive and cannot really be thought about as economic development. The jobs created are simply moved from one entertainment venue to another. The economic benefits are small because people usually do not travel a long distance to watch a game. Even the players often spend much of their income elsewhere. Of course, there are additional economic arguments, such as improving the inner city, but these do not hold up precisely because the public dollar is almost always better spent on true economic development, if not on something else.

Why does this happen? Politicians, if not always the people, want to keep the teams at even very high costs. One theme throughout this book is that someone is cooking the numbers, and it is the governing elites. Just why they do this is open to speculation, but most likely it is because they want to have a major sports team in the city. In countless referenda, public subsidies are voted down, sometimes over and over, as happened in San Francisco for several years in the early 1990s.

Chapter 5 is one of the most interesting for the political scientist. Rodney Fort shows how political officials have the power to offset the possibility that voters may not support a referendum on sports stadium subsidies. By controlling the agenda, political officials, acting almost like professional team shills, often can rig direct democracy in a way that favors professional teams. We cannot blame the public, as voters have not been inclined to support sports subsidies, so agenda control can be a major source of power for those favoring support for professional teams.

Noll and Zimbalist do an excellent job of summing up the empirical evidence and offering policy proposals in the last chapter. The most innovative suggestions look to the federal government for solutions. The most esoteric involves antitrust regulation. Sports leagues, by their very nature, are monopolies. The National Football League was granted a limited exemption so that it could merge with the American Football League in 1966. Major League Baseball had but recently lost this exemption, granted by the courts in 1922 (Federal Baseball Clubs of Baltimore, Inc. v. National League of Professional Baseball Clubs, 259 U.S. 200). Noll and Zimbalist focus on the fact that leagues are monopolists who limit output (teams), thereby increasing the competition among cities for franchises.

I like the policy proposals that Noll and Zimbalist offer. The most important is to force a breakup of leagues, not unlike the way AT&T was dismantled in the early 1980s. What would happen if federal courts mandated that the American and National Leagues in baseball become independent, as was the case in early professional baseball history? There would be competition across leagues and little ability to enforce guidelines about where a franchise could locate. The problem they identify is that sports leagues limit entry and movement (although this has been found to be a violation of antitrust laws), and cities are in no position to resist the urge to subsidize. That is why small cities have been very successful in procuring franchises when large cities, such as Los Angeles, have been without an NFL franchise for a number of years. Noll and Zimbalist suggest that a number of correctives to antitrust judicial interpretations would decrease the amount of competition among cities for franchises. Foremost among these would be restricting the ability of leagues to limit the number of franchises for any league. More leagues mean more teams and less competition among metropolitan areas for procuring these teams.

So far, so good. But why is there continued interest in subsidizing sports teams, especially since majorities in most cities are unclear in their support? I have one suggestion. Only a slim majority, and sometimes a minority, supports these subsidies. But what about the sports fan? Noll and Zimbalist estimate that the typical subsidy is about \$10 million a year. While hefty, this is not that different from the subsidy for the arts in the top ten or twenty metropolitan areas in the United States. Certainly, fewer people use museums than enjoy professional sports. Simply having a team in the area can be seen as a benefit. Even fans who never attend a game derive pleasure from rooting for a local team. These fans will not necessarily be a majority, but they

may have intense preferences. Thus, one argument in favor of subsidization is the pleasure derived by fans from simply having a team. Even if one accepts this point, however, two problems remain. First, measurement of fan satisfaction above and beyond ticket sales would be difficult. Second, without increasing the number of leagues, the subsidies provided by cities and states could easily exceed these public benefits. I believe this is why analysts have stayed away from arguments such as this.

In short, this is a very good book that details the problems associated with subsidizing sports teams. There are many nice case studies. Furthermore, the introduction and conclusion do an excellent job of bringing things together.

The Making of Telecommunications Policy. By Dick W. Olufs III. Boulder, CO: Lynne Rienner, 1999. 213p. \$49.95.

John Havick, Georgia Institute of Technology

Telephone rates increased when the U.S. government assumed control of the telephone industry during World War I. Government was blamed for the higher rates, which ended any thought of nationalizing the telecommunications industry. That industry, however, observed the advantages of limited government regulation, and for about five decades the governance of communications remained stable and regulated. In a rare instance of a substantial new direction, Congress passed the Telecommunications Policy Act of 1996. How it became law and was implemented is the subject of this book.

The 1996 act made numerous revisions in the status quo. One of the most sweeping changes required regional Bell operating companies (RBOCs) to open up their market to competition. In compensation, the RBOCs were permitted to provide long-distance service, but before doing so they had to prove to regulators that there was meaningful competition in their local market. Other aspects of the 1996 act included less regulation of broadcasting and cable. While much of the legislation was designed to increase competition within the telecommunications industry and decrease regulation, the exception was Title V, dubbed the Communications Decency Act, which sought to eliminate pornography on the Internet.

The Making of Telecommunications Policy explains how the 1996 act reached the agenda, passed Congress, and was implemented. To explain the making of the 1996 act, a political model and an analytic model are used. The political model, which is based on Kingdon's Agendas, Alternatives, and Public Policies (1984), examines problem, policy, and political streams. The analytic model is concerned with knowledge-based policymaking and focuses on efforts to forecast outcomes, preferences of different outcomes, and choice priorities. The two models are not competing explanations; rather, they are different perspectives that each offer understanding and distinct explanation. The models serve to organize and guide the analysis in chapters that trace the evolution of telecommunications policy, examine the process that ultimately produced the act, consider the implementation of the legislation, and reach a conclusion.

What prompted Congress to alter significantly the structure of rules controlling telecommunications? This book argues that a confluence of factors supported a policy change. Several insightful pages explain why technology, and particularly digital technologies, played an important role. The philosophy of competition had already been embraced by policymakers, and the fortunes of electoral politics had produced leaders who were interested in new directions for telecommunications. Thus, the problem, policy, and political

streams all converged to favor change. This is not a quantitative analysis, so readers hoping for a regression coefficient or some other indicator that might weigh the importance of the various factors will not find it. There is no definitive weighting of the relevant factors.

The book's rich understanding, information, and insights about telecommunications and the passage of the 1996 act are skillfully integrated into the narrative and extend the explanation well beyond the confines of the models. Sufficient facts are given to understand a range of scenarios and interpretations for the passage of the legislation. For example, technological change had already altered telecommunications, and U.S. industries were feeling the pressures of global competition. To upgrade and expand the nation's telecommunications infrastructure would cost billions of dollars; and in a regulated environment government would be blamed for increased phone, cable, and other rates. In an unregulated environment, the market would be the culprit. Under such circumstances, a philosophic acceptance of competition and deregulation could be viewed as prudent for politicians seeking reelection. The idea of competition provided a convenient and powerful argument for policy change.

What does the analytic model reveal? Olufs finds meager analytic policymaking at work during the passage of the 1996 act. Telecommunications is notorious for substantial special interest influence over policy. The large telephone companies poured money into congressional campaigns and lobbied energetically, activities that are not conducive to rational decision making. It is not surprising that the industry had a say, albeit not a complete say, in the 1996 legislation. As for the general public, fully one-third still believed their local phone company was AT&T, which had not been the case for more than a decade. The public usually ignores telecommunications and certainly in this case, remained silent and uninformed.

The 1996 act has created change, but because priorities and outcomes were not well articulated, what occurred was not always what was anticipated. Local telephone competition has been slow to emerge, and regulators have not permitted RBOCs to enter long-distance markets. Implementation of the act largely fell to the Federal Communications Commission, and Olufs maintains that the FCC controlled the outcomes. "So it appears that the study of policy in this case should not focus so much on legislation as on the rules adopted by the FCC" (p. 186).

About one-sixth of the national economy focuses on telecommunications, so many scholars and analysts should have an interest in the topic. In particular, the book should attract several sets of readers: policy analysts seeking to expand their focus beyond the policy areas more frequently studied, people interested in agenda setting and formulating of policy, students of Congress, and anyone interested in the making of telecommunications policy or the 1996 act. This book falls between an insider account, such as Coll's The Deal of the Century (1986), about the break-up of AT&T, and Temin's The Fall of the Bell System (1987), a densely detailed and referenced account of the break-up. The Olufs book has considerably more analytic power than either of these. It is closer in quality and emphasis to Bicker's chapter in the work edited by Campbell, Hollingsworth, and Lindberg, Governance of the American Economy (1991). The Making of Telecommunications Policy is among the more important contributions since Horwitz's The Irony of Regulatory Reform (1989). Anyone who wants to understand telecommunications policymaking should read this book.

Tales of the State: Narrative in Contemporary U.S. Politics and Public Policy. Edited by Sanford F. Schram and Philip T. Neisser. New York: Rowman & Littlefield, 1997. 270p. \$65.00 cloth, \$22.95 paper.

Anne L. Schneider, Arizona State University

Tales of the State offers interesting examples of the role of stories, or narratives, in politics and public policy. Although some chapters are stronger than others, the collection overall makes important contributions to a more pluralistic approach to policy analysis. The editors justify the approaches represented in their volume on the grounds that "stories are used by elites and masses alike to make sense of events according to a less than factually documented storyline (p. 1). "Stories" are defined as discrete narratives that explain social or political phenomena, such as apartheid or welfare dependency.

The editors attribute to stories a foundational role and contend that stories are "already there" (p. 5). The foundational role is illustrated in two ways, and most political scientists and policy analysts will understand both. First, stories are "already there" in the sense that they are written into the perceptions of individuals, so that the facts surrounding events are selectively understood in light of predominate stories. Second, stories are foundational for behavior when behavior is motivated by an attempt to emulate norms or values that are found in narratives. The role of stories in politics and policymaking is explained as follows:

Political actors use stories as scripts for engaging in political performances, from lobbying to policy formulation, from mass mobilization to posturing by political leadership. And since it is almost always the case that more than one story can be told to make sense of any particular areas of concern or issue, stories are necessarily not innocuous and disinterested but instead are potentially dangerous and interested. They are critically constitutive elements in shaping political practice and public policy" (p. 6).

The editors claim that "the primary goal of policy analysis should not be to distinguish reasoned deliberation from instances of rumor mongering, but to interrogate all policy making activity for its narrativity and assess the consequences given the persuasiveness of particular tales" (p. 6). Despite this apparent nod toward strict constructionists and many postmodernists (that there are no "facts" against which "stories" or "representational practices" can be juxtaposed), the editors reject the notion that all stories are equally legitimate. "Although it might not be possible to get entirely beyond narratives to a purely factual basis of the world, some tales are tall tales, not only in that they do not accord with established facts, but also because such stories constitute a form of denial or a construction of reality that erases alternative knowledges, perhaps covering up what would otherwise be obvious facts" (p. 8).

At times, the reader may be somewhat confused by seemingly contradictory statements that, on the one hand, argue for the primacy of interrogating stories and assessing their consequences (rather than comparing factual with narrative knowledge) but, on the other hand, acknowledge that there often are facts available and it is well worth one's time to interrogate stories against accepted facts. Some of the articles rely mainly on the first approach—examining the consequences of stories without (much) regard for their factual basis—but most use a "contextual constructionism" to examine both the facts underlying various claims as well as the consequences of those claims. Joel Best's article, "The Van-

ishing White Man: Workforce 2000 and Tales of Demographic Transformation," is an excellent example, as he examines in detail the origin of a major common misinterpretation of workforce demographic changes. From the original study, the authors could have concluded, based on the data, that the proportion of white males in the workforce would decline from 48% to 45% by 2000. Instead, the authors wrote that "the small net growth of workers will be dominated by women, blacks, and immigrants. White males ... will comprise only 15 percent of the net additions to the labor force between 1985 and 2000" (quoted by Best, p. 176). Best offers possible explanations and traces the consequences of this particular claim made in the social science study.

Another approach is illustrated in Michael J. Shapiro's chapter, "Winning the West, Unwelcoming the Immigrant: Alternative Stories of 'America.'" He traces hostility toward immigrants back through history, attributing much of it to the notion that a unified, homogeneous, white, national, European "culture" first "conquered" the West against the "savage" Indians and has been constantly threatened by other races and cultures. Against this orthodoxy, Shapiro takes an ethnohistory position and argues that Indians were not merely driven off or defeated but left lasting cultural marks on "America. "Certainly, many of the Native American cultures became to some extent Europeanized, but it also is the case that English colonial culture became in part Indianized, with lasting historical effects" (p. 24). The history of culture, he argues, has been a process of coinvention, not the single achievement of one dominant culture. "Unified and fixed national cultures are fictions, productions of national fantasies" (p. 26).

Other chapters take similar approaches in examining how particular stories rise to ascendancy, how alternative stories are just as likely (given the facts and events), and the consequences of accepting one story rather than another. Carl Swidorski offers a Marxist analysis of constitutional tales; John Kenneth White examines how contemporary U.S. presidents have used storytelling to gain popular legitimacy; Sanford Schram deconstructs the politics of the language found in the Contract with America; Leslie Vaughn analyzes the rhetoric of Clarence Thomas in his confirmation hearings. There are interesting short articles on welfare, gay rights, NAFTA, globalization, black criminality, and many others.

The editors initially posed three goals for the book. First, highlight the role of narrative in politics and policymaking. In this the book succeeds admirably, as its 19 chapters examine stories across a wide array of policy arenas and political events. Second, promote a "more politicized policy analysis." This goal is not clearly explained. The editors could mean that the book will introduce into the adversarial political arena counternarratives to the dominant (elite-inspired) stories and their consequences. Or they could mean (by "politicized") that they hope to democratize policy analysis by illustrating how elite-inspired narratives are deficient either in their factual bases or in crowding out equally likely narratives that would have more democratic consequences. Some of the chapters fall into the first category and become little more than "claims" that have no more legitimacy than the stories they seek to replace. This would be a devastating approach to policy analysis, in my judgment, because it would only encourage the type of degenerative, hyperpluralistic politics that has produced numerous policy failures over the past decade. The second possible meaning of the phrase "politicize" is to democratize policy analysis, but to do so while adhering to common standards of evidence and reasoning. Building on these two, the third goal is to "undermine the validity of the elite/folk distinction" that permits some (elites, experts) to construct tales of the state and discourages

others (common folks) from finding a voice in public policy. The book contributes to this goal, and it is one worthy of more attention from political scientists.

The Congressional Black Caucus: Racial Politics in the U.S. Congress. By Robert Singh. Thousand Oaks, CA: Sage, 1999. 240p. \$54.00 cloth, \$24.95 paper.

Dianne M. Pinderhughes, University of Illinois, Urbana-Champaign

Robert Singh introduces his study with a review of three perspectives on why the caucus has not been successful in advancing its distinctive policy agenda. One possibility is the strategic argument, namely, that there has been a failure of black leadership to develop appropriate political plans to win. A second is the ideological case of black exceptionalism: Because American blacks are distinctively liberal, emphasize liberal governmental programs that require substantial expenditures, and remain so in a polity that has grown increasingly conservative, their preferences are simply outside the mainstream. Singh also explores a third structural explanation: institutional factors such as a majoritarian system, bicameral legislature, separate bodies at the national level, and a variety of other features interact with and exacerbate the group's ideological position at the edges of the political continuum. The volume carefully weighs these three explanations and offers an impressively detailed view of the complexities associated with the efforts of an organization to represent its own constituents and a national black constituency within Congress.

Singh treats the caucus as both a collective black entity and a congressional entity. He explores its growth and development over time in both these dimensions, which means that Singh integrates the perspectives of the caucus members in all their complexity. The study is substantially enhanced by the use of a comparative theoretical framework, in which Singh illuminates the contrasts and consequences of institutional variations in national polities, a majoritarian versus a parliamentary system, and a two-party versus multiparty system. The comparative framework enables the author to distinguish the structural characteristics from the racially specific characteristics of the American institutional system.

The book addresses the caucus's activities as a group, activities as members of or within the caucus, and views on its role by blacks and other congressional actors. Chapters 2-4 examine black congressional representation historically; chapter 2 covers roughly the century after the Civil War, when there was modest if inconsistent congressional representation; the three decades when there were no black representatives; and the period from the Depression through the 1950s, when northern black representation began and grew. Singh considers the caucus's origins in chapter 3 and in chapter 4 examines its growth and development within the context of increasing conservative Republican strength in national political institutions. Chapters 5 and 6 treat the caucus members within their "representational environments...at home" and their "institutional environments... on the hill" (p. v). Chapter 7 reviews the caucus in the 1990s, when it underwent rapid expansion as a consequence of broad use of majority minority districts after the 1990 Census, and when a number of its members achieved considerable seniority. Singh concludes with a reconsideration of the theoretical puzzle he posed at the beginning of the book, that is, why the caucus seems not to have been more effective in representing the policy interests of its constituents.

Singh has created an unusually broad theoretical foundation for the study, reviewing both American and European legislative, institutional, racial/ethnic, and partisan research. The author also devotes several chapters to an historical and highly detailed examination of the growth of African American political participation and congressional representation, as well as to the growth and evolution of the caucus. The book is a highly theoretical discussion of a racially distinct group that also occupies structural space within an American political institution. Singh sustains these dimensions throughout the study with admirable control and clarity.

Singh's exploration of racial and theoretical factors as well as his careful evaluation, comparison, and weighing of strategic, ideological, and institutional forces in their effect on the caucus makes this book such an important contribution to the literature. Singh examines the importance of the caucus as a racial group but also compares its role as a caucus with other legislative service organizations, of which there are many kinds. The black caucus's homogeneous partisanship and strong liberalism, for example, mean it holds "a distinct strategic position compared with most caucuses" (p. 60).

Race is a powerful force in American political life, as Singh's work affirms, but it is not a stable and unchanging factor. The caucus was conceived as a body that would have national responsibilities for representing the black population as a whole, but as it has grown these external orientations have been abandoned for institutionally oriented activities. As the national political environment has become more fiscally conservative, under both Republican and Democratic administrations, the caucus has played a more oppositional role in terms of policymaking. It has been less successful as an interest group, but Singh argues that it was an effective labor union for its membership in the earlier decades of its existence.

Those who expect a highly partisan analysis of the caucus will be disappointed; Singh is not especially critical of recent work, such as Swain's Black Faces, Black Interests (1993) or Thernstrom's Whose Votes Count (1987), which challenged the effectiveness of black elected officials. Rather, Singh offers a distinct alternative to research centered on the racial/ethnic character of the group as problematic; he also does not explain away the faults and weaknesses of the caucus or emphasize its strengths as a racial group. Singh reframes the way in which scholars will examine racial and ethnic organizations by developing the complex institutional context within which such caucuses functions and by illustrating the ways in which groups with similar structural rather than racial or ethnic characteristics, in other national settings, successfully influence policy. The latter do not operate in majoritarian two-party systems, with bicameral legislatures, single-member districts, and other structural features that limit the effect of coherent minority political interests.

Singh concludes: "The institutional consequences of that presence for public policy regarding blacks remain modest. It is difficult to see what more the caucus could have effectively done within the structural confines of the American political system to achieve its goals without either alienating its black constituents or forfeiting its ideological integrity" (p. 198). Singh has written an unusually important analysis of a racially constructed American political body, which operates within the heart of a national political institution.

The Reagan Effect: Economics and Presidential Leadership. By John W. Sloan. Lawrence: University Press of Kansas, 1999. 311p. \$35.00.

Erwin C. Hargrove, Vanderbilt University

This book is aptly titled because, while it is a study of the politics of economic policy, it is also an acute analysis of

Ronald Reagan the politician. Sloan's central thesis is that Reagan was skillful in combining broad thematic appeals for a popular audience with pragmatic flexibility in policymaking. This is the best Reagan portrait that I have seen because it helps close the gap between Reagan's ignorance of policy substance and his political ability. He provided a sense of direction and let subordinates do the heavy lifting, thus protecting himself. This was anything but incompetence. The drawback, of course, is that a price had to be paid for ignorance in the form of chronic budget deficits. But we did experience 92 straight months of economic recovery.

The Reagan problem is that one cannot get into his mind, and he was not forthcoming with his intentions and calculations. Sloan says, correctly, that Reagan was less impressive privately—in the White House—than publicly. He was passive and not curious and relied on platitudes. But the combination of persuasive popular rhetoric and tactical flexibility revealed his astuteness and resiliency. The historical timing was fortunate. The Carter presidency reflected the inability of Democratic politics and Keynesian economic theory to address the "stagflation" of the 1970s. The oil shocks, high taxes, and inflation of the Carter years were sitting ducks for the charge that excessive government was the problem and that a liberated, more entrepreneurial economy would see a return to prosperity in which, in a populist message, all would benefit. Americans wanted an optimistic view of the world and associated Jimmy Carter with sour pickles. The hour was right for Reagan.

The book is worth reading for the very strong case studies alone, each in a separate chapter, on the economic program of the first year, the passage of tax reform, the savings-and-loan debacle, and the administration's skill in balancing lipservice to free trade with defensive measures to serve domestic politics. One will also find a close analysis of conservative economic ideology as it impinged on Reagan—supply-side economics, monetarism, orthodox budget balancing—and how Reagan played these off against one another to his own political advantage.

But the central story is that of the politician who kept to his rhetorical goals but compromised in particulars whenever it was politically necessary. He began well by selecting James Baker as the White House chief of staff, over the ideologue Ed Meese, and thus permitting Mike Deaver, the most skillful of all public relations men, to join with Baker and other pragmatists in balancing populist rhetoric and practical politics, but without ever saying a word about his strategy. Reagan was more impressive publicly than he was privately, and necessarily so, because he knew how to dramatize public issues and to radiate personal authenticity, but he saw no purpose in engaging in detailed discussion of economic policy choices. He would set the general direction and rely on others to do the work. But, and this is Sloan's key conclusion, Reagan knew how to push policy in a general direction by trimming and tacking and compromising whenever necessary. Thus, although he and his economists had not expected Paul Volker, chairman of the Federal Reserve Board, to tighten the supply of money so sharply in 1981 and 1982, Reagan was happy to sign tax increases in 1982 that mollified Volker and led to a looser Fed policy as well as economic recovery and reelection in 1984.

Sloan gives us the delicious irony that Reagan often did better as a politician when he was ignorant of policy substance. Too much briefing in the first debate with Mondale confused him because, habitually, he would try to learn it all, and his mind did not work that way. Keep to the big picture and rely on him to turn the dynamics of debate in his direction by rhetoric. That was what worked. I once heard Richard Darman, Baker's assistant in the White House and at the Treasury, say that Donald Regan, the second-term chief of staff, did not understand that the president was dependent upon staff aides to give him reasonable options from which to choose. Regan's effort to control the streams of advice thus served his boss poorly. A passive president needs more help than an active one. Sloan makes all this very clear. It followed that Reagan's staff assistants were fearful of the president doing things on his own.

This behavior would seem to be at odds with the prescriptions of presidential scholars that presidents must have detailed knowledge of their "power stakes" and use staff assistants to achieve "multiple advocacy." I am not so sure. Reagan may very well have known his power stakes without delving into policy substance, and he surely did balance pragmatists and ideologues always in his own political interests. But it is also the case that ignorance had costs. Supplyside economics claims to increase savings, but that did not happen. Economic recovery was achieved at the price of enormous budget deficits. The United States became a creditor nation in both trade and foreign investment. New jobs did grow but not real wages. Companies became leaner and meaner, with high salaries for executives and boons for stockholders but little for wage earners and their unions. Reagan showed other politicians how to deal rhetorically with economic and social inequality and thereby retarded our politics. Promise that markets will do it all and tacitly blame the victims for not grasping their bootstraps. Turn a deaf ear to poverty. The poor do not vote.

Reagan was an original whose conservatism was all his own. That is why the Republicans are so desperate to find a worthy replacement. That is not likely to happen. His abilities had developed over a lifetime. At the end of the day, I agree with John Sloan that Reagan was a successful and effective president but not a moral leader.

Desegregation in Boston and Buffalo: The Influence of Local Leaders. By Steven J. L. Taylor. Albany: State University of New York Press, 1998. 259p. \$54.95 cloth, \$19.95 paper.

Christine H. Rossell, Boston University

This craftsmanlike analysis, rich in detail, will serve as a resource for students of school desegregation and urban politics for decades to come. I have written extensively on school desegregation in Boston and, to a lesser extent, in Buffalo and was appointed by Judge Garrity in 1976 to monitor its implementation in Boston, but nevertheless I learned something new on virtually every page. I have not read a more detailed analysis of these events or its participants.

Taylor compares the characteristics of the two cities in order to explain the differences in community reaction to school desegregation. Boston erupted in violence with the implementation of a mandatory court-ordered reassignment plan in 1974 and a second plan, greatly expanded, in 1975. In Buffalo there was no violence when a voluntary desegregation plan was ordered by the court in 1976. The author's central argument is that the difference in the reaction is largely explained by the behavior and attitudes of local leaders.

Taylor describes in great detail the legacy of ethnic politics in the two cities, the politics of school desegregation, and the response of local leadership and parents. He convincingly argues that the presence of black school board and city council members in Buffalo caused whites on these bodies to behave responsibly, whereas their absence in Boston meant that white members were free to behave irresponsibly. Where

I disagree is with regard to the influence of these leaders on the community.

The Boston school desegregation plan was one of the most extensive mandatory reassignment plans in the North, not only in 1974 but also in 1975, when the deck was completely reshuffled, and in 1976, when it was modified again. The Buffalo plan was voluntary for whites during its first five years. My research indicates that the percentage of whites mandatorily reassigned in a school desegregation plan is the single most important factor explaining white flight, white protest, and the position of leaders, who are supportive of voluntary plans and opposed to mandatory plans. Thus, it seems to me that the influence of leadership in Boston and Buffalo on public response cannot be definitively determined, since the plans are so different.

Nevertheless, I learned a great deal from Taylor's analysis of local politics, and he convinced me that the presence of blacks on the Buffalo school board and city council influenced their white peers to be more moderate in their rhetoric. I would add, however, that their greatest influence was probably on the judge. The responsible behavior of the Buffalo officials during the liability hearings probably convinced the judge to allow them to implement a voluntary remedy. Of course, the experience of Boston also helped.

Overall, this book succeeds admirably at being objective and scholarly, but there could have been more appreciation of the tragedy of these events. A second and in some ways related criticism is that I object to Taylor's contention that school desegregation remedies had no grand design: "These informants seemed to believe that busing, like prohibition, was a social experiment designed to achieve a broad objective. This interpretation of the order is squarely at odds with the federal courts' reasoning behind ordering busing. The reason for busing is simply the enforcement of the Fourteenth Amendment as interpreted by the 1954 Brown decision" (p. 203).

Thus, Taylor rebuts his informants' complaints that school desegregation accomplished nothing with the assertion that the courts never intended to accomplish anything more than the mechanical dismantling of a de jure segregated school system. I do not think that is correct. The Boston Public Schools were not de jure segregated in the way that the South was. There were many integrated neighborhoods with integrated schools in 1973, the year before desegregation. Indeed, there was a smaller percentage of racially isolated schools and more interracial exposure in the Boston public schools in 1973 than there is today. Only nine years after desegregation and while still under court order, there was less interracial exposure than when the schools were convicted of de jure segregation. Taylor acknowledges this, but he asserts that integration was not the goal.

I disagree and think the Boston school desegregation plan has all the elements of a Greek tragedy. I was a member of the Citywide Coordinating Council, and I remember that we did have grand plans of social engineering, and we did want to increase integration. So did the judge. Indeed, to my mind the only thing that explains the northern court rulings, with their massive systemwide reassignments on the basis of the flimsiest of de jure evidence, is that these judges saw themselves as social engineers implementing a remedy that would accomplish integration not only in the schools but also ultimately in society. Sadly, however, we cannot determine whether it did.

The fact that Boston was not a true de jure segregated school system like the pre-Brown South also helps explain some of the reaction of the community and its leaders. Boston paid the highest price—massive, systemwide busing—

just as if it had had a system of total apartheid. Only two years later, Buffalo and Milwaukee were allowed to desegregate with a voluntary plan, although they had virtually the same level of racial imbalance in the schools (the index of dissimilarity ranged from .63 to .72 in these three districts, with Boston in the middle). I think these inconsistencies are important explanations of why the people of Boston and their leaders still feel a real sense of injustice and the citizens of Buffalo do not.

Taylor concludes that a major lesson is the futility of trying to seek the reversal of a decision when the violations are blatant. Except for the notion that it is necessary for the violation to be blatant, I could not agree more. But this only heightens my feeling that Taylor has done a wonderful job of documenting what in the end is a tragedy, or at the very least, an existentialist experience.

The Sound of Money: How Political Interests Get What They Want. By Darrell M. West and Burdett A. Loomis. New York: W.W. Norton, 1998. 220p. \$15.00 paper.

Diana Evans, Trinity College

For decades scholars have debated the degree of influence wielded by interest groups over public policymaking. E. E. Schattschneider's classic, The Semi Sovereign People (1960), has influenced generations of political scientists with the argument that representative democracy offers the masses the potential to get their way against large wealthy interests by expanding the scope of conflicts that initially advantaged those interests. At the same time, it was recognized that the interest group system itself favors the same moneyed interests. Building largely on Schattschneider's argument that such interests have inherent advantages in conflicts over public policy, Darrell West and Burdett Loomis challenge both his optimism that conflict expansion benefits the unorganized and the modern pluralist view that the proliferation of interest groups, including mass-based citizens' groups, over the past three decades has resulted in broadened representation of the unorganized public in policymaking. They acknowledge a time in the 1960s, 1970s, and 1980s when citizen groups and social movements scored significant policy successes. They argue, however, that large, well-organized, wealthy interests have learned to exploit modern information sources and communications techniques in ways that enable them to shove aside poorer, more broadly representative groups and dominate public debate over a wide range of issues. They challenge pluralist theory by arguing that changes in interest group influence techniques have undermined the traditional system of countervailing powers.

The book elucidates in Part 1 the theoretical argument and describes the techniques used by moneyed groups to influence policymaking. Part 2 consists of four case studies that illustrate the type of activism described in Part 1: Clinton's health care reform effort, the Contract with America, telecommunications policy reform, and incremental reforms of health care policy. Finally, the authors assess the broader effect on American democracy of the changes they observe and evaluate various reform proposals.

The central argument is that large, wealthy interests have learned from more broadly representative interests, such as citizen groups, the techniques of conflict expansion. They have used their superior resources to develop those techniques with the goal of controlling the definition of policy problems, the policy agenda, policy alternatives, and ultimately policy outcomes. In the modern advocacy world, wealthy interests employ such techniques as polling, focus

groups, and commissioned studies to create broadly persuasive, yet often misleading and manipulative, policy narratives that serve their purposes. Using the techniques of modern public relations, they communicate those narratives both to the public and to policymakers.

The methods of dissemination are as broad as they are expensive: Elite and mass advocacy campaigns and grassroots organizing are added to more traditional inside lobbying techniques and election campaign contributions. In communicating their narratives, these wealthy groups rely on frequent repetition to drive home their point to a public and to policymakers who otherwise might miss the message in this age of information overload, drowning out other voices through sheer persistence and ubiquity. These techniques are used openly by many groups; in addition, those that prefer for strategic reasons not to be connected publicly to a cause wage "stealth" campaigns, working under misleading and innocuous covers to conceal their association with an issue. West and Loomis argue that these efforts are aided by journalists who fail critically to examine issue advocacy campaigns that are unconnected to elections. The result is impoverished public discourse and, ultimately, given the centrality of interest groups in the governance of the United States, a threat to representative democracy itself.

Once we make allowances for the growing sophistication of the techniques used in developing and mounting issue advocacy campaigns, the obvious question is: How new is the concentration of influence among wealthy interests? If the cases examined here represent a new level of dominance of public policy debates by large interests in the 1990s, we should expect that, if those cases had occurred in the 1970s or 1980s, the broader public would have been more effectively represented by some of the groups now drowned out of the policy debate. Yet, if that were the case, would there not have been serious consideration if not enactment, given Republican dominance of the presidency, of some sort of national health care policy before the 1990s, when public interest groups presumably were stronger?

More to the point, do the policy outcomes that West and Loomis describe pose a genuine challenge to existing theory? They note correctly that issues of high visibility normally are expected to reduce the influence of moneyed interests. They argue, however, that high issue visibility today actually enhances the influence of large moneyed groups, as the fate of Clinton's health care reform proposal illustrates (p. 106). Alternatively, it can be argued that in that case, government played its traditional role of taking up the public's concern with health care, socializing the issue through Clinton's reform proposal. Had it not, the private interests threatened by the proposal would not have needed to expand the conflict to try to shift the balance of forces to their side. That they were able to influence public opinion through often misleading ads supports the authors' argument.

It is also the case that the president's proposal suffered from his own bad timing, as West and Loomis themselves argue: Labor and consumer organizations that normally would have supported health care devoted their resources to fighting NAFTA, which the president chose to push at the same time (p. 80). Thus, the influence of antireform interests on a reform-minded public can be explained as well by the president's priorities. Those priorities prevented a corresponding and countervailing escalation of the conflict by more broadly representative interest groups; if it had occurred, it may well have produced a classical pluralist outcome closer to the public's desire for expanded health care availability. Admittedly, those broadly based groups were forced to choose between two important conflicts, while the

private sector, with its wider range of organizations and interests and greater wealth, was able to mobilize both for NAFTA and against health care reform.

The fact that the health care case arguably conforms to conventional interest group theory as well as to West and Loomis's own argument does not negate the value of the authors' challenge to the dominant way of thinking about interest group influence. Through a number of carefully analyzed examples, West and Loomis make a plausible case that we have reached a point at which the concentration of wealth and the development of sophisticated techniques of opinion manipulation and lobbying have combined to concentrate even more influence in the hands of those who already had the lion's share of it. In the concluding section of the book they advocate several reforms, including expanded disclosure requirements that would cover issue advocacy and outside lobbying campaigns, as well as a proposal, which is

somewhat less realistic, to enact truth and accuracy provisions for mass advocacy communications. Yet, because the processes that led to the increased sophistication of public advocacy campaigns arose from the wealth of certain kinds of groups, none of the potential reforms offers great promise for reversing the phenomena described in this book.

The Sound of Money is a book with a point of view, lively and engaging. It will be stimulating for students and scholars alike. Although its theoretical basis is familiar, its challenge to pluralist theory is intriguing. The case studies that buttress the argument are well documented and of interest in themselves. Some readers may disagree with some of the authors' interpretations of recent public policy battles, but West and Loomis make a vigorous case for their analysis of changes in the interest system. Finally, they offer a warning that should be taken seriously by all who are concerned about the effect on democracy of the unequal distribution of resources in the interest group system.

Comparative Politics

Legislative Politics in the Arab World: The Resurgence of Democratic Institutions. By Abdo Baaklimi, Guilain Denoeux, and Robert Springborg. Boulder, CO: Lynne Rienner, 1999. 278p. \$59.95 cloth, \$22.50 paper.

Mehran Kamrava, California State University, Northridge

Amid the growing literature on the prospects and preconditions for democracy in the Middle East, this is a welcome addition that adds considerably to our knowledge of the institutional dynamics at work in the transition to democratic rule in the Arab world. The authors correctly maintain that the literature on political liberalization in the Middle East has largely ignored the pivotal role of institutions in general and of legislatures in particular. In fact, much of the recent work on the topic concentrates on the economic imperatives of political liberalization or the compatibility, or lack thereof, of religious and other cultural forces with democracy. Legislative Politics in the Arab World, however, sets three primary objectives: (1) Demonstrate the analytical utility of concentrating on legislatures in better understanding processes of democratization in the Arab world; (2) highlight the many contributions of legislatures to the processes of democratic transition currently under way; and (3) provide information on key legislative developments in the six countries on which the book focuses: Lebanon, Morocco, Jordan, Kuwait, Yemen, and Egypt (p. 6). The book meets these objectives with unqualified success.

The book is especially important in two regards. Part 1, which provides a framework for the analysis of Arab legislatures, contains one of the most rigorously constructed theoretical treatments of the prospects for and obstacles to institutionally based transitions to democracy in the Arab world. Such transitions, whose less rapid pace in comparison to those in Latin America and Eastern Europe has attracted considerably less attention, have been taking place in three general phases: the drafting of a national pact, initiated at the behest of state actors themselves; the initiation of a national dialogue, designed by incumbent elites to enhance their declining legitimacy or that of their unpopular policies; and the assertion of the legislature's authority (pp. 33–43).

A second contribution, in Part 2, is the wealth of empirical data on the various aspects of the legislatures in each of the six countries. Such details of legislative inner workings—the historical evolution of national parliaments, assessment of internal capacities and weaknesses, the constitutional responsibilities of each chamber, important legislative initiatives, and more—are usually found in relation to European political systems. In this important respect, the book provides invaluable and in many instances original data. The legislatures of Morocco, Yemen, and to a lesser extent Kuwait have been especially neglected, and the book considerably enhances our knowledge of the institutional evolution of these and other Arab states in general and their legislatures in particular.

There is, however, no mention of the research methods employed in acquiring the rich empirical details. It appears that the authors relied on a combination of interviews with Arab parliamentarians, field research and observations, and the available (overwhelmingly English) literature. If this is indeed the case, it should have been spelled out, preferably in the acknowledgments or the introduction. It is unclear, for instance, how the data on the distribution of parliamentary seats in Lebanon's various elections were obtained (pp. 87, 88, 97, and 100). Other insights, for example, on Yemen's little studied parliament (pp. 214–6), also leave the reader hoping for an indication of how the information was acquired.

There is a larger problem. The authors repeatedly assert that in one form or another parliaments are playing a most central role in the democratic transition in the Arab world, the assumption being that such a process is indeed under way. But on a number of occasions the authors reveal their own discomfort with this assumption. "Instead of manifesting itself through the downfall of existing regimes," they assert at one point, for example, "change has taken the form of attempts at relegitimizing the system through elections and the incorporation of new groups into the institutions of the state" (p. 31). This trend, which indeed has been taking place throughout the Arab world, does not indicate a meaningful rearticulation of existing (i.e., nondemocratic) patron-client relations between state and social actors. It points instead to modifications within existing patterns of state rule amid mounting domestic difficulties. As the authors' own analysis shows, in fact, both Egypt and Yemen have moved farther away from democratization in recent years than was once believed. Even in discussing the three countries in which

parliaments are more active than ever before—Jordan, Morocco, and Kuwait—the authors do not convincingly show that the legislatures are any more than safety valves designed to deflect criticism from monarchs who have changed their modus operandi only slightly. The authors themselves concede that only in Lebanon does the legislature play a meaningful role in political life, but "even there, Syria's overwhelming influence constrains very significantly what parliament can and cannot do" (p. 42).

The inconsistency in the assessment of on-going democratization in the Arab world stems largely from the sole focus on the institutional forces at work in Arab politics. From the outset, the authors state their intention to examine the contributions that Arab parliaments have made to widening the scope of the political arena and, over time, legitimating new rules for the political game. But exclusive focus on the reviving of the legislature runs the risk of overlooking equally important social and cultural developments in concurrence with which institutional changes take place. Although the authors did not set out to examine all the imperatives of Arab democratization, they could have paid closer attention to the larger sociocultural contexts within which institutional changes are unfolding. In Morocco, for example, there is controversy over the live broadcast of parliamentary debates (pp. 118-9), but how widely watched are these debates? The answer would reveal a great deal about the level of popular skepticism or legitimation regarding the legislature as a viable forum for political representation or, alternatively, as yet another appendage to an otherwise authoritarian system. Repeated reminders that voter turnout is consistently low—in Jordan, only 44% in the 1997 elections (pp. 165–6) also warrant more in-depth analysis of the perceptions of social actors toward legislative bodies.

These shortcomings notwithstanding, Legislative Politics in the Arab World is a book of profound importance, both for its theoretical contribution to the institutional dynamics of democratization and for the risk empirical data it presents on the legislatures of Lebanon, Morocco, Jordan, Kuwait, Yemen, and Egypt.

Party Discipline and Parliamentary Government. Edited by Shaun Bowler, David M. Farrell, and Richard S. Katz. Columbus: Ohio State University Press, 1999. 304p. \$59.95 cloth, \$22.50 paper.

Paul V. Warwick, Simon Fraser University

Parliamentary systems are typically investigated under the premise that parties are so disciplined that they, rather than the individual legislators, can be taken as the relevant parliamentary actors for most intents and purposes. In recent years, however, this assumption has come under increasing scrutiny, stimulated in part by the emergence of noticeable disciplinary lapses in certain parties, such as the British Conservatives, that once epitomized the trait. Attempts to account for such lapses naturally raise the question of why we should expect it to be otherwise; why, in other words, should elected representatives give up their right to speak and vote freely, a phenomenon so at odds with the American congressional experience?

This collection of essays emanates from a workshop on the issue held as part of the 1995 Joint Sessions of the European Consortium for Political Research (EJPR). It is common in collected volumes that the individual contributions are uneven in import and divergent in approach; with EJPR workshop collections, these properties are especially likely because there is typically no organizing criterion beyond the

topic itself. So it is here. The collection is bracketed by two interesting theoretical pieces based on fundamentally opposed premises, and they enfold a number of country or party-level studies that rarely make reference to either of them. What we have, then, is not so much an attempt to bring coherence to the topic of party discipline as an attempt to open it up.

How well does it do this? At the theoretical level, rather well indeed. The most stimulating piece is undoubtedly Laver and Shepsle's "How Parties Emerged from the Primeval Slime." This contribution represents an extension to intraparty politics of their well-known and controversial "portfolio allocation" model of government formation and survival. The model's key departure, and the source of the controversy surrounding it, is the assumption that ministers in coalition cabinets are free to impose their own policies in the portfolios they are allocated; for Laver and Shepsle, reconciling policy differences among coalition partners is not in the theoretical cards. In this extension of the model, the assumption is made somewhat more palatable by pointing out that parties may have an advantage in maintaining a stable of ministrables (potential ministers) with slightly different policy biases. This would make it possible for them to offer potential coalition partners a range of different policies (albeit a relatively small one) for any portfolio; more to the point, it may enhance the chances that an equilibrium coalition government can be forged. If this observation points to intraparty diversity, Laver and Shepsle also demonstrate that their model might facilitate party discipline by allowing one faction to elicit total submission from the other factions if it alone represents a policy stance that figures in an attractive equilibrium outcome.

The other theoretical book-end is Mitchell's "Coalition Discipline, Enforcement Mechanisms, and Intra-party Politics." Mitchell's approach is fundamentally opposed to that of Laver and Shepsle: It assumes that coalition partners must reconcile their policy differences. The implications for party discipline are serious because party leaders must sell the compromises they accept in cabinet to backbenchers (noncabinet members), who do not receive the office "perks" that can make them so much more digestible. The detailed investigation of two Irish coalition governments shows that a large number of the destabilizing threats they faced consisted of backbencher revolts against the concessions given up by party leaders for purposes of coalition preservation. These challenges typically did not reach the level of voting dissension, not because discipline was imposed by the leadership but because the leadership caved in. The lesson: There may be a good deal more intraparty disunity than the highly disciplined voting in West European parliaments suggests.

After reading these two pieces, one cannot help but wish that the remaining empirical fare were more oriented toward testing them out. As noted earlier, this was not the purpose of the workshop. Instead, the empirical contributions tend to go their own ways: two on dissent in the British Conservative Party, four on party discipline in other European parliamentary systems, and two on the state of affairs in the European Parliament. Some have interesting observations of their own. Sánchez de Dios, for example, points out that discipline in the relatively new Spanish parliamentary system emanates from the remarkable array of institutional powers that party leaders wield over backbenchers. Raunio observes that the sequence in which legislation is voted on in Norway allows leaders to tolerate voting dissent at earlier stages in the consideration of a bill in the knowledge that it will not affect the final outcome; in this sense, party discipline in Norway may actually be higher than the roll-call voting suggests.

In other cases, the attempt to say something new leaves previous stones unturned. Mershon's investigation of the coexistence of high cabinet instability with low coalition turnover in Italy, for instance, ignores the tradition of French literature on the topic. More significant is that, in placing the blame for Italian cabinet instability on the low office and electoral costs of defecting from a coalition, she does not mention that these low costs are implications of Sartori's conceptualization of polarized pluralism. This need not mean that Sartori is right—perhaps it is time for a new explanation—but his framework surely ought to be considered.

In general, the reader should not look to this collection to resolve the puzzle for party discipline but to broaden an appreciation for its many facets. What is clear is that numerous factors may encourage or undermine party discipline in parliamentary regimes; finding out which ones are operative and under what circumstances will not be easy. There is a subterranean side to the party discipline we normally take for granted in parliamentary politics, and much of it remains to be mapped out.

Reinventing Russia: Russian Nationalism and the Soviet State, 1953-1991. By Yitzhak M. Brudny. Cambridge, MA: Harvard University Press, 1998. 352p. \$45.00.

Lowell Barrington, Marquette University

Nationalism is one of the most important topics in both international and comparative politics. One cannot understand the collapse of the Soviet Union without understanding this topic. Yet, the discussion of nationalism during the Soviet period generally centers on the strength of nationalism during the Gorbachev reform era and on the nationalism of the non-Russians. Yitzhak Brudny's excellent book instead focuses on the entire post-Stalin period and on Russian nationalism itself.

Brudny begins with a concise overview of the topic of nationalism and a thorough discussion of its Russian variants. He effectively distinguishes between Russian and "official Soviet" nationalism. Russian nationalism is especially difficult to understand because Russian nationalists themselves have difficulty answering the central questions: Who belongs to the nation? What territory should the nation control? Brudny offers his own typology of Russian nationalism (liberal, conservative, radical), describes the policy positions of the three camps, and links this to a discussion of post-Soviet nationalism in Russia, a theme to which he returns in an all-too-brief epilogue. In a useful table (p. 10), he also lays out the major publications associated with the three nationalist movements both during and prior to perestroika. This table gives an introduction to the periodicals and movements that dominate the discussion later, although one of the few editorial problems in the book is that some of the publication titles are either misspelled or incorrectly capitalized throughout the table.

The rest of the chapters provide a tour, chronological and thematic, of the evolution of post-Stalin Russian nationalism. There are individual chapters on 1953–64 and 1965–70, but Brudny uses three chapters to cover 1971 to 1985. The last two chapters before the epilogue then return to the chronological approach, covering 1985–89 and 1989–91, respectively. This mixing of the thematic and chronological leads to some repetition among chapters, although that is arguably hard to avoid in a book spanning several decades and numerous subjects.

The main argument is that the roots of Russian nationalism in its various forms today can be found in the tolerance and even encouragement of it by Khrushchev and Brezhnev, especially in the "village prose" movement. Particularly under Brezhnev's "inclusionary politics" approach, Russian nationalists were wooed, ultimately unsuccessfully, by the Soviet leadership. Through detailed discussions of publications such as Molodaya gvardiya, Nash sovremennik, and Novy mir, Brudiny points out how these Russian nationalists created problems for the Soviet leaders—both by stressing the value of the village and the Orthodox Church and by criticizing social ills, such as alcoholism and the steady destruction of the environment.

The problem of alcoholism in Soviet and post-Soviet Russia is no secret to even casual observers, but the relationship between Russian nationalists and the efforts by Soviet leaders to confront the problem is less familiar. Brudny points out that the explosion of alcohol consumption in the 1970s resulted in a series of articles, letters, and editorials in Nash sovremennik describing the problem and criticizing government policies for contributing to it. At the same time, the publication carried numerous essays critical of the spread of "urban culture" to the countryside and the resulting demographic changes among ethnic Russians, focusing on their declining birth rate. Thus, by the time Western scholars were beginning to identify the emergence of economic and social "stagnation" in the Soviet Union under Brezhnev, Russian nationalist writers had already been openly discussing for years numerous social ills throughout the country and in the Russian Republic in particular.

The discussion of the link, as early as the 1950s, between the Russian nationalist movement and the topic of the environment is a particularly interesting aspect of Brudny's work. Again, scholars interested in the subject have tended to examine non-Russian movements under Gorbachev. Brudny convincingly argues that a movement existed in Russia, and well before Gorbachev's rise to power. Russian nationalists were critical both of specific problems or policies (pollution in Lake Balkal, plans to divert Siberian rivers, and so on) and of the general trend in the Soviet Union toward a more urban, industrial society. The importance of ecology to the village prose movement grew significantly during the 1960s and 1970s, and Brudny argues it had become the dominant topic. Essays and articles routinely contained striking depictions of the increase in pollution of the air, water, and land as a result of Soviet agricultural and industrial policies. On the eve of the Gorbachev era, nationalist hostility to the planned river diversion project was impossible to ignore. By 1986, the concept had been scrapped, handing Russian nationalists one of their most significant policy victories of the Soviet period.

Anyone interested in either the Soviet Union or the topic of nationalism would benefit from reading this book. It covers important Russian nationalist events of the Gorbachev years, such as the publication of the famous Nina Andreeva letter, but it shatters any illusions that nationalists were a tool of the Soviet state before Gorbachev and the assumption that they only got out of control during the glasnost period. Brudny demonstrates that nationalists were already out of control—reined in only by periodic government action, such as the censorship of their attacks on government policy in the nonblack-earth region villages and the dismissal of the Molodaya gvardiya editor. Brudny also convincingly illustrates that nationalists were already wedded to such glasnost-era hot issues as the environment long before the Gorbachev reforms.

Other than the repetition across chapters, the only real weakness of the book is what is not covered. Because of the chosen period, there is only a very limited discussion of Russian nationalism before Stalin (not to mention nationalism under the tsarist system). More on the link between Stalinism and post-Stalin "radical nationalism" would be

useful. Likewise, Russian nationalism in the post-Soviet period is covered just enough to leave the reader wanting more. But these are small complaints. In writing history, one has to draw lines somewhere. Brudny has elected to focus on 1953 to 1991, and he has certainly discussed the topic of Russian nationalism during that period in a highly effective manner. Even with its limited coverage of the Lenin and Stalin years, this is simply the most thorough and thought-provoking work available today on Russian nationalism during the Soviet period. It has important implications for our understanding of the Soviet past, and it provides valuable guidance for those seeking to understand the present and future of nationalism in post-Soviet Russia.

The War in Bosnia-Herzegovina: Ethnic Conflict and International Intervention. By Steven L. Burg and Paul S. Shoup. Armonk, NY: M.E. Sharpe, 1999. 520p. \$49.95.

Francine Friedman, Ball State University

The Bosnian war is over for all intents and purposes. The implementation forces for the Dayton Peace Accords are imposing democratic forms where possible in an attempt to force the national groups in the area to live together in a somewhat civil society. At the time of this writing, NATO planes are attempting to relax the murderous grip of Slobodan Milošević on Kosovo. Perhaps now is a good time for reflection on how all this mayhem came to southeastern Europe. Steven Burg and Paul Shoup have produced a long and extraordinarily detailed study of the events leading up to the Bosnian war as well as the internal and external situation throughout the war. More than that, they have attempted to analyze the motivations and actions of the warring parties themselves, both with regard to each other as well as to the regional and international actors that inevitably became involved in the prosecution and solution of the conflict.

The authors divide the volume into two halves. The first part deals with conditions in prewar Bosnia and Herzegovina, the origins of the war, and the major developments in the conflict through 1994. The second half details the involvement of the international community in the effort to end the conflict as well as the lessons that may be learned from the war itself and the way it was approached by the international community and regional actors. The authors, however, intertwine the parts with frequent references to preceding or succeeding points, so that one is treated to more of a seamless whole than a divided commentary.

Burg and Shoup do an excellent job of reminding the reader that the Bosnian conflict was a most complex and bewildering situation. As they point out in the first chapter, an understanding of the conflict would be narrow if one only embraced "a narrowly ethnic definition" (p. 4). Yet, one is left with the impression that the economic and moral issues that they insist also informed the conflict might have been overshadowed by the ethnic, and especially the political, aspects of the controversy. One strength of the book is that the authors are able to show how the political exigencies of the various actors, both domestic and international, drove their actions.

Another particularly fine feature of this volume is the numerous maps provided in the chapters on the various plans put forward to resolve the Bosnian war. The bewildering array of proposals and demands is more easily understandable with these well-done visual aids.

All in all, this is a quite remarkable and valuable book. The authors have obviously attempted to be as even-handed as possible in presenting the points of view of the various sides,

although their sympathetic explanations of the attitudes and demands of the various Serbian actors may be surprising to a public that has not been exposed to a very sympathetic view of anything Serbian, even of the Serbian victims. Their allusions to hard-headed Bosnian Muslim calculations and actions during the war and the search for peace are similarly rarely dealt with in the public or even academic press. This is a part of the conflict that needed to be examined.

Nevertheless, a few questions remain. The authors imply that a "nascent nationalism" was emerging in late 1993 among the Bosnian Muslims (p. 283). The reader might benefit from a closer definition of the meaning of that remark in light of earlier as well as later political circumstances. That is, do the authors mean to imply that the Bosnian Muslims until this time had publicly expressed little or no national feeling? If that is the case, then the 1991 census in which almost two million Bosnians identified themselves as Muslims (in the national sense) is puzzling ("The National Composition of Yugoslavia's Population, 1991," Yugoslav Survey 33 [1992]: 4-5). One also wonders how that nationalism is being expressed in Bosnia today. While the SDAdominated government is obviously pursuing a chauvinistic policy, expressed by the name Bošnjak that it would like all Muslim citizens to use, anecdotal evidence suggests that many Bosnian Muslims, especially many of the youth, prefer to use a regional identity, expressed in the concept of Bosanac/Bosanka. A thorough discussion of this thorny issue is probably beyond the scope of this book, but more explanation of the nationalistic tendencies of the Bosnian Muslim population would be welcome.

It is interesting that the authors challenge what appears to be an intent by the international actors to remain in Bosnia for a long time: "The international military force deployed to support" the Dayton Peace Accords does not promise "to be long-lasting" (p. 318). Both OHR and OSCE representatives publicly and privately vow that SFOR or some such institution will remain in the area for many years (e.g., Carlos Westendorp, "Don't Bargain with Bosnia," Wall Street Journal, April 6, 1999). Fifty years is the most frequent forecast, with the expectation that one or two generations of Bosnians must be reared under a Western-imposed democracy before the West deems it safe to leave. The authors' point of view is interesting, as would be an explanation for its basis.

These minor points aside, Burg and Shoup have provided the most definitive volume to date on the Bosnian war. They have meticulously documented the opposing perceptions and goals of the combatants and the external actors and have placed them within the context of the events of the time. One must read this volume to understand how the Bosnian war occurred, was prosecuted, and ended.

Restructuring Political Power in China: Alliances and Opposition, 1978-1998. By An Chen. Boulder, CO: Lynne Rienner, 1999. 279p. \$59.95.

June Teufel Dreyer, University of Miami, Florida

This is an important book. The author's central argument is that, since the 1970s, political reform in the People's Republic of China (PRC) has been determined by the interaction of three major forces: the central elite, local party cadres, and the subordinate population; the latter generally are termed "the broad masses" in Chinese communist parlance. In An Chen's analysis, these interactions, engendered by the reforms that began in the late 1970s, have created the basis for the PRC's evolution toward democracy.

This thesis can be stated simply, but the research on which

it is based is complex and multifaceted. The author has skillfully woven together Western social science concepts of political reform with a wide range of Chinese sources and his own interviews. The restructuring, he finds, has taken place in two stages: 1978 through 1986 and 1986 to the present. The first stage was characterized by pragmatic considerations. Deng Xiaoping exploited mass dissatisfaction with his predecessor, Mao Zedong's atrocities, and promised drastic changes. An Chen opines that, with the PRC on the brink of social upheaval, Deng's priority had to be the restoration of order and stability. Yet, he could not simply restore the party-state institutions of the 1950s, with their quintessentially Maoist combination of political indoctrination, organized terror, and legitimacy conferred by winning the civil war in 1949. In the interim, citizens had become more intractable and disobedient, and the revolution had lost its elan vitale.

Deng's reforms were primarily economic: privatization of agriculture, large-scale opening to foreign investment, and somewhat tentative measures to increase the autonomy of enterprises and encourage the urban private economy. The underlying structure of political power remained essentially the same. By the mid-1980s, Deng and his advisers realized that economic reforms were being thwarted, since they depended on transforming a structure in which local party cadres had a vital stake. This was not simply a matter of individual prerogatives being harmed, but impinged on the entire party-state apparatus. Massive official corruption had also become a problem and was greatly resented by the citizenry. While a drastic shake-up was in theory possible, there was a serious possibility that such a move would destroy social stability.

Stage two began when the thirteenth party congress of 1987 took a different approach: major political reform. The two key institutional changes were the separation of the party from government and greater authority for the legislature, the National People's Congress. The general citizen's indifference to party-government activities of the early 1980s was gradually replaced by a fresh political awareness and concern with protection of civil rights. Elections of deputies became hotly contested, and more strident voices in the legislature could affect the leadership's policy choices. Deng showed impressive manipulative abilities, playing the "citizen card" (i.e., the legislature as the "highest organ of state power") and the "party [committee] leadership card" through a strategy of alignment, dealignment, and realignment among the three major relevant forces; the central leadership, local and grassroots cadres, and ordinary people.

Leaders in Beijing attempted to work out a certain balance of power at local levels between the party committee and the people's congress, poised to shift to the opposite group if one side became despotic, arbitrary, or seemed to be slipping from the center's control. Although the leadership's efforts to develop social support through reforms achieved some success, Deng recognized, especially after the Tiananmen demonstrations of 1989, that party cadres remained the mainstay of the communist regime. Hence, substantial power resources had to be left in the hands of party cadres, who were selected by higher levels and kept out of the reach of the masses. The organizational guarantee of party leadership was needed in order to prevent mass participation from exceeding the political and ideological limits determined by the ruling elite. Anything more, they argued, would plunge China into political chaos. A cynic might interpret this argument as a thinly veiled rationale for protecting their own privileged positions.

Deng's solution had the unintended effect of making political corruption difficult to control. What emerged from political reform was not a true market but a welter of networks protected by cadres and bureaucrats and acting in secret. The press was not free to publicize corruption scandals, since this would tarnish the party's image. Anonymous letters of complaint could be written to party committees, governments, judicial organs, and congresses to demand an investigation. But, as An Chen points out, it is important to notice who undertook the task. In general, people's congresses and deputies were excluded from investigations of not only party cadres but also government officials. Even judicial officials asked party committees for instruction, especially if senior officials were involved. In short, impartial investigations were rare.

An Chen argues that, these deficiencies notwithstanding, the past two decades have provided the masses with a training ground for democracy. Citizens became familiar with representation procedures, albeit not yet genuinely democratic procedures, and learned how to use legal rights to protect themselves. The signs of decline in party influence were visible as early as 1989, when party organizations and cadres in many regions were unable to dissuade citizens from taking to the streets.

Perhaps An Chen could have addressed the question of whether the decline in party influence and unbridled corruption could have resulted in some other future for China than democracy, possibly the "thugocracy" experienced by parts of the former Soviet empire or a resurgence of regionalism and localism. But An Chen cannot be accused of starry-eyed naiveté and is aware of the rise of both territorial and class politics. This is not an easy book to read; there is nothing wrong with the author's prose, but the topic is so multifaceted that any competent analysis must be as well. Those who choose to essay it will learn much; this is quite simply an excellent book.

The Basques, the Catalans, and Spain: Alternative Routes to Nationalist Mobilization. By Daniele Conversi. Reno: University of Nevada Press, 1997. 312p. \$44.95.

Cyrus Ernesto Zirakzadeh, University of Connecticut

Daniele Conversi has written an intricate, interesting book about the emergence and development of nationalist politics in two areas within Spain: Catalonia and Euskadi (or the Spanish Basque country). The first parts of the study focus on the economic, political, and cultural histories of both regions since the mid-nineteenth century. In the second half, Conversi develops a complex, general theory about why nationalist movements sometimes contain groups that engage in political violence.

In the historical chapters, Conversi juxtaposes Catalan and Basque responses to economic development. Nineteenth-century industrialization posed myriad problems for small merchants, craftspersons, and urban professionals. The often disgruntled members of the urban middle class provided most of the leaders and followers of the nationalist movements.

Although both the Catalan and Basque movements arose in Spain at roughly the same time, their social settings differed. Catalonia, Conversi reports, had been commercially developed long before the late nineteenth century. It also had repeatedly experienced waves of immigration. As a result, Catalan nationalists for the most part viewed all residents, including recent immigrants, as potential contributors to a commercially and politically vibrant Catalan nation. Basque nationalism arose in a very different context. Most parts of Euskadi were precapitalist before the late nineteenth cen-

tury, and residents had little previous experience with large numbers of immigrants from other parts of Spain. Basque nationalism, consequently, tended to be considerably more anticapitalist, antiindustrial, religious, and xenophobic than Catalan nationalism.

Conversi is not a simple structuralist in his historical thinking. He is sensitive to the role of human agency, particularly in the construction of symbols and ideologies. The ideological founders of Basque nationalism, according to him, never successfully synthesized the region's disparate worries about the ramifications of industrialization into a coherent doctrine. The Basque Nationalist Party often collapsed into warring factions and splinter groups because there was no positive program or symbol around which all nationalists could rally. There was no "core value," to use one of Conversi's key concepts.

Conversi argues in the second and more theoretical half of the book that the absence of a stable nationalist core value explains the emergence and persistence of political violence in the Basque region during the 1960s, 1970s, and early 1980s. Nationalists, needing to find common ground, agreed temporarily that expressions of daring self-assertion would become the litmus test of one's nationalist convictions. The preoccupation with confrontational action predisposed many (especially younger adults) either to engage in armed action or to support friends and neighbors who did. Conversi acknowledges that other circumstances most notably the extraordinary amount of state repression in the Basque region during the Franco regime—reinforced the general predisposition toward armed action. But repression alone, Conversi insists, cannot provide a complete explanation for nationalist violence during the 1960s, 1970s, and early 1980s. There also had to be a particular type of shared core value.

Conversi's general point—that the presence or absence of particular core values can affect the day-to-day activities of a nationalist movement—intuitively makes sense. After all, articulated programs and evocative images make certain types of political action possible. Conversely, the absence of programs and images makes certain types of action inconceivable. People are mobilized, in part, by visions and values.

The author's general argument about culture, however, also raises some theoretical questions that social scientists might want to discuss. Some of these puzzles have to do with his assumption that a durable political movement requires members who are nearly uniform or homogeneous in their beliefs and assessments (pp. 164, 199, 243, 263, 268). Is this true? How much consensus on core values does a nationalist movement or party (or any movement or party, for that matter) have to attain before the movement or party becomes stable? Can a successful and durable movement (or party) be an alliance or partnership among people with different values? If political alliances among disparate interests can occur, why was the pressure for ideological innovation greater for nationalist leaders in the Basque region than for those in Catalonia?

A second set of empirical and theoretical puzzles arises from Conversi's generalizations about the unusually strong appeal of an activist ethic among younger Basque nationalists (pp. 148, 203–4, 226, 238, 251). First, was "activism," in fact, valued more intensely there than in Catalonia (or, for that matter, in other parts of Europe)? Second, in terms of the origin of the new core value, was the popularity of activism due primarily to the absence of unifying core values among Basque nationalists (pp. 149, 222, 226, 238–9, 243, 255, 263)? Might the fascination with activism have been a reflection of broader, Europeanwide cultural trends?

Finally, several methodological questions arise about the

alleged causal connection between activism as a core value and violent political behavior (pp. 102, 203–5, 234–5, 251–6). Is there an established body of empirical evidence showing that people who deeply value aggressive action also have a propensity to violate the rights and lives of others? Cannot activism also lead to nonviolent behavior, particularly once liberal-democratic options are available? What does the literature on political behavior suggest about the social and psychological factors that intervene between core values and courses of action? Has Conversi, in his construction of a causal model, tried to control for such variables?

The above sets of questions suggest the theoretical boldness of this monograph. Conversi has written a scholarly book, in the best sense. In the historical section, he cites an extraordinarily broad range of sources. He is scrupulously fair in handling both scholarship that corroborates his views and scholarship that challenges them. The result of his labor is a dependable, readable, and detailed synthesis of recent scholarly work that probably should be on the shelf of every serious student of Basque and Catalan nationalism. Conversi's own ideas, especially in the second half of his book, are novel, eclectic, and provocative. His theories about core values and about their behavioral consequences diverge noticeably from mainstream social science theorizing about political culture, political violence, and movement integration. This book, as a result, compels us to look at current political events in new ways. Regardless of whether one agrees with the author's theoretical points, Conversi says a lot of interesting things.

Democratic Politics in Latin America and the Caribbean. By Jorge I. Domínguez. Baltimore, MD: Johns Hopkins University Press, 1998. 253p. \$59.95 cloth, \$19.95 paper.

Scott Morgenstern, Duke University

This is a compilation of insightful chapters penned by Domínguez for various other volumes in the last several years. Each deals with a different aspect of democratization in Latin America, and each provides a wealth of interesting hypotheses to lead the reader to the books from which they were drawn.

Domínguez attempts to generalize about the disparate chapters by focusing on two factors that he claims are underdeveloped in the democratization literature. The first is the role of individual leaders in navigating a country through the democratization and consolidation processes. His evidence for this thesis is a chapter on "technopols," which focuses on the biographies of five political leaders in four countries. He hypothesizes that their successes in the economic realm came from a deep understanding of the political process. The chapter offers interesting descriptions of the ideas and policies that were implemented under the watch of these leaders, but it neglects to explain how they gained their influence or the weight of their influence relative to others. For example, Domínguez cites Cardoso's (when finance minister of Brazil) use of Congress to pass economic reforms and Argentine Economy Minister Cavallo's concern with politics as key examples of technopols' grasp of politics and economics. He fails to explain, however, the role of the president or the degree to which the Congress modified proposals. Furthermore, it is difficult to gauge the importance of this political know-how to the policy process or the deepening of democracy, since important parts of these programs were implemented by presidential decree. The chapter fails to provide a full development of the theory or details about case selection or hypothesis testing, but it is an interesting introduction to this aspect of policymaking in posttransition Latin America. This and other chapters may lead readers not only to the volumes from which they were drawn but also to new research directions.

The international system is the second factor that Domínguez uses to tie the volume together. His short but interesting conclusion suggests that international factors have had a variable role in opening political regimes around the globe but have had very consistent and positive effects on democratic consolidation. The varying influence on democratic opening is clear in the contrast between military intervention that led to political openings in Panama, Haiti, the Dominican Republic, and Grenada, on the one hand, and the lack of centrally important international factors that led to the opening in Brazil, Chile, or Uruguay, on the other. He also cites the end of the Cold War, which had hardened the Cuban regime and Central American dictators as a positive international force.

Here again, however, the purpose of the discussion seems more to spur further research than to develop the theory. Advancing this promising thesis would require a careful look at the change in international policies and environment. In the chapter on the Caribbean Domínguez offers several examples of the international community intervening to shore up weak democracies or fend off democratic opponents. Without this causal argument, his discussion of the consistent effect of the international system on consolidation seems more a by-product of outcomes than a careful look at specific policies. This is an important limitation, especially in light of how the international system has challenged the young democracies by pushing often harsh economic reforms on them. In the Caribbean, for example, Domínguez focuses on the "statist bargain" in which pork, fed by parties to voters, generated loyalty to a democratic system. The fall of international commodity prices, therefore, adversely affected the democracy. Democracy did not fail in the Caribbean, however, and thus Domínguez attributes more weight to the positive international support for democracy than the adverse consequences of the international economy. Development of the thesis about how the international community supports democracy, which may rely on the contingency of international loans to complete the restructuring, remains for future testing.

The other chapters touch on these two central themes but focus on other issues. Chapter 1 examines why the Caribbean nations, despite long odds, have built and sustained democracy. The effect of the international environment enters through Domínguez's discussion of how British colonialism generated a set of political institutions propitious for democracy. As noted above, he also cites several cases of international intervention when democracy was threatened. Chapter 2 focuses on democratic transitions in Central America, noting not only the role of international pressures but also the importance of economic distress and the hardship of long civil wars to explain political liberalization. Chapter 3 takes a similar course; entitled "Constructing Democracies," it argues that in addition to the international environment, political institutions, economic reforms, and the shape of the military determine democratic success.

After the chapter on technopols, which, as noted, discusses the importance of leadership, chapter 6 looks at the electorate, in particular the changed values of Mexicans since the surveys by Almond and Verba in the 1950s. It describes the newly prized democratic values and implies that once the last vestiges of authoritarianism are cast off the change will help Mexico sustain democracy. The final case-specific chapter focuses on possible directions for Cuba. Domínguez sets up

several scenarios, including a continued political monopoly with more market openings, a more repressive regime (in response to possible uprisings), and a regime transition. Leaders and the international climate are certainly central to all these. The point of the discussion about a transition scenario, however, is to explain the role of cultural and historical traditions. These, it is argued, may lead Cubans to support a strong leader and an interventionist state, a combination that may have negative consequences for democracy and economic development.

In sum, like other collections, this book seeks a theory to generalize about loosely connected chapters, rather than propose a theory and then seek evidence to support it. Still, the new introduction and conclusion, combined with the previously published work, offer a useful survey of issues and an interesting group of hypotheses for further study.

Coalition Government, Subnational Style: Multiparty Politics in Europe's Regional Parliaments. By William M. Downs. Columbus: Ohio State University Press, 1998. 316p. \$50.00 cloth, \$19.95 paper.

Kaare Strøm, University of California, San Diego

William Downs does what coalition analysts often preach but rarely practice: examine coalition politics in subnational arenas. Specifically, the author compares executive alliance building in the regional assemblies of Belgium, France, and Germany. His main interest is in the membership of the coalitions that ran the regional governments in these countries between the early 1960s and the mid-1990s. His book clearly demonstrates that these coalitions are worthy of our attention.

Coalition Government, Subnational Style falls into four parts and nine chapters. The meatiest part is the third, which contains chapters 5 through 7 and most of Downs's original empirics. The four chapters that collectively set the stage for this analysis are in aggregate a bit long, but they give a thorough introduction to coalition bargaining and particularly to what Downs calls "constraints on rationality in government formation." Part 1 also includes a commendable discussion of methods and research design. A main problem in studying subnational coalition bargaining is that it is so heavily conditioned by the relevant parties' national-level strategies. Therefore, any attempt to analyze regional politics in isolation from this supergame is bound to be incomplete. This is a challenge to which Downs responds admirably. Indeed, his most obvious contribution lies in the ways in which he captures the contextual and organizational constraints of subnational coalition bargaining.

More successfully than most coalition analysts, Downs makes use of both objective and subjective data. His objective data represent 109 Belgian provincial governments, more than 90 German ones, and 44 from the French Fifth Republic. The key to the "underrepresentation" of France lies in its truncated history: Downs's French data cover only the first two elections (1986 and 1992) after Mitterrand's reform of regional government. In addition, Downs surveyed regional assembly members in these countries, using a closed-ended mail questionnaire. These surveys had about 80 items, but Downs gets the greatest leverage from questions concerning the members' perceptions of their electoral and organizational environments. For each country, Downs selected a sample of (four to nine) regions in which he polled all regional assembly members. The roughly 600 completed questionnaires correspond to a response rate approaching 40%. Finally, the author complements these data with less formal elite interviews.

The main concern of Downs is to understand the constraints under which politicians make their coalition bargains. He assumes that party leaders are goal oriented, rational, and motivated by office, power, career, policy, party democracy, and social change concerns. But different politicians give priority to different goals. Thus, "loyalists" give priority to the former of these objectives, and "radicals" to the latter. The behavior of political parties is determined not only by the ratio of radicals to loyalists, the party's "organizational radicalism," but also by other aspects of the organizational context, such as national coalition commitments and the vertical distribution of authority within the party.

A second intervening force is the electoral context by which Downs primarily means the volatility of the voters. The more volatile the voters, the more party leaders have to condition their choices on the expected electoral consequences. But such relationships are more subtle in subnational politics than in the national arena. The constraining force of volatility depends on the nationalization of the electorate, that is, on the extent to which citizens in their voting decisions respond to national or subnational events. The most constraining electoral environment, therefore, is one in which votes are highly volatile and subnational party switching can be decoupled from national trends, perhaps because voters are attuned to local rather than national politics.

These are the main ideas that guide the analysis. The argument that Downs presents in chapter 3 builds on a complex conceptual scheme rather than a deductive or parsimonious theory. Nonetheless, Downs formulates 13 hypotheses that he tests in the various empirical chapters. He finds that electoral considerations do influence subnational coalition politics and that regional politicians do not always defer to their national leaders. In chapter 8, the last empirical chapter, the author asks whether regional politics has served as testing grounds for potential national coalitions. Can regional coalitions thus have an informational purpose? Downs explores this issue by examining the record of regional coalition innovations, by posing pertinent questions to his regional and national respondents, and through case studies of three such experiments. He finds that regional parliaments indeed serve as laboratories for national coalition politics.

These are important questions and interesting results. Yet, the concluding chapter is somewhat of a disappointment. It is difficult to fault Downs's characterization of the different national experiences, or even his more analytical points, but the conclusion lacks a powerful punch line. In part, this is because Downs typically only seeks to establish that his various constraints have certain simple and rather plausible effects. Rarely does he discuss the interaction of these effects, or their specific implications for our received wisdom. In the process, Downs implicitly falsifies a set of null hypotheses, but few of these are propositions with great curb appeal or accepted truths with famous names attached to them. One therefore occasionally wishes that Downs had formulated his argument more boldly and with greater attention to its implications.

This is a worthy and appealing book that should be read. Anyone with an interest in the politics of Belgium, France, or Germany will find interesting and little-known anomalies in the description of their regional coalitions. These countries have all gone through extended periods during which the national coalition has been constrained by fairly rigid coalition formulas. Downs shows that underneath this rigidity, quite a bit of coalitional deviation and experimentation has gone on regionally. Theoretically, the book tells us less about the end results of coalition bargaining, such as who gets what, when, and how, than about the multitude of circumstances that nudge negotiators in one direction or another. Downs

certainly drives home how large and wide open the agenda for the study of coalition politics really is. He also demonstrates how much we can learn from all those political negotiations that typically do not make the headlines of the national, much less the international, media. The most intriguing lesson, and one that the field surely needs to heed, is the importance of learning and experimentation in coalition politics.

The Entrepreneurial State in China: Real Estate and Commerce Departments in Reform Era Tianjin. By Jane Duckett. New York: Routledge, 1998. 273p. \$49.95 cloth, \$17.95 paper.

David Zweig, Hong Kong University of Science and Technology

Since 1982, whenever China's leaders have tried to trim the bureaucracy and the state budget by cutting the number of state employees, a "company fever" has emerged. Faced with redundancy and financial constraints, bureaucrats, on their own initiative, have established semiindependent companies within their administrative field; these rely on profit-making activities but also drew heavily on their administrative ties to improve their business opportunities. Some of these companies' profits also pay for the bureau's own administrative costs. Only a few political economists studying China have paid any attention to this widespread and remarkable phenomenon. Jane Duckett's book, which uses the growth of these companies to build a new model and theory of the socialist state in transition—the "Entrepreneurial State" (ES)—fills a very important gap in the study of the post-Mao reforms.

Duckett differentiates ES from the "developmental state" and the predatory "rent-seeking" state. Unlike the developmental state, which facilitates but does not actively engage in business activities, bureaus in the ES are risk takers; they invest capital in firms that compete among themselves and with other companies for market share and profits. Unlike the rent-seeking state, these state-financed companies adapt to, rather than resist, market activity; they are productive rather than simple seekers of rents; and they support, rather than block, reforms of the socialist economy. Duckett's model, therefore, can explain why China's gradual market transition has succeeded without the high costs of a "big bank," as occurred in Poland or Russia, because state agents, who under the rent-seeking model should resist reform, actually have personal and organizational interests in seeing reform proceed.

Conceptually the book is innovative and thought provoking, but it lacks the empirical data to prove or test the existence of this new state. Also, the historical analysis ends in 1993, before it was possible to assess the significance of these companies for China's socialist transition.

To her credit, Duckett outlines the type of micro- or firm-level behavior predicted by her model. She also readily admits when she sees groups of firms acting in ways that do not fit her model. Her two case studies, the housing market and the commercial goods sector, document the emergence of many new companies from government bureaus, but surprisingly there are no case studies of any companies in the book, so Duckett cannot prove that they are actually motivated by the values she attributes to the ES, such as profit seeking, entrepreneurship, productiveness, and adaptiveness. To test her model, we need the source of firms' incomes and how much they benefit from their ties to their bureaucratic allies. We need to hear the managers speaking for themselves, describing how they view the market, outlining their

own motivations and future plans, and showing how their actions and those of their firms herald the emergence of a new entrepreneurial role for the state.

Although a survey of managers probably would have been impossible, Duckett does not even present in-depth interviews with managers of these new companies, which might show how their mentality and motivations have shifted away from colluding with their bureaucratic allies and toward embracing the market. In fact, Duckett admits that for the bureaucrats who spawned these companies, "the profit motive seems to have been secondary to their desire to find alternative employment for their officials" (p. 136). Perhaps this is true of the managers as well. But Duckett cannot assess their motivations because, as the list of informants in the Appendix shows, her interviews were almost exclusively with Tianjin bureaucrats. She interviewed only seven employees in real estate companies, none in any commercial firms, and perhaps no managers at all. Therefore, while her theory is indeed provocative, the gap between theory and data is simply too great to convince us that her portrait of the Chinese state is any better than the two models that she challenges.

Furthermore, Duckett can assert that the phenomenon she is studying is entrepreneurial and not rent-seeking because of her narrow definition of the latter. Rents are not merely wasteful activity that results from competition over licenses or because individuals or groups spend time lobbying for favors or access to licenses, rather than carrying out productive activity. Rent-seeking also entails profits that are earned because government regulations create inflated rather than market-clearing prices that are attained through legitimate business activity. In the case of China, profits that result from regulations limiting market access to a few firms spun off from government bureaus fall within the definition of rent-seeking, since the consumer and society at large are paying highly distorted and inflated prices that create wealth for state bureaucrats and their allies in these firms.

Is this a real market, full of productive entrepreneurship, or merely bureaucratic capitalism? Will these companies develop their own interests, pull away completely from their bureaus, and create a true market economy, or will collusion between the companies and their bureaus—which Duckett recognizes—trap China in a cycle of endless corruption? Duckett's model leads us to be optimistic, but only time will tell. And here, again, Duckett's interviews cannot help us, because they were completed in 1992–93, just as the most recent "company fever" occurred. Duckett, recognizing this limitation, often speaks in terms of possible trends, but she needed to return to Tianjin in the mid-1990s for follow-up interviews if she wanted to show that ES really exists or has taken hold.

Clearly, Duckett is onto something important. Chinese bureaucrats have been given strong incentives to "love the market." My own research on China's "open policy" suggests that such companies can become self-interested and highly profit oriented, and therefore they can act in ways that undermine the regulatory regime established by the state to monitor global transactions. Duckett's firms, therefore, could be the harbinger of a new force that will help expand the domestic market, decrease the size of the bureaucracy, and facilitate a smooth transition away from the planned economy.

The formation of companies does not prove Duckett's theory, however. As Duckett recognizes, the principles under which these companies function is the true test of whether an entrepreneurial state is emerging. We need to know much more about the internal workings of these companies and the

extent to which their market-oriented activity is undermining the regulatory regime inherited from China's planned economy before we can determine whether a new pattern of state behavior, heretofore unseen in any other reforming socialist or postsocialist state, is emerging.

The Rights Revolution: Lawyers, Activists, and Supreme Courts in Comparative Perspective. By Charles R. Epp. Chicago: University of Chicago Press, 1998. 326p. \$45.00 cloth, \$17.00 paper.

C. Neal Tate, University of North Texas

The Rights Revolution is an important book that is truly rare. It is a genuinely comparative analysis of the political/policy roles of a sample of national courts, based on a solid foundation in the literature on law and politics in the sample nations. Most rarely, it is an empirical analysis of a systematically selected sample of the actual decision-making behavior of those courts. To my knowledge, there is no comparable work, one that is genuinely comparative and based on solid, relevant empirical data on national courts. The only study that comes close in spirit, design, and execution, although not in breadth or elegance of exposition, is Schubert's Political Culture and Judicial Behavior (1985).

There is a growing realization that courts around the world are up to something—or at least are in many places expected to be up to something—that appears to be expanding their political significance or policy roles. Nevertheless, to date no study can match Epp's in comparative design, empirical analysis, and consistency of execution. It should be a role model for those who wish to work in this important and expanding research field.

Epp begins with the well-known conclusion that in recent decades the United States has undergone a "rights revolution." His analysis does not treat this conclusion as axiomatic but instead demonstrates the evidence in various ways. Epp then focuses on an important and deceptively simple research question: Why did the rights revolution occur? More accurately, what were the sources and conditions for the rights revolution?

Epp recognizes several conditions as possible or likely explanations for the occurrence of the rights revolution. Most important are "constitutional guarantees of individual rights and judicial independence, leadership from activist judges (especially Supreme Court justices) who have been willing to use those constitutional provisions to transform society, and the rise of rights consciousness in popular culture" (p. 2). More important still, he contends, is the development and existence of a "support structure for legal mobilization, consisting of rights advocacy organizations, rights advocacy lawyers, and sources of financing, particularly government-supported financing" (p. 3).

Having telegraphed his central finding before reaching page 5, Epp certainly reduces the suspense, but that does not make his book less worth reading. Instead, he proceeds to show how answering the question the way he does, rather than in other ways, has important implications for contemporary approaches to politics, institution- and constitution-building, and democratic government. He also conducts the kind of comparative analysis necessary to document that his answer is the most plausible one. To demonstrate the superiority of the support structure explanation, Epp recognizes the need for a comparative research design and develops and executes one.

Epp analyzes four nations: Canada, Great Britain, India,

and the United States. All represent the Anglo-American legal/judicial tradition. He chose similar nations to control the possible confounding effects of legal and judicial variables associated with that tradition. This leaves him free to analyze the possibly differing effects of constitutional guarantees, judicial leadership, public rights consciousness, and rights support structure on the development of a rights revolution.

After laying out his analytical puzzle, Epp presents in chapter 1 a theoretical exposition of the conditions for the rights revolution. In chapter 2 he documents the occurrence of and analyzes the "standard explanations" for the rights revolution in the United States. Chapter 3 explains the significant roles apparently played by the support structure created by modern rights advocacy organizations, changes in the legal profession, and the emergence of new sources of financial support for the revolution.

Epp then analyzes the Indian situation. India is an "ideal environment" for the development of a rights revolution: It has strong constitutional guarantees of rights and of judicial independence; its Supreme Court justices have encouraged and even solicited rights litigation in a manner that will surprise those unfamiliar with that court; and there appears to be a widespread public rights consciousness, at least among the educated elite. There is also abundant evidence that India has had longstanding problems of rights abuse, particularly with regard to prisoners, the accused, and women. Epp finds both quantitative and qualitative evidence to warrant the conclusion that India has experienced at best a weak rights revolution. The apparent explanation? India has not developed a significant support structure to sustain the revolution.

Great Britain is at the opposite pole from India. It has a political and legal environment that has not been very supportive of rights litigation. Its constitutional system is not rights based, and British judges have exercised little leadership in support of rights, even though there has been some significant development of public rights consciousness. Nevertheless, Epp finds that the country has experienced a modest rights revolution, apparently founded on a support structure of rights advocacy groups, the growing size and diversity of the legal profession, and publicly funded legal aid.

Finally, Epp analyzes Canada, which has experienced a "vibrant" rights revolution in the last three decades. Why? Canada has changed from a nation that, like Great Britain, had little constitutional or legal foundation for rights, Supreme Court justices who did not exercise rights-relevant leadership, and limited public rights consciousness to one that, like the United States, now exhibits all the enabling conditions for a rights revolution. The final piece was added to the Canadian rights puzzle only in the 1980s, when a support structure for rights litigation developed.

It may be worthwhile to let Epp have the last words: "Neither a written constitution, a rights-supportive culture, nor sympathetic judges is sufficient for sustained judicial attention to and support for rights. Protection of civil liberties and civil rights depend, in addition, on a support structure in civil society.... Participants in constitutional democracy would do well to focus their efforts not only on framing or revising constitutional provisions, and not only on electing judges who interpret them, but also on shaping the support structure that defends and develops those rights in practice" (p. 205).

Namibia's Post-Apartheid Regional Institutions: The Founding Year. By Joshua Forrest. Rochester, NY: University of Rochester Press, 1998. 408p. \$49.95.

Gretchen Bauer, University of Delaware

Over the last decade, dozens of African countries have sought to make a transition from authoritarian to democratic rule. In some countries, such as postapartheid Namibia and South Africa, the transition has been successful, while in others it has failed, stalled, or been reversed. For countries such as Namibia and South Africa, the challenge is now to consolidate fragile democracies. One aspect of this, insufficiently covered in the vast literature that has emerged to chronicle democratization in Africa, is the creation and institutionalization of strong regional and local level governments. Joshua Forrest demonstrates what a significant contribution to democratic consolidation regional institutions can make, and in so doing he helps fill a gap in the literature.

Forrest's book combines a rich empirical base with a carefully articulated conceptual analysis drawn from the field of public administration. The author conducted a prodigious amount of field research in Namibia during 1993—"the founding year" of Namibia's regional institutions. This included 115 semistructured interviews with regional councillors, regional governors, officials of the Ministry of Regional and Local Government and Housing (MRLGH), and other government ministries and offices, traditional leaders, national councillors, members of parliament, development workers, and more. The book is further based upon observations of hundreds of hours of regional and national council meetings in nearly all of Namibia's 13 regions and the capital, Windhoek.

In analyzing the findings, Forrest focuses upon both the macro level, where policy and decision making take place, and the micro level, where the daily activities and behaviors of members of Namibia's regional institutions are located. He uses a "forward-mapping perspective" in order to interpret relations between regional and central government and a "backward mapping analysis" to investigate the organizational capacity building of Namibia's new regional institutions. Forrest contends that such an organizational approach "can significantly inform the comparative study of state change, particularly in newly established democracies, by assessing public officials' activities and contacts both within the inner circles of national level decision-making power and, especially, in government units located within less formally empowered niches of the state's administrative hierarchy" (p. 2).

Especially for those interested in Namibia, but also for those who follow emerging democracies, Forrest's findings are fascinating. It has been clear since the drafting of Namibia's independence constitution in 1989-90 that the now ruling party, Swapo, never favored the creation of a second legislative house—the National Council, composed of members elected by regional councils. It feared (quite unnecessarily) that such a regionally based chamber would be dominated by other parties. But the level of condescension and indifference during the founding year from some members of the National Assembly portrayed by Forrest as urban elites-toward the National Council-seen as rural and "working class"—was unexpected. Such attitudes were evident, for example, in the National Assembly's refusal to interpret broadly the legislative powers of the National Council and in the assembly's failure to provide higher levels of infrastructural and legal support to the council.

Similarly, it has been clear since independence that the MRLGH was never interested in ceding any substantive

powers to the regional governments, first elected in 1992. This sentiment was embodied most clearly in the statement by MRLGH Minister Dr. Libertine Amathila that the regional councillors' most significant role would be to provide the ministries with information about their constituencies, along the lines of "how many chickens and goats there are" (p. 72). But the depth and breadth of such resistance to empowering the regional councils and governors revealed by Forrest is again surprising. For example, the MRLGH interpreted the 1992 Regional Councils' Act in such a way as to grant only part-time status and an allowance (rather than full-time status and a salary with benefits) to regional councillors. In Forrest's view, this was a clear-cut decision by officials of the MRLGH to "denigrate the role of the councillors" (p. 76). In a similar vein, the MRLGH interpreted the role of the regional governors very narrowlyconsidering them to be governor of their regional council only, rather than governor of their region.

Despite these and many more impediments, both regional and national councils managed to survive and even thrive during their founding year. The National Council established its own set of standing rules and orders, lobbied hard for much needed staff and legal advisors, eventually got its own new space adjacent to the National Assembly, and transcended party divisions in favor of a National Council identity. Most important, it came to perform a significant grassroots transmission role by serving as a "state-society connecting agency," acting on behalf of Namibia's predominantly rural areas. Similarly, the regional councils, councillors, and governors all assumed roles far more significant than the MRLGH ever imagined. Regional councillors and governors alike took their position seriously and made every effort to meet their obligations as they understood them, despite severe resource constraints. Councils in nearly every region tackled significant issues during their first year, ranging from drought relief, to economic development, to their own organizational issues. In the end, according to Forrest, by insisting on challenging the obstacles strewn in their path, the regional and national councils are playing an important role in asserting the concerns of the rural majority and in consolidating democracy in Namibia.

Namibia's Post-Apartheid Regional Institutions provides unique insights into the crucial early years of institution building in Namibia, and at the regional level, which is seldom sufficiently examined. The book is an excellent model for investigations of state building and democratization in Africa and elsewhere. As such, it will very much appeal to a wide audience of political scientists and Africanists alike.

Rendering unto Caesar: The Catholic Church and the State in Latin America. By Anthony Gill. Chicago and London: University of Chicago Press, 1998. 204p. \$41.00 cloth, \$15.95 paper.

Stathis N. Kalyvas, New York University

The allocation of social science research to the issues vying for attention is oddly uneven. Some matters tend to attract great attention, while others remain for a long time in disproportionate obscurity. The allocation process appears not always to take into account the importance of the issue at hand. For example, research in European politics has long privileged the study of left-wing parties despite the fact that right-wing parties have played at least as defining a role in politics. One of the most blatantly underrepresented topics in social science research is religion, particularly church-state relations. Despite its obvious political and social relevance,

this issue has for a long time remained mostly in the hands of antiquarians and constitutional legal scholars rather than social scientists.

There are indications that this is slowly changing. Consider the study of secularization, which has long been the prominent research program in the sociology of religion. As in modernization theory, the main prediction of secularization theory is that economic and social development will cause religion to fade. Since religious practice has failed to vary as predicted, the addition of new sets of conditions to salvage the core of the theory gradually has become dominant. The study of religious practice has been revitalized, however, by the recent infusion of an incentive-based, "microeconomic" approach. Thinking of religion as a "product" produced by "religious firms" is bound to be controversial, but the empirical returns so far have been extremely encouraging.

Anthony Gill successfully adapts and expands insights from this recent wave of research. The empirical puzzle Gill identifies is politically relevant and theoretically compelling: What explains the political position of Catholic churches in Latin America vis-à-vis authoritarian regimes? Why do some churches ally with these regimes while others oppose them? Despite its relevance, this question has not been addressed before, at least never in a systematic and comparative fashion.

By addressing this issue, Gill embarks on the first sustained effort to define the microfoundations of state-church relations and test them systematically. He correctly points out that religious belief per se may be placed outside the realm of rationality, but leaders of religious institutions are subject to concerns and constraints similar to those of their secular counterparts: Bishops are also bureaucrats running large organizations. Gill begins by specifying a model of statechurch relations based on simple and highly realistic assumptions. On the one hand, given that states minimize the cost of ruling, ideology is the least expensive method of obtaining popular compliance, and churches specialize in the production of ideological norms and values, it then follows that states will seek to cooperate with churches in order to minimize the cost of ruling. On the other hand, given that churches maximize membership and are vulnerable to both free-riding and competition, they will seek protection from competition through cooperation with states.

This is particularly true for Catholic churches, which are vulnerable to competition for a number of reasons. Because monopolized religion underproduces religiosity, a Catholic church suddenly facing competition from Protestant churches will suffer substantial membership losses. These will trigger a major political reorientation, and eventually the church will dissociate itself from the authoritarian government. The underlying mechanism is this: Protestant growth is typically fueled by mass conversions of the poorest segments of society. Unless the church is willing to oppose a regime that blatantly enacts antipoor policies, it has no hope of retaining its members, let alone bringing those who defect back to its fold. In other words, "if the Church was to remain a spiritual and moral force in Latin American society, it needed to match the pastoral efforts put forth by the Protestant churches. Having been associated with the political and economic elite for so long, a credible commitment to the poor meant publicly distancing itself from abusive governments" (p. 120). A Catholic church in a monopoly position does not face the risk of membership loss, so it lacks any incentive to dissociate itself from the authoritarian regime.

The empirical prediction yielded by the model (high levels of religious competition will trigger official Catholic antiauthoritarianism) is falsiflable, and Gill proceeds to check it against the available evidence. His empirical analysis is exemplary. He combines an impressive array of tools and approaches typically found in isolation: regression analysis, controlled comparison, short case studies, and an historical overview informed by a good grasp of the large relevant secondary literature and a number of interviews. He is able to disqualify alternative hypotheses, such as repression, poverty, and internal church reform, as well as scrutinize the outliers. The statistical test provides very strong correlation support for the competition hypothesis. A carefully controlled comparison between Chile and Argentina (in which the Catholic church responded to military regimes in opposite ways) sheds light on and confirms the plausibility of the causal mechanism at work. Finally, the hypothesis is extended both vertically, through a discussion of the future of Catholic Progressivism in Latin America, and horizontally, through a preliminary exploration of the challenges faced by Orthodox churches in Eastern Europe. Overall, this is a superior demonstration of how to combine deductive thinking with rich, comparative empirical research.

Gill's model has yet to be fully specified. This is clear with respect to three broad issues. First, the focus on legitimacy understates the fundamental reality of authoritarianism, namely, its coercive nature. At the same time, stressing legitimacy risks reducing religion to little beyond an "opiate of the people." And while the focus on authoritarian states is fully appropriate to the question at hand, the real challenge is to specify a model of church-state relations under democracy. Such a model will have to take into account the role of parties (which in Gill's model are largely substituted by the state), democratic institutions, and cleavages and issue dimensions.

Second, the mechanism of individual defection and the ways in which it affects the options of both church and state remain opaque. A key assumption is that cooperation with authoritarian regimes hurts churches that operate in competitive religious markets. This is not part of the model's core assumptions but derives from an additional implicit assumption, namely, that the poor maximize ideology (broadly understood to include policy). Yet, the mechanism of individual defection toward Protestant churches turns out not to be driven by ideology. What is being maximized by the poor is the quality of the religious product, including such nonideological features as the density of interaction with priests and pastors. But the nonideological utility function of the poor is incompatible with the ideological response of the church to competition (i.e., its opposition to authoritarianism): The church would be providing the kind of inducement that the poor would be disregarding. Indeed, the church could be more successful by improving its services, such as increasing the number of priests and making priesthood a more attractive career option. Gill points out (p. 186), however, that the church is unwilling to act rationally in this respect; it faces such constraints as a conservative pope and a centuries-old doctrine and tradition. These constraints are not exogenous to the church and ought to be specified in the

Finally, it is unclear why competition is the only option available to a church that faces membership losses. It presumably can lobby an authoritarian state to quell competition. Assuming that Protestantism and Catholicism are interchangeable legitimation devices (i.e., equally strong opiates), then the state will be indifferent between them and will have no incentive to provide preferential treatment to any of them; instead, it will encourage competition. If the church turns antiauthoritarian to fend off competition, then the state will have a strong incentive to favor Protestantism over the newly

progressive Catholic church. Knowing this, the Catholic church should think twice about opposing the regime. Backward induction, however, does not seem to be part of the church's calculus. Assuming that Protestantism is somehow more harmful to the regime than Catholicism, the church can credibly threaten to withdraw its support from the regime in order to obtain the restriction of competition. These examples illustrate the need to introduce a dynamic dimension into the model, possibly through the use of game theory.

The points I raise suggest that the payoffs from specifying a full-fledged model of state-church relations are likely to be considerable. Such a model will generate interesting empirical predictions about the configuration of "religious regimes," both across countries and within countries across time, especially under a democratic regime. Rendering unto Caesar is, thus, not just an exemplar of imaginative, theory-driven, and solid comparative research; it is also the first step in what promises to be a novel and exciting research program.

The Emergence of Insurgency in El Salvador: Ideology and Political Will. By Yvon Grenier. Pittsburgh, PA: University of Pittsburgh Press, 1999. 222p. \$40.00.

William Stanley, University of New Mexico

Yvon Grenier has written a critique of what he calls the "dominant paradigm" regarding causes of insurgency in El Salvador. Many writings from the 1980s argue that the Salvadoran revolutionary movement developed because of a combination of economic inequities, worsening social conditions for the poor majority, and violent repression. Scholars depicted the Farabundo Martí Liberation Front (FMLN) as having emerged as an organic extension of peasant organizations, labor unions, and Christian base communities (CEBs) too long repressed by the government. This bottom-up account, according to Grenier, misses the crucial role played by a core group of revolutionary leaders drawn from the middle class. His book seeks to demonstrate that these revolutionary elites triggered an otherwise avoidable internal war. He traces the bellicosity of the Salvadoran Left to a particularly dogmatic ideology and argues that war resulted because the would-be revolutionaries, as well as far Right elements of the oligarchy and military, eschewed moderate solutions during a crucial period in 1979 and 1980.

Grenier makes a number of valuable contributions, largely by pulling together arguments now widely accepted among close observers of Salvadoran affairs but so far seldom incorporated into published scholarship. Among these correctives is a clarification of the relationship between the emerging guerrilla groups and the "popular organizations." Grenier points out what scholars familiar with the Salvadoran Left already knew or should have known, that the guerrilla cells infiltrated and dominated many popular organizations, using them to foment revolution, often at the expense of the organizations' own goals. He also argues convincingly that by focusing on the influence of Catholic liberation theology on the political values of the rural poor, scholars have wrongly neglected the radicalizing effect of the liberation Church on the urban middle class. Similarly, he goes into greater depth than other scholars in highlighting the political role, and extremely rigid ideological environment, of the national university.

Unfortunately, Grenier does not seriously test whether his ideological approach offers a better explanation than alternatives, such as rational actor and opportunity structure approaches. He asserts the importance of ideology and proceeds to tell a story about ideology. His argument would

have been more convincing had he attempted to integrate ideological, materialist, and political opportunity elements. For example, he fails to establish that the bellicosity of rebel leaders during the crucial 1970-80 period was necessarily the result of ideological factors rather than rational calculation. One could argue that hard-line leadership groups on both sides deliberately fanned armed conflict as a strategy to veto moderate political solutions under which the radicals of both sides would be unlikely to prosper. Rebels and state terrorists alike did not want reforms that would reduce political tension, because their strategies for gaining power depended on polarization and conflict. Granting that ideology and identity shaped these political entrepreneurs' chosen paths to political power, many of the actions Grenier cites as evidence of ideological distortion are plausible (although risky) strategies for seizing control of the state. Moreover, the structural and political conditions that Grenier seeks to downplay arguably shaped what strategies for taking power were available to would-be revolutionaries.

By confining his inquiry to the initiation of the insurgency, Grenier biases his study in favor of finding the ideology of leaders to be important. He largely avoids the crucial question of why people followed the revolutionary vanguard. The willingness of large numbers of people to join or support the fight was a crucial factor in making a maximalist revolutionary strategy feasible, and this willingness was in turn a result of structural preconditions (that Grenier dismisses), combined with skillful manipulation by the revolutionary leaders (see Tom Gibb, In the Shadow of Dreams: El Salvador's FMLN Rebels, forthcoming). Moreover, the initiation of insurgency in El Salvador would not be interesting had the FMLN not become a powerful force capable of fighting a prolonged war. International material assistance was crucial to making it reasonable for revolutionary leaders to attempt to overthrow the government. Grenier almost completely ignores the extensive evidence now available regarding the FMLN's international connections and the effect this had on the opportunities before rebel leaders.

In a book that purports to examine the influence of leaders' ideology, Grenier cites few direct interviews with major actors on the Left. Much of the evidence is from secondary sources, including latter-day confessions of youthful zealotry by leading FMLN commanders (such as those appearing in Marta Harnecker, Con la mirada en alto, historia de las FPL Farabundo Martí a través de sus dirigentes, 1993). Grenier seems rather uncritical in interpreting these sources: One cannot help but wonder whether it was better for FMLN leaders to confess to ideological excesses than to manipulation of their trusting followers. He also ignores some of the strongest recent contributions to scholarship on the Church and on the FMLN's relationship to its followers, including Teresa Whitfield's Paying the Price (1994) and Elizabeth Wood's Forging Democracy from Below: Contested Transitions in Oligarchic Societies (1998). More systematic interviewing might have helped avoid a number of factual errors. For example, Grenier misses very substantial differences among the five rebel groups. The National Resistance and its popular front (FAPU), for example, took substantial risks in 1980 to support reformists in the military, which suggests an openness to moderate solutions that contradicts Grenier's depiction of all factions as equally rigid.

Finally, the account of the role of ideology would be more complete if the author had addressed the idea of counterrevolutionaries. Grenier is too often glib in his treatment of the Right (such as when he declares [p. 41] that the goals of the ruling right-wing party, ARENA, have remained unchanged during the 1990s). Extremism by both sides helped start the

war, and moderation by both sides helped end it. We need a good account of how these changes took place and of how ideological and materialist factors interacted on both sides. Grenier's elegantly written book offers an initial piece of such an analysis, and it correctly calls our attention to the need to reassess why El Salvador's bloody war happened.

Democracy and Civil Society in the Third World: Politics and New Political Movements. By Jeff Haynes. Cambridge: Polity, 1997. 211p. \$26.95 paper.

Manochehr Dorraj, Texas Christian University

The emergence of the two contradictory forces of economic integration and cultural particularism unleashed by globalization has generated much theorizing and speculation among academics and policymakers regarding the nature of the new political reality. Jeff Haynes's contribution is another attempt to reconfigure the meaning and the ramifications of these changes in the Third World in the post-Cold War era. Haynes is particularly interested in the study of indigenous movements (what he refers to as "action groups") and how they affect the development of civil society and enhance empowerment in Third World societies. Haynes is also interested in whether such groups will endure and their political actions will usher in a genuine democratization of political life or are a flash in the pan, and as soon as authoritarian regimes are able to regroup and assert themselves, they will disappear. Finally, Haynes undertakes a valuable comparative study of these action groups. He compares them with their Western counterpart at an earlier stage, thus making many insightful theoretical observations that shed light on commonalties as well as peculiarities of the Third World experience in democratization in recent years.

A major goal of Haynes's research projects is to determine how development can be synthesized with democracy. Haynes accurately sees the rise of indigenous movements in the Third World, with their often populist and nativist political agendas, as a response to economic and moral malaise, what he terms "macro economic decline." Unlike some scholars who confuse the symptom with the cause and focus entirely on the reactive, nativist, and some of the antidemocratic attributes of the Third World noninstitutional movements of empowerment, Haynes sees these "action groups" as movements of liberty, equality, and autonomy. According to Haynes, these are not a threat to democracy; rather, they are at the cutting edge of a democratic revolution. These action groups are an essential part of the civil society and play a crucial role in the democratization process. Haynes provides a wealth of information and comparative data to substantiate his theoretical observations with specific references to select countries in the Third World. His differentiation among substantial, formal, and façade democracy is also helpful in identifying controlled democratization from above, designed to maintain authoritarian regimes in power, from genuine democratization from below, initiated by the political action of civil society.

Haynes's attempt to understand the people of the Third World and their political behavior in the context of the social forces that animate their conduct is laudable. It stands in sharp contrast to the often ethnocentric notion of supposedly authoritarian predisposition of Third World people, which is common among some Western scholars. For example, Haynes asks the pertinent question regarding the alleged undemocratic proclivities of Islamist groups: Why should such groups necessarily conform to what is expected of them by people steeped in often a very different culture and life

experience? Haynes's method of understanding "the other" in its own terms has its merits, but it falls short of capturing the domestic sources of authoritarianism in the region, which have deep roots in culture, history, and institutional developments in the Third World as well as the effect of colonialism. To be sure, structural adjustment programs and the austerity measures proscribed by the IMF and the World Bank as well as the influence of cultural imperialism have played a large role in the rise of the indigenous movements, but these, as demonstrated by many recent examples in the Third World, have not all been movements to foster civil society or build community. Afghanistan, Sudan, and Somalia are a few tragic recent examples of indigenous movements that destroy civil society and traditionalize authoritarianism under the banner of either religious authenticity or nationalism.

In other words, many Third World nations not only have to face the devastating effect of economic dislocations induced by a world economy in which their influence is minimal but also struggle against "demons within," historical legacies of authoritarianism. Haynes also seems to be too willing to grant the label of democracy to regimes in Bangladesh, Mongolia, Nepal, Pakistan, the Philippines, South Korea, and Taiwan, which are in fact a kinder and gentler kind of authoritarianism. And many of them fall in the category of façade democracies that tolerate political opposition only if it is loyal opposition. Contestation of political power is not yet a part of the rules of the game in may parts of the Third World, and democratization is often accompanied by retrenchment and the return of repression.

There may be some ground for optimism, however. One can make the case that just as it took the Western world centuries of struggle to define and bring to maturation its democracy (a project still in the making), Third World societies must go through their own conflicts and concord, steps forward and backward, to define their own brand of democracy. Surely, the Third World democracy at the end will not look anything like its Western counterpart. It will carry the stamp of its own history and cultural baggage as it manifests itself. The major contribution of Haynes's work is that, unlike many, the author understands the basic thrust of this historical march toward democratization and has accurately identified the forces that animate it. Thus, his analysis goes a long way in contributing to the literature on social change in the Third World in general and on democratization in particular.

Booty Capitalism: The Politics of Banking in the Philippines. By Paul D. Hutchcroft. Ithaca, NY: Cornell University Press, 1998. 278p. \$39.95.

Carl H. Lande, University of Kansas

Paul Hutchcroft seeks to explain the Philippines' longstanding "development bog." He attributes it to the country's system of booty capitalism, which he examines in the particular area of private commercial banking.

Patrimonial systems, Hutchcroft notes, following Max Weber, by failing to provide calculability in the political sphere, impede the development of more advanced forms of capitalist accumulation. But there are different types of patrimonialism. The Philippines presents a case of "oligarchic patrimonialism." Unlike administrative patrimonialism, in which the primary beneficiaries are members of a state bureaucracy, the oligarchic variety enables a politically influential business elite to extract privileges from a largely incoherent bureaucracy. Such a system need not decline as economic growth

proceeds, for that growth by itself may only strengthen the existing oligarchs, and these have little incentive to press for a more legal-rational political order that deprives them of their special privileges.

In a useful historical section (pp. 23–30), Hutchcroft explains how the Philippine oligarchy became dominant over the state. Under both Spanish and American rule, wealthy land owning families, many of part-Chinese descent, emerged in the provinces alongside a weak and understaffed colonial administration. When elections were introduced early in the century, these families won control of the legislature. But there emerged no countervailing bureaucratic-aristocratic elite, such as in Thailand, capable of defending the state's resources against plunder by these families. From the beginning, such self-interested oligarchs have pressured public officials to do their bidding.

The remedy for the Philippines, Hutchcroft believes, is not more "state bashing," of the sort fashionable in the United States, but the creation of a stronger state that can provide essential services, tame the country's booty capitalists, and create a level playing field for economic growth. Changes in the Philippine form of government have not improved decision making. Until a more effective bureaucratic apparatus exists, the author concludes, neither democracy nor authoritarianism is likely to produce the type of discipline essential to development.

The bulk of the book is a detailed history of the Philippine banking system. It begins with the American colonial period, which saw the emergence of a powerful native oligarchy and a weak state. The sugar planters routinely raided the Philippine National Bank for production loans and thereby drove it to insolvency. During the postwar years, Philippine oligarchs diversified their interests and, like their Japanese and South Korean counterparts, founded family-owned banks that have provided inexpensive credit to other parts of their conglomerates.

Ferdinand Marcos, under this dictatorial regime, had an opportunity to reform the banking system. Instead, he and his cronies gained ownership of twelve banks and raided the state's financial institutions through loans "at the behest of the President." After Marcos's fall, the Aquino administration (1986–92) could only begin to address the longstanding weaknesses of a banking system that had been exacerbated by its predecessor.

From one regime to the next, favoritism in the allocation of lucrative banking privileges, combined with inadequate bank regulation by weak, intimidated regulators, has permitted family conglomerates to loot the loan portfolios of their family-owned banks. More independent banks, meanwhile, have made extraordinary profits through the collusive maintenance of a large spread between rates for loans and deposits. The results have been low levels of saving, inefficient allocation of credit, and feeble economic growth.

In the final chapter, Hutchcroft describes the efforts of the Ramos administration (1992–96) to curb the cartels and monopolies of major oligarchic families as a part of a broadly ranging program of economic liberalization and privatization. That included opening the banking system to more domestic and foreign competition as well as stricter regulation. Even this, in the banking sphere, was largely stifled by those who were to be reformed.

Hutchcroft attributes Ramos's ability to impose at least partially successful reforms to a new recognition by many Filipinos of their country's place among Southeast Asia's competing economies following the expulsion of the U.S. military bases. More specifically, he attributes it to the influence and political skill of the president's security advisor

and main ideologue, retired General Jose Almonte, who like his chief was determined to free the bureaucracy of the influence of the oligarchy.

Events since the book's publication will have lessened the author's optimism. In the presidential election of June 1998, which ended Ramos's one-term presidency, the voters replaced him with a former Marcos follower and aging movie star, Joseph Estrada. Although he campaigned as the special friend of the poor, Estrada quickly restored much of the wealth and influence of Eduardo Cojuangco, Marcos's most notorious "crony capitalist" and Estrada's main campaign financier, and other discredited figures from the Marcos regime. Less favored businessmen have groused but can do nothing. A recent survey of 21 executives of multinational corporations based in Asia found that, among those that had withdrawn from the Philippines, most had done so because of the capricious court system, ambiguous government policies, and the prevalence of graft and corruption.

Ramos and his staff are now in the political wilderness and so, too, one must suspect, are their hopes for further reform. The Estrada administration meanwhile is preparing charges against those involved in multibillion-peso "behest loans" allegedly granted by government financial institutions to cronies of the Ramos administration.

Hutchcroft will not be surprised at these developments. The major push for change under Ramos, he found, came not from a business sector anxious to alter unproductive modes of production but from a committed core of reformers within the administration. No such core has emerged under Estrada. As the author observes (pp. 248-9), while "top down reforms can help initiate major political and economic transformations," they "must be supported by concerted pressures from below." But "there is as yet little evidence of the creation of a broad social coalition able to sustain reform pressure in future years."

This is an outstanding piece of research and analysis. The author's argument is persuasive and seems well supported by the facts. It makes a significant contribution to the study of Third World political economy. It would have been helpful, however, if the author had included a fuller discussion of the dynamics of an electoral democracy that, for most of this century, has enabled prominent families to dominate, for their own advantage, decision making at local, provincial, and national levels. That would have made clear why a broad and effective social coalition for reform is not a likely prospect.

Women and Democracy: Latin America and Central and Eastern Europe. Edited by Jane S. Jaquette and Sharon L. Wolchik. Baltimore, MD: Johns Hopkins University Press, 1998. 250p. \$49.00 cloth, \$16.95 paper.

Jennifer Abbassi, Randolph-Macon Woman's College

In Prague recently, the director of a small theater described the difficulties of staging performances at a time when there is no social consensus on what is worth laughing about. This director had been an active member of the dissident community and in the late 1970s was one of the few women to sign Charter '77, which demanded the regime's respect for basic human rights. It was easier back then, she reflected, to produce and mobilize when the identity of the enemy was clear and energies could be channeled toward ousting what was not wanted. This perspective from the Czech Republic was not unlike that offered last summer by a Chilean feminist scholar, who credited the country's 17-year military dictatorship with creating the conditions against which a broad

spectrum of women would mobilize. Opportunities to join forces and react to multilayered oppression drew women around active agendas for change. The end of military dictatorships in South America and of communism in Central and Eastern Europe have blurred familiar points of reference and caused a shift in the rationale for women's action. It is no longer as clear as it once might have been to define the contours of democracy for women or even what it means to be liberated and how to get there.

These monumental changes of the past decade or so have compelled researchers to cover new ground related to theories of democracy and democratization. One approach takes a critical view and sees the establishment of any form of meaningful democracy as unattainable under current global social, economic, and political conditions. Another school accepts the democratization process as such and explores the ways citizens define themselves in relation to the state and (usually representative) democratic processes, this definition being the basis for one's involvement in the rebuilding of civil society. Such is the case with the volume under review, which focuses on changes in women's social, political, and economic status related to expected improvements in the distribution of authority and resources.

For the contributors, whether and what kinds of gains are made reflect the nature of each transition under study, with the assumption that the more active women are in demanding improvements and the more willing states are to accommodate such demands, the greater are the prospects for and evidence of democratic consolidation. This sets the parameters for a liberal/egalitarian, rights-oriented approach that sees the range of transitional experiences as potential opportunities for women to expand their citizenship powers. Those working in this vein know democracy when they see it; the challenge is to account for the variations and exceptions. Because the authors are sensitive to the distinct historical experiences of women in the regions under study as well as the particular conditions of each national context, the temptation to overgeneralize is avoided. The approach not only adds to prospects of middle-range theory building around gender and democratization but also gives a realistic picture of the very uneven and nonlinear process of political transition. Its implicit emphasis on the absurdity of the term "posttransition" is, in my view, one of the book's most important contributions.

The combined studies operationalize women's participation relatively broadly. The dimensions of participation are not defined uniformly across chapters, but all relate to women's representation and citizenship. Women's voting behavior, party and interest group involvement, and issuebased legislative agendas are analyzed alongside their activities in nongovernmental organizations (NGOs), communitybased groups, the formal and informal labor markets, and autonomous social movements. Excessive dependency on foreign funding for women's NGOs, the need for linkages between feminist policy agendas and autonomous women's organizations, liberal feminism's critique of home-based activities, and other points of current debate are explored in useful detail. Political economy is not given sufficient systematic attention in the book, but there is some discussion in the chapters of the links between the effects of structural adjustment policies and the circumstances that condition women's political and economic realities and "choices." These are important factors that tie directly into questions about democracy, representation, power, and citizenship.

The authors generally recognize the many barriers that limit women's participation in politics. Faltering ideological support for gender equality, conservative institutions and criticism, suspicion of elitist male-oriented establishment politics, contradictory state policies, and basic noninvolvement of women themselves are issues with which those in the business of theorizing gender must grapple. One would expect a significant relationship between the nature, historical context, and speed of the transition process and the ability of women to construct gendered citizenship. The case studies bear out this assumption. Differences in states' responses to women's demands, differential effects on women of the prevailing neoliberal economic model, and the varied ability of women as actors to adapt to new political realities are just several of the factors that explain the mixed bag of women's successes and setbacks.

The necessary ingredients for democratic consolidation seem to include conditions that invite participatory construction of civil society and conditions for sustainable improvements in the status of women as well as rich public debates over gender and power. If so, then no single case in the volume invites unhindered optimism, which raises the analytical question of how much discrimination against women can be "allowed for" in countries bearing the classification "transitional." Read in this light, the case studies draw attention to the intersection between democratic and feminist theory building and both the opportunities and challenges inherent in that endeavor. At the same time, they test the range of the comparative method at a time when the field of comparative politics is pushing for ways to find patterns in and make defensible generalizations about developments in transitional societies.

In the opening chapter, the editors ask what women's participation can tell us about democratic consolidation. Certainly, the roles they played in bringing an end to authoritarianism reflect the capacity of mobilized women to induce change. The case studies also show that women are now acting in capacities that tie in broadly to the process of postauthoritarian politics and living, a level of involvement which would not contradict the general knowledge that women do not (cannot) bury their heads in the sand in the face of new challenges. Yet, the chapters present substantial evidence that, at least according to liberal egalitarian measures, women's issues and gender agendas are largely on the defensive. This is especially the case in former communist countries, although the numbers and influence of women in the South American cases are down as well. Reductions in the number of women in formal politics, the weaknesses of women's government ministries, state cooptation of women's organizations, deteriorating standards of living, preoccupations regarding family survival, and other forms of economic and political marginalization of women from Peru to Poland very much dominate the stories of women in the book.

How are these realities to be factored into theorizing about women and democracy? If they are assumed to be similar to the struggles that women from the North faced over the last century and part of the growing pains of democratization, the normative undercurrents may result in oversimplistic, overgeneralized, or out-of-context tendency statements. In reading the case studies of Chile and Bulgaria, for example, it is easy to question whether the end of authoritarian rule has in fact meant a transition to democracy along dimensions that mean something to the lives of women. If there is the core of the research question, then these realities suggest at the very least that democratization per se should not be presented as a "given" (as the introductory and concluding chapters tend to do), but rather as a variable that, along with the rather vague concept of "transition," must be defined and systematically tested.

There is a related need in my view to shift focus from women to gender, to include questions of power and identity in a broader socioeconomic context, in order to make the deeper contradictions of the transition process available for analysis. Anything less would deprive readers of the analytical tools needed to situate the factors that complicate the lives of women and the potential evolution of sustainable democracy in transitional societies (and perhaps elsewhere). The book would have been strengthened if this type of discussion had framed the case studies, but as it stands the volume offers much to the debates on women/gender and the prospects for democracy in postauthoritarian societies.

The Transition from Communism to Capitalism: Ruling Elites from Gorbachev to Yeltsin. By David Lane and Cameron Ross. New York: St. Martin's, 1999. 259p. \$55.00.

Terry D. Clark, Creighton University

Lane and Ross offer a study of elite circulation and renewal in the late Soviet and post-Soviet eras. Their thesis begins with the argument that the nomenklatura, the privileged members from among whom the highest positions in the party and state were filled, did not constitute a programmatically and ideologically cohesive group. Instead, it was highly pluralistic, incorporating numerous competing interests. While this description of the Soviet ruling class is in general agreement with the traditional view (e.g., Jerry F. Hough and Merle Fainsod, How the Soviet Union is Governed, 1979), Lane and Ross make the further point that the Politburo (the party organ responsible for policymaking) was not representative. They conclude, in contrast with Hough and others, that it was consequently not capable of integrating the various interests of the party and state and achieving consensus among them. The implication of their argument, while straightforward, is nonetheless somewhat astounding: No one governed the Soviet Union, it merely "went forward under its own momentum" (p. 39).

The authors maintain that this fragmentation of elite interests was institutionalized in the relative autonomy of the economic ministries. Although the party decided on policies, the ministries implemented them in such a way as to defend their interests and preserve the primacy of the bureaucratic style to which they had become accustomed. Party reformers, including Gorbachev, unsuccessfully attempted to overcome the resulting systemic inertia by moving party personnel into the economic ministries and endeavoring to tighten the oversight function of the party Secretariat over the government. These efforts failed as the party aparatus was coopted by the ministries it was charged with monitoring and controlling. Gorbachev, however, went farther by creating a legislative system and an executive presidency to which he attempted to transfer oversight functions. The result was a power vacuum as the Politburo and Secretariat were removed from their traditional positions within the system.

The logic of the presentation falters somewhat at this juncture. Since the Politburo was insufficiently representative to integrate the government and party structures and the party aparatus was coopted by the former, merely undermining the party's role would not in and of itself suggest that a systemic collapse would necessarily follow. Subsequent analysis of the new Russian political elites instead provides a more complete picture of why the collapse occurred. As Lane and Ross demonstrate, these elites, in contrast to those of the former Soviet Union, were drawn primarily from outside the

party aparatus and government. Most were from the intelligentsia. What the authors failed to consider sufficiently is that, in essence, Gorbachev's political reforms created alternative paths to power than those available through the party or government. In so doing, they served as a catalyst for elite fragmentation at a more fundamental level than before. If the system previously functioned on its own momentum despite government autonomy from the party, it no longer could do so in the face of newly elected legislatures in the republica, which served as platforms for counterelites to seize control over agencies of the central ministries. The consequence was chaos as the central government and republican legislatures vied for control at the republic level.

The thesis is not altogether new. What Lane and Ross contribute to the analysis of the Soviet implosion is a more explicitly elite focus, but a unifying theoretical framework is lacking. In fact, any of three such frameworks are addressed throughout the book. The introductory sections focus on the Cold War debate in Soviet studies over whether the Soviet Union was a totalitarian system or better understood as a system of controlled interest articulation within bureaucratic structures. Later chapters focus on elite circulation theory, which permits consideration of the degree to which Russian elites represent a reproduction or circulation of Soviet elites, but the theory's utility is limited to little more than that. No theoretical explanation is offered as to how and why elite circulation occurred or its effect on the post-Soviet political system. Finally, although the authors give some attention to democratization theory, they fail to capitalize on its focus on the rational choices of elites. This has been most eloquently explicated in Adam Przeworksi's Democracy and the Market (1991). Adoption of the framework would have permitted consideration not only of the degree to which there has been elite turnover but also why it occurred and with what consequences for post-Soviet politics.

Despite this, the book is likely to generate substantial interest, largely because the main conclusion is that there has been a circulation of political elites in the post-Soviet era, an argument in direct contrast with the common view that Soviet era elites have been reproduced in Russian political structures. Those who challenge this conclusion will doubtless do so on the basis of the representativeness of the Russian elites included in the analysis. This will be less problematic in the analyses of career patterns, as they are fairly comprehensive, but it will likely be an issue with the elite surveys used to demonstrate disagreement on policy and ideological issues. The fact that fractional membership is used as a means for discriminating between value and attitudinal differences among elites in the Soviet era legislature whereas socioeconomic background is used in the Russian era analysis will also likely generate some criticism. Indeed, for the sake of consistency it would seem that socioeconomic background would provide the more useful typology, given its relationship with career pattern.

Nonetheless, the conclusion will doubtless prove thought provoking, and the debate likely to be generated will add to our understanding of the nature and problems of the current Russian political elites. It is somewhat regrettable that Lane and Ross did not help guide that process by addressing some of the implications of their findings. Of particular interest are the implications for the Russian state of an elite circulation in the central government confronted by an elite reproduction in the provinces and a partial circulation among economic elites.

The Sudan: Contested National Identities. By Ann Mosely Lesch. Bloomington: Indiana University Press, 1998. 336p. \$39.95 cloth, \$19.95 paper.

John P. Entelis, Fordham University

Africa's largest country has produced and reproduced contested political arrangements ever since its independence from Britain in 1956, alternating between brief moments of parliamentary democracy (1956-58, 1964-69, 1985-89), lengthy periods of military dictatorship (1958-64, 1969-85), and concluding with the current era of coalition rule fusing radical Islamist ideology with authoritarian military power (1989 to the present). Overlapping theories of political economy, imperialism, and neocolonialism have been put forward to explain why and how systems break down in Third World settings, but Ann Lesch uses a culturalist paradigm. She concentrates on key cultural, ethnonationalistic, and religious variables in attempting to explain the Sudan's inability to overcome the chronic crises in political legitimacy and political institutionalization that have long beset state and society.

Conceptually and organizationally, this is a study in modern political history that concentrates on developments since 1983. The book is organized chronologically into three parts: "Background and Context," "The Democratic Period, 1985-1989," and "Polarization under the Islamist Government." Long a student of the subject with extended in-country research experience, Lesch uses a wide range of archival, published, and interview data to elaborate a theory of "contested national identities" that explains the Sudan's difficulty in achieving a consensus concerning its national identity. Given the country's enormous size (a million square miles), extraordinary ethnolinguistic diversity (its nearly 30 million people are classified into more than 50 ethnic groups, further subdivided into at least 570 distinct peoples), and clashing religious identities (Christian, Muslim, animist), it should come as no surprise that postindependence rulers have had such difficulties in forging a common identity from which to structure a constitutional arrangement that is both representative and effective. These challenges in nationbuilding have been compounded by the bitter legacy of Britain's colonial divide-and-rule policies, which exacerbated existing ethnoreligious and regional cleavages.

Lesch locates the Sudanese identity dilemma within the broader concepts of ethnopolitics and territorial nationalism as criteria for the determination of membership in the nation-state. This is followed by an elaboration of two contrasting models for governance—the control model and the ethnic pluralist model. The author succinctly summarizes each model's arguments, but her overall conclusion, anticipating the details of the Sudanese experience, remains pessimistic: "Crafting an ethnic pluralist system that respects minority aspirations and creates an overarching identity for the country is a difficult process" (p. 15).

Almost from the Sudan's creation, in the wake of the British conquest in 1898 of the area that currently defines the country, the northern and southern regions have put forth contested interpretations of what constitutes nationhood. The mostly Arab and Muslim north has consistently rejected the south's claim to a separate identity, although that region is populated mainly by Christians and animists possessed of distinct histories, cultures, and languages. British efforts to impose a unitary system of rule aggravated rather than ameliorated these differences. This background helps explain the immediate collapse of the parliamentary system adopted

at independence, a system which assumed "that participants agreed on the rules of the political game and adhered to a basic national consensus" (p. 37). Since neither condition obtained, parliamentary life quickly lost its legitimacy, and regional tensions mounted. It was in the context of this "deflated legitimacy" that the first of the Sudan's string of military coups took place under General Ibrahim Abboud.

Opposed to secularism and a pluralist model of cultural assimilation, Abboud established a pattern of imposed integration in which Arabization and Islamization were to constitute the core identities of a unified nation-state. This was the pattern that future leaders, whether military or civilian, would pursue with disastrous consequences for people both in the north and the south; civil war has consumed the country for more than twenty years, causing widespread famine, disease, and death. Abboud's forced resignation in 1964 and the brief parliamentary interregnum (1964–69) did little to satisfy contesting groups in the north and the south. Rather than provide the opportunity to resolve tensions and conflicts, civilian rule further divided an already deeply alienated society. Not surprisingly, it was the military once again that intervened in the form of a coup in 1969 led by Ja'far Numairi, an army officer initially perceived as antagonistic to sectarian politics and therefore potentially open to a more inclusive formula of national identity.

Yet, the reversion to authoritarian rule could not but undermine efforts at national reconciliation. Ultimately, Numairi shifted decisively toward an extreme version of the control model as he imposed an assimilationist policy that culminated in full Islamization by 1977. With no hope of reconciliation and more alienated from the core political system than ever, the south resumed its armed struggle against the north. It was during this period as well that multinational corporations discovered oil, further intensifying the north's determination to control the south, where most of these discoveries were located. At this point political culture and political economy crossed a critical threshold; aspirations for ethnonational identity could no longer be separated from economic calculations, which made resolution of the Sudanese civil war that much more difficult.

Despite the author's harsh appraisal of the Numairi legacy, which "undermined any possibility of establishing a pluralist political system [and] ensured that the south would react by resuming the civil war" (p. 58), her most bitter criticism is directed at the current Islamist regime and in particular its preeminent political leader and thinker, Hassan al-Turabi. Even more than the military man who staged the military coup in 1989 (Umar Hasan Ahmad al-Bashir), following another brief period of democratic rule (1985-89), it is Turabi's Islamist radicalism, Lesch believes, which is couched in the idiom of pluralist politics but operates within a suffocating system of indoctrination and control, that constitutes the country's greatest stumbling block to national unification. Given the reality of centralized control under a monolithic ideology, any potential for liberal or ethnic pluralism seems remote. It thus appears unlikely, drawing from Lesch's conclusions, that the current negotiations between the warring parties will result in a conclusive peace.

Some may disagree with Lesch's critical portrayal of Turabi's Islamist formula for "democratic" governance, but few will dispute the conscientious manner in which she has articulated the Sudanese national identity dilemma, using an ethnonationalistic framework that students of comparative politics will find instructive and useful.

Comparative Politics: Rationality, Culture, and Structure. Edited by Mark Irving Lichbach and Alan S. Zuckerman. Cambridge: Cambridge University Press, 1997. 321p. \$59.95 cloth, \$18.95 paper.

Herbert Kitschelt, Duke University

This edited volume provides an excellent overview of theoretical alternatives and complementarities in contemporary comparative politics debates. The philosophy of the editors and most of the contributors is to oppose what Lichbach calls a "messy center" of case- or problem-driven empirical research in comparative politics without much theoretical reflection. The book is divided into three parts. In the first, three authors present the basic theoretical perspectives in their purest analytical form. Margaret Levi offers a concise sketch of rational choice approaches in comparative politics. She correctly observes that rational choice is not itself a theory but a basic framework with a "capacity to spawn testable theory with clear scope conditions" (p. 20). Her essay also marks the limits of theories based on rational choice in explaining political behavior.

Also in Part 1, Marc Howard Ross rejects the "imperialism" of utilitarian interest-based accounts (p. 50) and emphasizes the cognitive and evaluative interpretations actors must achieve in order to act on interests. He also readily admits, however, the vagueness of cultural units of analysis and the difficulty of testing cultural explanations. Finally, Ira Katznelson advocates a return to "configurative macroanalysis" in the tradition of Moore and Skocpol. His essay is stronger on comparative logic than structural theory. Several times he emphasizes the distinctiveness of historical institutionalism from rational choice institutionalism (pp. 103-4), but his illustrations with examples from comparative macroanalysis do not convince me that this approach is really different from macrocomparative analysis based on rational choice. Moore and Skocpol, at least, but also more recent authors in the macrocomparative mode, do not try to account for the constitution of preferences and instead analyze how actors pursue their interests in given structural configurations.

The second part of the collection applies the three perspectives to some of the most important substantive areas of comparative politics: electoral behavior (Samuel Barnes), social movements (Doug McAdam, Sidney Tarrow, and Charles Tilly), comparative political economy (Peter Hall), and the state (Joel S. Migdal). All these essays not only offer rather comprehensive reviews of the state of the art in each field but also nicely demonstrate how rationality, culture, and structure account for different aspects of political phenomena. In the contributions by Barnes, McAdam et al., and Hall, however, I would have liked more emphasis on the contest between theories inspired by different paradigms in the explanation of identical phenomena, rather than their complementarity in explaining different aspects of each field. Does not the convivial perspective that many theories can be partially valid in a given field of empirical research promote what Lichbach chastises as the "messy center" and a certain disaffection with theory among practitioners of comparative politics? Only Migdal breaks this consensualist mode by attacking much structuralist and rational choice research on the state and warning against tendencies to divorce the state from social relations. This suggests a culturalist framework, but it is not clear to me whether different conceptualizations of the state/society relationship are endemic to the three basic frameworks or derive from particular theories within these paradigms.

In the third part of the book, the editors provide synthetic accounts of the development of comparative politics. Lichbach contrasts the three "research communities" by extracting the

basic ontological assumptions embedded in each paradigm, exemplified by representative works. Loosely inspired by Talcott Parsons's theory of social action, he then shows the complementarity of these approaches in a more comprehensive explanatory framework. Rather than validate the "messy center," Lichbach intends to contribute to "creative confrontations" among practitioners of different theoretical perspectives. He does not fully clarify the analytical foundations of the trichotomy of theoretical perspectives, however.

Following Parsons, one might see four rather than three alternatives. If the underlying dimensions of variance among paradigms are (1) different conceptions of human action (instrumental or expressive) and (2) different views of the constraining role of collective arrangements (individualist versus collectivist), four paradigms emerge: individual/instrumental (rational choice), collective/instrumental (structuralist), individual/expressive (symbolic interactionism, ethnomethodology), and collective/expressive (holistic cultural analysis). This fourfold typology brings the rival assumptions of different research traditions into sharper relief. It also signals the proximity of much rational choice and structuralist theorizing (pace Katznelson; both employ instrumental conceptions of human action but construct the tightness of constraints differently) as well as a critical ambiguity in the discussion of cultural perspectives, for example, in the Ross or Migdal essays. At times, they appear to waver between "individualist" or "collectivist" cultural analysis, although it is clear that the former is more prominent in the domain of sociology than of political science.

Alan Zuckerman's concluding essay has more an epistemological than an ontological or paradigmatic bent, although the author tends to identify a particular epistemological stance on the nature of theory and causation with rationalist and structuralist approaches. Zuckerman attacks the covering law model of theoretical explanation and associates it with a linear, causal, deterministic view of social science, intent on making point predictions and shunning "processes" in favor of "structures." His presentation of the positivist conviction, however, appears to fuse a large number of epistemological questions that are not necessarily linked. As reported by Levi (p. 20), some rational choice theorists subscribe to the covering law model without endorsing causal mechanisms. Furthermore, probabilistic, nonlinear law-like propositions that do not make point predictions are quite consistent with the covering law perspective. Finally, Zuckerman does not convincingly show me how a processoriented analysis could bracket questions of causality. Chaos and complexity theories are special cases of causal analysis. Despite these criticisms, Zuckerman's essay is a thought-provoking starting point for epistemological analysis in comparative

Overall, this book raises a large number of vital questions currently debated in the comparative politics literature. It should be obligatory reading for first-year graduate students in comparative politics. What Gary King, Robert Keohane, and Sidney Verba have done for comparative methods in *Designing Social Inquiry* (1994), the Lichbach and Zuckerman volume delivers for comparative politics theory.

For la Patria: Politics and the Armed Forces in Latin America. By Brian Loveman. Wilmington, DE: Scholarly Resources, 1999. 331p. \$60.00 cloth, \$23.95 paper.

J. Patrice McSherry, Long Island University-Brooklyn

Have military institutions in Latin America accepted subordination to civilian rule and shed their penchant for political intervention and coups? Brian Loveman presents a comprehensive picture of the Latin American armed forces in all their complexity and raises fundamental questions about the depth of change in military institutions and civil-military relations.

Loveman begins by posing several questions central to the analysis of civil-military relations and civilian control. Who decides, and through what procedures, what the armed forces' functions will be? When and how are their missions carried out? What is the relative autonomy of the armed forces on professional and institutional matters? What occurs when there are disagreements between and among military and civilian officials (p. xiii)? Most important, who decides when la Patria is threatened (p. xiv)? Loveman's main thesis is that military forces in Latin America's new (and renewed) democracies continue to be political actors whose subordination to civilian rule remains conditional. Within the "military lore" of the armed forces, he contends, the assumption persists that military commanders decide when threats exist and when intervention is warranted, as in the past (p. xiv). The notion of the military as guardian of the nation and the concept of "protected democracy" permeate the militaries, and many Latin American societies as well.

Loveman skillfully presents the complex historical and cultural traditions and influences that have shaped military institutions and their strategic doctrines. Although he is careful to point out the importance of national differences among, and institutional cleavages within, military institutions, he shows how and why armed forces throughout the region developed (and saw themselves) as supragovernmental tutelary elites (p. xx). The author examines the influences upon military forces from the arrival of the conquistadores, with their Iberian military tradition, to the present period of redemocratization. Rudimentary armies emerged during the wars of independence in the 1810s and 1820s and evolved during the era of the military caudillos. Territorial wars and political violence throughout the nineteenth century gave rise to nascent states as well as to militaries that disdained civilian politics and saw themselves as the repositories of national values. According to Loveman, "emergency powers and military ordinances against political opponents, 'subversives,' and bandits became routine" in nineteenth-century Spanish America, which provided a legal rationale for dictatorship and militarism that gradually came to pervade Latin America (p. 40).

Loveman traces the early origins of Cold War national security doctrines to turn-of-the-century Spanish, Prussian, and French military doctrines and attitudes imparted by foreign military professionals. These "modernizing" military trainers fostered "nationalistic, antipolitical, and quasi-religious military doctrines" that reinforced the militaries' view of themselves as saviors of the nation (p. 63). Latin American officers also became painfully aware of the economic and technological backwardness of their countries. Loveman examines variations on patriotic themes between the Depression and the Cold War, and he shows the growing influence in the region of the United States, which displaced European military missions in the 1940s. He documents the rise of Cold War national security doctrines that emphasized internal war, deepened the role of the militaries in politics, and encouraged a round of anticommunist and antipopular coups and repressive regimes from the 1960s to the 1980s. The legacies of this period were defeated leftist and progressive forces, weak constitutional institutions, eroded civil liberties, and thoroughly institutionalized military guardianship.

Loveman fully develops the core concept of "protected democracy" in chapter 7. He suggests that although political liberalization has occurred since the transitions from military

rule (as it did in other historical periods), human rights crimes remain unpunished, militaries retain political prerogatives and policymaking power, and military intelligence organizations continue to conduct surveillance of society. Everywhere, Loveman argues, even in countries where civilians have made notable progress in redemocratization (such as Argentina and Uruguay), military forces still regard themselves as the last bastion of defense and still possess "residual sovereignty" (p. 215, chart on pp. 218–20, 280–1). Moreover, constitutional and juridical bases for protected democracy remain strong.

The author is suggesting that scholars should be cautious about assuming that democracy is consolidated in Latin America (pp. xvi, 212-3, 221-2). When threats are latent or conflict levels are low, protected democracy operates, in appearance, virtually as a formal liberal democracy" (p. 222). Coups and military rule are the exception, not the rule; "protected democracy" means that militaries continue to monitor civilian politics for threats to the country and hold in reserve the prerogative to exercise tutelary powers. Loveman's analysis is a useful corrective to excessive reliance on assertions by military officers of their democratic fealty and subordination to civilians. Officers admit that they do not enjoy intervening in politics, which they consider a dirty business, Loveman argues, and they do not perceive military institutions as appropriate governing bodies. Nevertheless, when circumstances "force" them to assume their duty to save the homeland they have done so, and presumably they will do so in the future. Moreover, threats to la Patria are defined extremely broadly: they may be political or cultural, violent or nonviolent, foreign or domestic (pp. xiv-xvii, xxii–xxiv, 257–8, 272).

The work is readable and well-suited for advanced undergraduate and graduate courses as well as for the specialist in civil-military relations. Much more than a narrow study of these relations, the book is a broad historical account of Latin American politics since colonial times and the role of military institutions as central actors. It draws on a variety of primary documents, and its illustrations—photographs, cartoons, and charts—are a valuable feature. Loveman does not directly engage contending scholarly interpretations that posit the militaries have accepted subordination to civilian rule in the region. His arguments provide an important caution, however, against complacency regarding the democratic commitments of Latin American militaries. Persistent attitudes about guardianship and expansive military missions are incompatible with subordination to elected civilian officials (e.g., pp. 68, 280). Today, as armed forces in Latin America take on new missions in counterdrug operations, counterterrorism, "protection of democracy," environment and immigration control, development, and nation-building, it is a caution worth bearing in mind.

Making Race and Nation: A Comparison of the United States, South Africa, and Brazil. By Anthony W. Marx. Cambridge: Cambridge University Press, 1998. 408p. \$29.95.

Michael Mitchell, Arizona State University

Anthony Marx has produced an ambitious work. It poses significant questions about the manner in which the demarcations of race figure into the calculus of consolidating the power of states. How do racial dyads and triads develop in the process of state formation? Are racial coalitions the

manifestations of elites struggling to find formulas for the smooth consolidation of state power? In more specific terms, why did the United States and South Africa embark on adopting policies of extreme racial exclusion, while another multiracial society, Brazil, apparently did not?

For clues to the answers, Marx turns to the factor of elite cohesion, or the lack thereof. According to him, the greater the extent of intraelite rivalry prior to a critical point in the process of consolidating state power, the more likely is the possibility of elites relying on a consensus over the legal exclusion of a subordinate group to gain such cohesion. In this regard Marx makes a significant addition to the literature in which the state, its properties, and its evolution are of central concern. He makes the issue of race a major axis in the development of modern states.

Marx has marshaled an extensive body of literature to argue his case. In the process he shows an impressive command of his sources. To his credit he does not engage in ax grinding to demolish the work of other scholars. Instead, he weaves his critical commentary into a broader argument about the choices made in the purposeful inventing of racial orders that would serve the ends of making a modern state.

Marx organizes his discussion in the following fashion. In Part 1 he lays the groundwork for analyzing the evolution of racial hierarchies under states in the process of consolidating their national power. He demonstrates the surprising uniformity in the experiences of the three countries in creating a racial order in which people of African descent and indigenous Africans were subordinated to the power of colonial and semicolonial white elites.

Having established common patterns of racial dominance in the three cases, Marx turns in Part 2 to explaining why these countries took different paths in consolidating a state authority on the basis of race. In South Africa and the United States, previous intraracial elite rivalries were settled through a compromise in which elites acceded to imposing a rigid racial order that excluded blacks from full citizenship. In Brazil no compromise was necessary since, as Marx argues, a crucial regime transition (from empire to republic) was made with a more or less intact elite held together by a common purpose of preserving the structures of domination.

In Part 3 Marx deals with the responses to racial exclusion. Ironically, he argues, it was the very consolidation of state power in South Africa and the drive to resolve intraelite conflicts that sowed the seeds of insurgency. As the state was being consolidated along the narrow lines of Afrikaner nationalism and apartheid, the subjects of racial exclusion responded with calls to action. The interethnic rivalries among the indigenous Africans were attenuated by the increasingly wide net cast by apartheid policies over all South African blacks. In the United States, a crystalized racial consciousness was already in existence by the time, after Reconstruction, that the intraelite accommodations to racial exclusion were beginning to take form. This consciousness reached its peak in protest insurgency soon after World War II, when the consolidated state achieved its own heights of power. Leaders of the civil rights movement shrewdly perceived that a state flush from national victories over an economic depression and in a world war could be enlisted to reshape the bargain that had excluded them from national life. And, consistent with his previous assertions of Brazil's "exceptionality," Marx characterizes Afro-Brazilian protest against the racial order as having been muted by the general acceptance of the myth of racial democracy.

Any work that synthesizes such a large volume of material is bound to have loose ends. Several unresolved questions crop up in Marx's discussion of Brazilian "exceptionality."

His argument for this assertion rests on the contention that the Brazilian racial order evolved without resort to legal means because cohesive elites could impose a hegemonic ideology of racial democracy. Having done so, they effectively diminished the possibility of Afro-Brazilian contestation based on overt racial exclusion, as was the case in the United States and South Africa. This argument, which seems plausible at first glance, is grounded in some arguable generalizations. One concerns the extent of elite cohesion in Brazil. The signal event that occurred in the period Marx discusses was the Revolution of 1930, a regional revolt that soon took on national proportions. The ultimate consequence was destruction of the monopoly on power held by the elites in Brazil's two largest states (São Paulo and Minas Gerais) in favor of new national elites catapulted to power by Getúlio Vargas. The turmoil surrounding the Revolution of 1930 and Vargas's assumption of power suggests that, contrary to Marx's assertion, Brazilian elites failed to achieve a degree of cohesion that would have prevented successful insurgencies against the state.

Moreover, the manner in which Brazilian elites transformed the idea of racial democracy into a hegemonic ideology deserves further exploration. Racial democracy may not have been an historically constant view of society fully shared by Brazilian elites. During the First Republic (1889-1930) they were steeped in the ideology of scientific racism. When Gilberto Freyre published his view that Afro-Brazilian culture was an integral part of Brazilian national identity (Case Grande e Senzala, 1933, or The Masters and the Slaves), he was a maverick intellectual who was standing the scientific racists on their heads. Even though his notion of tolerant race relations gained acceptance rather quickly, the idea of racial democracy as a semiofficial state ideology surfaced in rather curious circumstances. Fernando Azevedo's voluminous work, A Cultura Brasileira (Brazilian Culture), published in 1941 as an introduction to the 1940 census, is generally regarded as a classic illustration of official adoption of the racial democracy idea. Yet, Azevedo's work appeared during Vargas's dictatorship (1937-45). How elites, who arguably controlled the apparatus of the state, could have pushed for a racial democracy when the regime of the time was engaged in the general repression of civil society is enough of a twist of ideology and practical circumstances to pique more than casual curiosity. The strange irony of racial democracy being promoted under a dictatorship calls for some explanation.

Still, a work should be judged on the basis of the overall plausibility of its arguments. In this regard Marx has set the terms of debate. The manner in which elites fashioned rules governing the relationship among racial and ethnic groups in the formation of the modern state will be the subject of further exploration and refinement. What Marx has accomplished is to move the subject of state consolidation in multiracial societies, and his insistence on the strategic importance of policies of racial exclusion, closer to the mainstream of contemporary comparative politics.

The Evolution of Inequality: War, State Survival, and Democracy in Comparative Perspective. By Manus Midlarsky. Stanford, CA: Stanford University Press, 1999. 349p. \$45.00.

Christian Davenport, University of Maryland-C.I.D.C.M.

Manus Midlarsky undertakes an ambitious effort to investigate the political implications of inequality (principally that concerned with land but other forms, such as wealth and income, as well). The book begins with the proposition that

inequalities evolve over time as well as space and that these patterns of distribution compel individuals to respond to them either with the development of particular sociopolitical arrangements or with their dissolution. Specifically, Midlarsky discusses two types of goods distribution: the "exponential" form, which assumes there is no difference between actors but merely fewer goods to go around within the context of some expanding society, and the "fractal" form, which assumes there is a difference among actors, and the very nature of the interaction results in the decreased availability of some good.

The bulk of the research explores the relevance of these distribution forms for numerous types of political behavior. To accomplish this task, Midlarsky draws upon a wide variety of historical cases and data sets. For example, to examine ancient state dissolution (chap. 4) he investigates Byzantium, China, the Maya, Ammon, Judah, Israel, Egypt, the Incas, and the Aztecs; to examine the influence of inequality on democratization (chaps. 5, 6, and 7) he investigates ancient Athens, Sumer, Mesoamerica, China, and Crete as well as a global database of 97 countries in 1980; and to examine the relationship between inequality and domestic violence (chap. 8) he investigates various Latin American countries in the 1950s and 1960s as well as African Americans during the period of "urban disorders." The research thus spans the breadth of human history as well as the social sciences. This combination of approaches (formal, qualitative/historical, and quantitative) and cases (across both time and space) is to be commended.

Perhaps one of the more interesting contributions of the book is the discussion of various theoretical paradoxes of inequality, democracy, and state survivability (chap. 10). Although there are five in total, two are particularly noteworthy. The first postulates that a certain amount of inequality seems to be necessary to prompt state development as well as democratization, but too much (especially sustained over time) can result in the destruction of political systems or autocratization. The fourth paradox suggests that although democracy is to a certain degree an internally based development, the process is subject to important "environmental" influences that can completely overwhelm domestic factors. These comments provide a very sober assessment of political change, for they force us to think carefully about the delicate balances that must be sustained in order to achieve as well as keep particular social arrangements intact over time.

Despite the numerous insights provided by the book, it has certain limitations—admittedly beyond the control of the author but relevant nonetheless. First, given the unbalanced nature of the social sciences, certain lines of inquiry are rather well developed in terms of operational definitions and evidence, whereas others are not. In the literature on "the state," for example, it is not clear whether one means the particular institutions affiliated with a commonly recognized governing authority, the size of these entities, the degree of attachment of the resident population to political institutions, or some combination of factors (e.g., Samuel Finer, The History of Government, 1997). The definition is important, for it directly influences the type of evidence needed to conduct and evaluate investigations of "state formation." Because these matters have not received enough attention, there is little guidance for researchers as well as readers.

Second, there appear to be no measures of inequality that can be used reliably and validly across time and space. This forces Midlarsky to be extremely creative (perhaps more so than any research effort thus far), but he occasionally pushes up against the limitations of reasonable data and causal inference. For example, in his discussion of inequality and

early state formation in Egypt, Midlarsky suggests that the evidence for stratification can be found within how individuals were buried at the time (p. 64). He concludes that communal graves indicate relative equality, whereas specially marked/designed or adorned graves indicate social stratification. These patterns are then used to explain state formation.

Different explanations for the special treatment of the dead are possible. A separate, adorned gravesite could indicate wealth (in line with Midlarsky's argument), or it could mean the deceased had lived a good life according to the cultural parameters of the society and was rewarded for this in the afterlife. If burial practices simply reflect the cultural belief system, then a materialistic conception of them would perhaps be inappropriate (Marimba Ani, Yurugu: An African-Centered Critique of European Cultural Thought and Behavior, 1994; Cheikh Anta Diop, Civilization and Barbarism: An Authentic Anthropology, 1991; and John Mbiti, African Religions and Philosophy, 1990).

The data and inference issues appear in various sections of the book. In Part 3, it is argued that democratic political systems emerge from one of three sequences. In the first (pp. 188-91), a certain degree of population density leads to land inequality, which in turn leads to an allocation of rights that reflects the differential effect of various groups/classes on society. In the second (pp. 191-2), land inequality leads to political violence, which in turn promotes democracy as elites attempt to pacify the insurgents. In the third sequence (pp. 192-3), density leads to land inequality, which leads to urbanization, which leads to economic development and, finally, democracy (rights merely represent the successful lobbying power of the now-conscious middle- and working-class coalition).

The models of sequences are tested empirically within the context of a cross-sectional examination of 97 countries in 1980. The principal reason given for the method is data availability, but it is problematic because it does not provide an adequate test of the various theoretical propositions (i.e., it does not allow the examination of sequential influences, thus capturing evolutionary dynamics). Rather, Midlarsky is forced to simplify theoretical models and extract causal inferences from what is described to be the end of a very fluid/complex developmental process. Again, one cannot fault Midlarsky, and his effort to overcome this limitation is both admirable and insightful.

One concludes the book with a sense of how Midlarsky must have wrestled with the sheer magnitude of the subject. Because of his efforts, the reader emerges with a general understanding of the significance of inequality for a variety of subjects that social scientists have considered throughout human existence but have not tied together very well (e.g., S. N. Eisenstadt, Political Sociology, 1971). This is one strength of the work, for it provides a framework for understanding the numerous intersections of inequality with politics. One also gains a strong sense that more attention needs to be given to the subject and that better measures need to be developed across time and space for the proper examination of causal dynamics. Perhaps another strength of the book is that Midlarsky explicitly identifies the varied manifestations of inequality (within different periods, contexts, and locales) and the places where we could possibly collect this informa-

It can only be hoped that inequality will be placed (back) on the agenda (funding for research and incorporation of the variable into the right-hand as well as left-hand side of causal models). This book should move us in that direction, for it reminds us that inequality has existed since the dawn of

human civilization and likely will be here as long as we are, influencing both our actions and our quality of life.

Electoral Systems and Democratization in Southern Africa. By Andrew Reynolds. Oxford: Oxford University Press, 1999. 341p. \$80.00.

John W. Harbeson, City University of New York

Andrew Reynolds makes a welcome and useful contribution to the literature of comparative politics from several perspectives. First, he offers systematic comparisons of the democratization experiences of five southern African countries at a time when intercontinental comparative studies are relatively rare. Second, he helps broaden the database of presumptively globally applicable theories of electoral systems and of constitutional systems appropriate to multiethnic/racial settings to be more inclusive of Africa. The African experience continues to remain grossly underrepresented in the empirical data upon which much of contemporary theory in comparative politics has been grounded. Third, the book presents carefully analyzed and quite rich comparative, empirical material on southern African democratization. Fourth, although not intended as a text, it would work well in upper-level undergraduate comparative politics courses because of its careful presentation of standard literature and the pros and cons of alternative constitutional and electoral systems.

Perhaps most important, Reynolds advances a potentially important modification of the well-regarded Lijphart theory on the structure and applicability of consociational political systems. His integrated, consensual, power-sharing formulation employs singular sub-Saharan African data both to illuminate a limitation of the Lijphart model and to offer a variant that may successfully address that limitation. The use of African data, which routinely is given short shrift in contemporary theory building, to improve upon that theory and truly globalize its foundations still remains a largely unexplored frontier.

Electoral Systems and Democratization in Southern Africa compares experience in Zimbabwe, Zambia, Malawi, Namibia, and South Africa. Arguing persuasively that those democracies that are most inclusive are most likely to become consolidated rather than metamorphosize into "delegative democracies" and worse, Reynolds finds within this set that those tending toward parliamentarism, proportional representation, and power sharing "provide the foundational level of inclusion needed by precariously divided societies to pull themselves out of the maelstrom of ethnic conflict and democratic instability" (p. 268). By contrast he finds what he considers to be strong evidence that "presidencies, plurality single member district electoral systems and majoritarianism combine to create the democratic cousin of Hobbes's allpowerful Leviathan state" and, he might have added, perhaps worse. The analysis lends theoretical support to an emerging consensus that, major problems notwithstanding, South Africa and Namibia are farther along the road to democratic consolidation within this group, Zimbabwe and Zambia are the most laggard, and Malawi is somewhere in the middle.

This conclusion leads to a problem, given the ten criteria employed by Lijphart to identify conditions favorable or unfavorable for consociational democracy. Reynolds's scoring suggests that, within this set, the countries least suited to consociational democracy are those that have progressed farthest toward democratic consolidation, and vice versa. His scoring for South Africa is less favorable than Lijphart's own, but only slightly so. But then Reynolds finds that South Africa

and Namibia have in fact begun to emerge as cases that suggest empirical support for a variant of consociationalism not emphasized in Lijphart's work: "inclusive power-sharing between all significant political forces but not ethnically based minority vetoes of segmental autonomy" (p. 106). Reynolds argues that this integrated, consensual, powersharing sets South Africa and Namibia, especially, apart from Lijphart's model and in fact suggests a creative elaboration of

Reynolds recognizes a limitation of Lijphart's model, namely, that a consociational democratic system may entrench the very ethnic autonomy it has been established to protect. Pursuing consensual governance without minority vetoes or segmental autonomy may escape this shortcoming and, he argues, in fact may be necessary when ethnic and racial identities are more fluid than the consociational model may assume. The evidence of such fluidity in electoral circumstances offered by Reynolds may confuse fluidity in ethnic identity with tactical dexterity. But there is a wealth of African evidence, in particular, on which he might have drawn more fully, that argues for the prevalence of just such fluidity in ethnic identity to a far more profound degree than Reynolds himself observes. Thus, his variant on Lijphartian consociationalism may be both necessary and already evolving in African circumstances as well as an enrichment of existing theory.

It does not detract from the value of the book to suggest that it has not definitively claimed the frontier upon which it has advanced. More scholars and more books are needed to explore and settle the issue for the academy. Reynolds is on solid ground in conceiving institutional engineering as an intervening variable between sets of socioeconomic conditions and given political outcomes. But broader historical forces, externalities, and characteristics of leaders and movements are among the other key variables also important in explaining democratic successes and shortcomings as well as institutional engineering. Democratic outcomes, or the lack of them, are the product of multifaceted historical processes as well as institutional interventions at particular junctures. This book does not necessarily settle the debate about the roles of race and ethnicity in postapartheid South African elections or those about the relative merits of proportional representation and parliamentary democracy versus their opposites in southern Africa or elsewhere. But the book is an important contribution to these debates as well as to those on still larger issues.

The important point is that Reynolds helps bring home to the political science profession as a whole a reality still underrecognized in some circles: The supposed separation of "area studies" from the fields of comparative politics and international relations has long been an inhibiting and destructive myth. In fact, the comparative study of politics within underdeveloped regions of the world continues to be an important contributor to the formulation and testing of theory in these fields.

Behind East Asian Growth: The Political and Social Foundations of Prosperity. Edited by Henry S. Rowen. London: Routledge, 1998. 358p. \$90.00 cloth, \$24.95 paper.

Gregory W. Noble, Australian National University

For the last two decades East Asia has dominated international debates about the political economy of growth. In the 1980s statists and neoclassical economists competed to claim credit for the combination of rapid economic growth with egalitarian distribution of income attained by Japan and the

newly industrializing countries of East Asia. In the early 1990s the spread of growth to China and the larger countries of Southeast Asia spurred scholars and policy analysts to devise hybrid models capable of comprehending the wide range of experiences in East Asia. Behind East Asian Growth is a significant attempt to decipher the secrets of success. Despite its ambition and its publication date, however, the volume gives little hint that the East Asian model of development may be problematic and makes virtually no reference to the dramatic financial crisis that erupted in 1997.

Edited volumes are often criticized for lacking focus, but this one is more schizophrenic than scattered. The first three chapters present a unified model of development that is often cast in doubt by the later chapters. The introductory chapters avoid traditional debates over trade, industrial, and financial policies on the grounds that a wide range of approaches have proved successful in East Asia. Instead, the authors propose a four-part framework based loosely on Douglass North's institutional approach to economic history, with emphasis on limiting government intervention and predation. The first element is effective governance, particularly protection of private property rights (although the authors make an interesting exception for one-off land reforms) and close government-business communications, which they claim limit predation. The second is the creation of achieving and learning societies, including the attainment of universal primary schooling before significant public resources are devoted to university education and a vague notion that policymakers should learn from their mistakes. Equality of opportunity rather than redistribution comes third, followed by external influences, in the Asian case primarily the bracing effects of communist threats, along with positive models and influences provided by Japan and the United States.

The effort to create a single Asian model is not entirely convincing. The volume is a clear example of selecting on the dependent variable: Comparisons to less successful cases in Asia and elsewhere are few and ad hoc. The Philippines occasionally appears as a counterexample, but no chapter examines it in detail, although that country weathered the financial crisis better than many others in Asia. Moreover, the volume makes surprisingly little effort to operationalize and test contentions, much less compare them with competing hypotheses. The discussion of government-business advisory councils, for example, greatly exaggerates their importance in Korea and Taiwan. The authors seem unaware of the pervasive complaints in Japan that advisory councils serve as manipulative "magic cloaks" under cover of which bureaucrats pursue their own interventionist agendas and are routinely used to sustain cartel-like arrangements.

Similarly, the book makes no attempt to quantify or test the argument that differences in protection of property rights explain divergences in private investment and thus economic growth. While the link between property rights and investment may explain extreme cases, such as the transition from feudalism to capitalism or the deleterious effects on private investment of massive and uncompensated confiscations, it is less obvious that it can explain variation in economic performance across countries in which basic property rights are

reasonably well protected.

The volume also does not enter the thorny thickets surrounding intellectual property rights, whose protection has hardly been a hallmark in East Asia. Empirically, the proposition is questionable, as the Korean case makes clear. After the coup of the early 1960s the new Park administration immediately pressured large capitalists to confess to improper activities and "donate" part of their wealth to the government. In the early 1980s, when recession led to massive overcapacity in the automobile industry, the government forced some producers to exit and others to reduce the number of models and engine sizes. Only after seven years of controls helped the industry attain economies of scale and international competitiveness did the government lift restrictions and effectively restore property rights. Similarly, since the recent financial crisis the government has exerted great pressure on the chaebols to exchange many of their major assets to boost economies of scale.

Despite periodic infringements on property rights, Korea has maintained one of the highest rates of investment and economic growth in the world. This should not come as a surprise. Investors seek to maximize not stability of property rights but profitability discounted by risk, and instability of property rights is only one possible source of risk. In the take-off stages of economic development, growth and profitability tend to be high. This leaves governments plenty of room to intervene without scaring off investment, as long as the interventions are reasonably effective, as they were in Korea.

The generally even distribution of income in East Asia appears to support the argument that equality dampens demand for government intervention and thus promotes economic growth, but the chart belatedly introduced on pages 310–1 demonstrates that economic inequality is pronounced even in some successful Asian countries, including Thailand and Malaysia, whose governments have implemented an aggressive policy of ethnic affirmative action. Even when measures of equality look fairly good, it is not always clear that the causal logic holds. In Korea and Taiwan income distribution was not notably equal until export-led growth boosted the incomes of the poor, and in both Korea and Indonesia the popular discontent with the concentration of wealth in the hands of leading conglomerates is a major political force.

Three chapters examine external influences, but two are actually about Japan as model rather than influence. The chapter by Woo-Cumings demonstrates how nationalist ideologies, security threats, and U.S. influence caused some Asian governments to resist predation and take economic performance seriously, but much more could be said about American and Japanese policies as well as the influence of the World Bank and IMF.

Many of the substantive chapters contribute useful empirical material on a range of Asian successes, such as smooth demographic transitions, the alleviation of poverty in Indonesia, and technology promotion in Taiwan, without particularly substantiating the introductory arguments. Others, especially the strong chapters by Raphael and Rohlen on Japan as model and by Stevenson and Snodgrass on educational practices in Asia, implicitly or explicitly call into question the attempt to create a single, modified neoclassical model; it is suggested, instead, that we need to consider differences between northern and southeastern Asia as well as variation over time and industries. Similarly, the excellent chapter by Haggard suggests that superior economic outcomes stem from an "optimal distance" between government and business that only some Asian countries have maintained. His thumbnail sketches of regional countries not only illustrate the ways in which bureaucratic capacity intersects with party structure and private sector organization but also provide glimpses of the strains that led to the Asian financial crisis.

All in all, Behind East Asian Growth is a provocative effort with much useful material. It would have been stronger if it had more consistently followed Haggard's injunction to ex-

plore the variations in regime organization and support that mold economic policy.

Armed Struggle and the Search for State: The Palestinian National Movement, 1949–1993. By Yezid Sayigh. Oxford: Clarendon, 1997. 953p. \$99.00 cloth, \$39.95 paper.

Erika Alin, Hamline University

Yezid Sayigh provides the most comprehensive examination to date of the emergence and evolution of the Palestinian national movement. The book is meticulously researched, drawing extensively on Arab and Palestinian primary sources and hundreds of interviews with Palestinian political leaders and activists. A former adviser to the Palestinian delegation at the Madrid peace talks, Sayigh spent more than a decade researching and writing this detailed historical account of developments in the Palestinian national struggle, from the establishment of Israel in 1948 to the 1993 Oslo Accords.

The themes highlighted by Sayigh will come as no surprise to those familiar with the evolution of Palestinian nationalism. The book is organized into four sections, each corresponding to a major phase in the post-1948 Palestinian struggle. Events associated with each of these phases have been analyzed in previous works, but Armed Struggle provides an incredible wealth of factual information and insight unavailable in the literature. It is an authoritative work as well as an invaluable reference source for anyone interested in the history of the Palestinian national movement.

Armed struggle, Sayigh demonstrates, has shaped not only the goals and tactical choices of the Palestinian movement but also the development of Palestinian political institutions and social identity. External events, notably the establishment of Israel and the 1956, 1967, and 1973 Arab-Israeli wars, as well as domestic developments in countries such as Jordan and Lebanon, where the PLO had a heavy presence, have influenced and constrained Palestinians' political and military options. Yet, the development of the Palestinian national struggle is rooted not only in historical grievances and Palestinians' relations with regional and international actors but also in the particulars of individual leadership, social class, and political organization.

Sayigh provides extensive and detailed accounts of the ideological, tactical, and policy differences that have existed within and among Palestinian groups and the effect of these on the national movement. The disparate beliefs, motivations, and goals of Palestinian leaders have largely determined the character and strategic choices of the political organizations they have created, and the vast divergence of ideological orientations among Palestinian organizations has made for a complex and often contentious process of policy formulation that has, at times, sharply divided and constrained the national movement as a whole.

Following the 1948 war, ideologically oriented political parties began emerging primarily among the Palestinian middle class. Life in the refugee camps took on many of the traditional characteristics of pre-1948 Palestinian social and political organization, but increasing education and urbanization gradually created a new generation of nationalists. These young people joined the Ba'th and other pan-Arab ideological movements, but, as Sayigh argues, following the 1956 war they increasingly realized that "the real challenge lay... in responding to the absence of autonomous Palestinian organizations" (p. 83).

As organizations such as Fatch emerged on the scene during the 1950s and 1960s, differences erupted between them and the pan-Arabists as well as within their ranks over

ideological and tactical goals and priorities. Fatch members, for instance, were divided over the desirability and timing of initiating an armed struggle against Israel and, following the 1967 Arab defeat and Israeli occupation of the West Bank and Gaza, over whether a focused guerilla campaign or a mass popular mobilization initiative would best further the establishment of an "autonomous Palestinian entity" in the occupied territories. Ultimately, organizational weakness, lack of popular organizing, and Israeli repression compelled Fatch to focus on building up its military and political base in neighboring states, which contributed to its subsequent involvement in domestic political conflicts in Jordan and Lebanon

A constant theme in Sayigh's analysis is the historical struggle Palestinian organizations have confronted in asserting an autonomous decision-making authority vis-à-vis prominent Arab states and leaders. Following the 1967 war, Fatch "perceived a near-miraculous opportunity to escape Arab control" (p. 154) and experienced, along with several other Palestinian groups, a revolutionary period of heightened military initiative and assertiveness. This period was shortlived, however, as Arab states proved unwilling to countenance an aggressive Palestinian presence within their borders and grew increasingly weary of future military engagement with Israel. By the mid-1970s the PLO, fearful of being excluded from regional and international diplomatic initiatives, was following the political lead of Arab states that appeared increasingly reconciled to seeking a diplomatic settlement of the conflict.

Sayigh's thesis affirms the centrality of armed struggle in defining Palestinian political identity, but it also clearly indicates the seriousness with which Palestinian leaders pursued the institutional and state-building project, first in Jordan, then in Lebanon, and finally in the West Bank and Gaza. At the same time, the emergence of entrenched and often elitist political structures dominated by exiled Palestinians lies at the heart of the PLO's recent challenges in establishing an accountable and inclusive political entity in the West Bank and Gaza, which until the mid-1980s the organization had considered relatively marginal to the national struggle. Although Sayigh's account ends with the Oslo Accords, the historical analysis it provides will be of interest to those seeking to predict the course of Palestinian political and institutional development.

Armed Struggle is an unprecedented look at the complexities behind the political and military evolution of the Palestinian national movement. The strength of Sayigh's work is the wealth of information it offers on every facet of Palestinian politics as well as its extensive bibliography and source listings. Readers seeking a basic overview of the history of the Palestinian movement should probably look elsewhere. Armed Struggle is for those who want a complete behind-thescenes description of the details and events that account for the accomplishments and failures of one of the century's most vibrant and durable nationalist movements. It is a monumental and ground-breaking undertaking that will undoubtedly stand the test of time.

The Politics of Lawmaking in Post-Mao China: Institutions, Processes, and Democratic Prospects. By Murray Scot Tanner. Oxford: Oxford University Press, 1999. 286p. \$75.00.

Shiping Zheng, University of Vermont

Since the post-Mao reforms began in the late 1970s, law reforms, legal institutional development, and rising power of

the Chinese national legislature—the National People's Congress (NPC)—have attracted much attention from a younger generation of China scholars in the West. For political scientists, the key research questions are both empirical and theoretical. Can we demonstrate that significant changes have taken place in the politics of lawmaking within China's political system? If so, what political implications do these changes suggest, especially in terms of system transition and democratization? These are essentially the questions that Murray Scot Tanner addresses in *The Politics of Lawmaking in Post-Mao China*.

The book is divided into four parts. Chapter 2 examines the strengths and weaknesses of certain theoretical models (the command, leadership struggle, and organizational or bureaucratic politics models) that have affected China scholars' thinking about lawmaking and the NPC. Part II (chaps. 3-6) analyzes the changes in post-Mao China's lawmaking institutions—central party apparatus, the NPC, and the State Council. Part III (chaps. 7-8) presents detailed case studies of the 1986 Enterprise Bankruptcy Law and the 1988 State-Owned Industrial Enterprise Law. In the last chapter, Tanner discusses any effects that changing lawmaking politics and institutions might have upon China's prospects for democratization. The central argument is that lawmaking power in China has been decentralized since 1978, due to the erosion of party control over lawmaking and the corresponding rise of competing legislative bureaucracies within the State Council. its ministries, and the NPC. Consequently, what was a unified, tightly run, top-down process has become a fragmented, multistage, multiarena process that involves three lawmaking systems of different power bases and interests.

Tanner proves beyond doubt that something important has happened in post-Mao China's lawmaking politics and legislature. He further demonstrates that the NPC's institutional development and growing assertiveness have shown great survivability in the face of sudden shocks from leadership struggles and political campaigns. Yet, this is not the book's major contribution to scholarship. Since the early 1990s, there has been a growing literature on the politics of lawmaking and legislatures in post-Mao China, including Kevin O'Brien, Reform without Liberation: China's National People's Congress and the Politics of Institutional Change (1990); Pitman Potter, ed., Domestic Law Reforms in Post-Mao China (1994), Stanley Lubman, ed., China's Legal Reforms (1996), and Lubman's Bird in a Cage: Legal Reform in China after Mao (1999). Although no one should expect Tanner to include studies published after 1996, it is fair to point out that his book is not the only one or the first on the politics of lawmaking and the legislature in post-Mao China. In this sense, Tanner overstates his case when he criticizes political scientists and Sinologists for ignoring the topic (pp. 6-7). This book's major contribution is that it offers a comprehensive and finely grained analysis. It is one of the best institutional analyses of the lawmaking systems not only in the party center and NPC but also in the State Council and its ministries. Tanner's two case studies also set a high standard. While the controversies surrounding these cases have been widely reported in the media in China and the West, Tanner's study is by far the most detailed and informative one.

In terms of theoretical analysis, the book is much less successful. The first problem is the description of the Chinese lawmaking process as "an organized anarchy." After finding serious problems with existing theoretical frameworks, Tanner concludes that the Chinese legislative system in many ways approximates an organized anarchy, suggested by the "Garbage Can model" (p. 34). Three defining features are unclear goal preferences, a decision-making system that lacks relatively clear rules and procedures, and key decision mak-

ers who are not all sustained or regular participants in the process (pp. 28-9). Tanner's two case studies add substance to the characterization of Chinese lawmaking as organized anarchy, but one wonders if the same could be said for the entire process of post-Mao reforms. Are there any issue areas and decision-making systems in China that the "Garbage Can model" cannot explain?

As if in anticipation of such criticism, Tanner suggests that the Chinese government's actions in Tiananmen Square in 1989 and the "one-child" policy are exceptions to an organized anarchy. These "brutally illustrate that, at least on some key issues, the senior Chinese leadership can reach a clear decision and arrange for it to be carried out" (p. 31). Yet, it could be argued that what happened in Tiananmen Square was precisely the result of an organized anarchy in that there were no clear goal preferences, no clear decisionmaking rules and procedures, and no key decision makers who were sustained or regular participants in the decisionmaking process. The implementation of the one-child policy also in many ways approximates the image of an organized anarchy. Would the politics of lawmaking look qualitatively different in different issue areas, such as criminal law, administrative law, national security law, or military and defense laws? This, of course, is beyond the scope of Tanner's book, but it points to the limitations of cases of economic laws in supporting "an organized anarchy" as a more accurate image of Chinese lawmaking systems and politics.

The second problem is that the author raises important theoretical questions in the book but largely fails to follow them through. For instance, Tanner first suggests that post-Mao Chinese lawmaking comprises five relatively distinctive stages: agenda setting, interagency review, top leadership decision making, NPC debate, and implementation. Later, however, he frankly (and admirably) admits that he lacks detailed data on two stages, top-level decision making and implementation, "which constitutes two of the most serious shortcomings in this study" (p. 209, n. 2). Early in the book (chap. 2), Tanner also offers the concept of "inadvertent transition" as a theoretical link between studies of policymaking and studies of system transition or democratization. But when he returns to this topic in chapter 10, what he discusses is "inadvertent transformation" within the system rather than system transition or democratization.

In short, of the three dimensions of lawmaking politics in China studied in this book—institutions, processes, and democratic aspects—the analysis turns out to be the strongest on institutions, weak on processes, and the weakest on democratic prospects.

Poverty and Inequality in Latin America. Edited by Victor E. Tokman and Guillermo O'Donnell. Notre Dame, IN: Notre Dame University Press, 1998. 245p. \$40.00.

James F. Petras, Binghamton University

As the title indicates, this collection of essays discusses the growing poverty and inequalities in Latin America from the vantage point of neostructural analysis. Accepting the basic tenets of free-market capitalism as inevitable or necessary, depending on the author, the contributors argue that there is a need for the return of a strong state as well as an increasing role for civil society, a competent social bureaucracy, and greater social expenditure directed at poverty alleviation, job creation and job training, tax reform, and education. Different authors look to different social-political actors to carry

out these changes. O'Donnell appeals to the democratic sensibilities of the privileged elites (p. 57) and what he calls the "middle sector of the middle sectors" (pp. 60, 61). Tokman looks to responsive governments and "empowering people" (p. 213), and Reilly identifies citizen action and NGOs (p. 174) as the change agents.

Both the diagnosis of the problems of poverty, equity, and social vulnerability and the proposals for new alternatives are all located within the ideological parameters of the globalization paradigm, which (for the authors) defines the terrain for debate.

The data presented on poverty and inequalities do not add much to what has been already published by international agencies and Latin American and U.S. scholars. What is problematical is the source of much of the data: Most are drawn from international agencies, which in turn rely on local government agencies, which have been notorious in inventing data to fit political ends. There is little independent data collection. The most basic problem is underreporting by the wealthiest groups. Today, the informal economy at the top is crucial in much if not all of Latin America, with income from the drug trade exceeding the legal exports of countries as diverse as Mexico, Peru, Bolivia, Paraguay, and, of course, Colombia. The result of unreported elite income is to magnify the degree of inequalities in countries such as Colombia. Because these essays accept and adapt to freemarket foundations as the basis of social policy, it is difficult to make sense of their claims to be creating a new development paradigm. There is a wide gap between the economic imperatives of the free market (and the neoliberal state that proposes and implements them) and the social reforms argued by the authors. Neither NGOs nor competent government agencies have demonstrated either the political or social capacity to bridge the gap.

Most authors propose measures that are compatible with the free market. Rene Cortazar argues for collective bargaining at the enterprise level, and Maria Antonia Gallant calls for more active participation of entrepreneurs and their enterprises in training and education. In Chile and elsewhere, enterprise-level collective bargaining has favored employers and weakened labor, leading to precisely the inequalities that the book purports to criticize. Increasing the role of business elites in the university will further reinforce their power to define socioeconomic agendas in their favor, which will reinforce cultural hegemony conducive to the reconcentration of income upward.

The descriptions of poverty are useful, but the explanations are weak and inadequate. For example, there is no mention of the military dictatorships linked to the big propertied groups that dismantled the social networks and reconcentrated income, and there is no analysis of how the transition to electoral politics was premised on continuities of the socioeconomic elites and the authoritarian military, intelligence, and judiciary systems. Apart from overlooking the historical events and deep structures, the authors fail to consider what Forbes describes as the flowering of a new class of Latin American billionaires ("The World's Richest People," Forbes, October 14, 1996, pp. 110-5), who have appropriated many of the privatized enterprises and who are centrally located to influence elected politicians and shape political agendas. What is more astonishing is the absence of any significant analysis of the role of international actors, multinational corporations, the International Monetary Fund, and so on, which have defined socioeconomic policies that have concentrated income, cut social programs, and increased inequality. To correlate poverty with education levels and other individual attributes is to overlook what

O'Donnell calls the "big picture," the deep structural links that have created an investor's and banker's paradise for the few and the growing inequities that the authors deplore.

The fundamental issues of poverty and inequality are located, as Adam Smith and a distinguished line of political economists since him have noted, in the concentration of state power and property—landed, industrial, and financial. This classical conception of inequality and poverty is indeed crucial for any attempt at reform mongering.

By ignoring the issues of concentrated power and property as sources of crises, the authors provide weak explanations of the economic boom and bust cycles that Latin America has experienced. For Tokman and others, the discussion of Latin America's "economic recovery" and "vulnerability" is largely a policy issue to be corrected. For O'Donnell it is inconceivable to move beyond the existing structures. He states: "So, what can be done? Not much I am afraid in terms of changing the overall situation, at least in the short and medium term" (p. 55). O'Donnell apparently overlooks the significant growth of antisystemic movements, such as the Rural Landless Workers Movement in Brazil, which has occupied large landed estates and settled almost 200,000 families through its direct action tactics; the Colombian Revolutionary Armed Forces, which are influential in more than half the municipalities of the country; and several other significant antisystemic sociopolitical groups. Whether or not the authors agree with the proposals of these movements, there is no basis for taking a position that deep structural changes are out of the question ("now-a-days nobody seriously believes in the possibility of a social revolution," p. 51).

Difficult issues are papered over in this discussion. For example, why has the advent of electoral regimes (what O'Donnell calls the democratic transition) deepened the vulnerability of countries, increased poverty in many countries, and widened inequalities? Why has poverty become chronic despite the proliferation of NGOs and the more than \$8 billion that has passed through their coffers? Can we continue to speak of "civil society," as Charles Reilly does, as a virtuous unitary entity in the face of deepening class divisions and inequalities? The civil society concept has very dubious analytical value in understanding contemporary social polarities. Billionaire owners of conglomerates and street vendors are both members of civil society. Do they have a common set of interests? What is absent is any serious discussion of class, gender, and race difference and conflicts that create and respond to inequalities. Diffuse occupational and income categories are largely descriptive. Correlations between personal attributes and collective social behavior are not causative explanations. Proposals for reform, some of which might provide temporary ameliorative balm to the pain of the many, are not linked to the real existing trade unions, peasant movements, and popular political parties that would put them into practice. The appeal, instead, is directed precisely to governments, entrepreneurs, and middle-sector groups who have over most of the past 20 years of electoral politics moved against redistributive politics.

Money Unmade: Barter and the Fate of Russian Capitalism. By David Woodruff. Ithaca, NY: Cornell University Press, 1999. 218p. \$29.95.

Juliet Johnson, Dartmouth College

Nothing demonstrates the importance of money like its absence. In this pathbreaking book, David Woodruff explains

why Russian enterprises, local governments, and finally the central government itself progressively substituted barter and monetary surrogates for formerly ruble-based economic transactions. In doing so, it challenges conventional thinking not only about the nature of postcommunist market transformations but also about the practice of state-building in the twentieth century.

Woodruff argues that we should study Russia's economic transformation through the "narrative frame" of monetary consolidation. In the process of monetary consolidation, a state asserts its sovereignty over society by centralizing control over the legitimate means of exchange. This gives a state the power to tax, exact seigniorage, and define the territorial limits of its fiscal authority. Political science suffers from a woeful dearth of research on monetary consolidation, and Woodruff's "sociological-institutional approach to money" ably engages and expands upon the work of scholars such as Polanyi, Simmel, Weber, and Knapp in weaving its theoretical web. In this view, money is an institution that finds its source of value in social conventions. Monetary consolidation, therefore, is a difficult political achievement that requires broadly based political support.

In convincingly explaining why Russia has failed so far in its attempts at monetary consolidation, Woodruff employs a truly impressive range of primary written sources and lengthy interviews with managers, bankers, and government officials from several Russian regions. Money Unmade first explores the evolution of the Soviet-era monetary and credit system in the 1920s and 1930s, then aptly demonstrates how the perpetual tension in the USSR between facilitating production and maintaining monetary stability, coupled with the development of "partitioned" money, structured the initial conflicts over monetary consolidation in the 1980s and 1990s without determining their outcomes. Gorbachev's partial decentralization of the economy under these institutional conditions led to rapid monetary expansion and repressed inflation. This trend, exacerbated by the battle for sovereignty between the center and the Russian Republic, encouraged an explosion of barter at the regional level and the strengthening of horizontal economic ties at the expense of vertical ones.

According to Woodruff, the subsequent battle for monetary consolidation in Russia took the form of center-regional conflicts over "legitimate" means of payment, with correspondingly weighty implications for Russian state-building efforts. If the central government could impose the ruble on the regions, then it would gain effective control over the state. If regional transactions permanently devolved into horizontal barter and giro networks, then regions would have little use for the vertical monetary interventions of the central government, and the Russian state would edge farther toward disintegration. At press time, the central government was losing this battle. Although the price liberalization of 1992 and the subsequent dismantling of certain Soviet-era payment mechanisms briefly reestablished the ruble as the preferred means of payment, barter and monetary surrogates had reappeared by 1994 and proliferated rapidly. Woodruff describes this as a "triple movement," borrowing and extending Polanyi's concept of the "double movement" whereby states simultaneously encourage the creation of all-encompassing, self-regulating markets while heavily intervening to buffer the catastrophic social consequences of unregulated capitalism. Although a short review cannot do justice to this complex argument, Woodruff demonstrates that a combination of Soviet-era economic legacies, the failure of the central government to build new administrative capacities, and the political and social imperatives facing local and regional governments forced the central government not only to

tolerate the emergence of barter and giro circuits but also to participate in them. As Woodruff observes, "in such subtle ways is sovereignty surrendered" (p. 147).

Money Unmade provides a timely and erudite corrective to two troubling theoretical trends in comparative politics. First, it demonstrates that the predominant narrative frame for studying postcommunist economic transformations, that of market reform, fatally obscures our understanding of the development process now taking place in Russia. Woodruff's approach reveals the emptiness of phrases such as "rentseeking," "corruption," and "reformers" when applied in the Russian context. Marketization advocates in Russia and the West (those concentrating on price liberalization, privatization, and stabilization) mistakenly thought that they were fighting the twentieth-century battle of the budget, when they were actually fighting the nineteenth-century battle for state control over money. As Woodruff notes, "that the emergence of barter in Russia came as such a surprise and appeared so absurd reflected a profound failure of historical perspective" (p. xii). The focus on monetary consolidation allows him to explain developments that have confounded those focused on market reform, such as the apparently erratic behavior of the Central Bank of Russia, the ultimate failure of shock therapy, and the reemergence of barter even as monetary stabilization took hold in 1994. These competing narrative frames also yield completely different policy prescriptions, inasmuch as monetary consolidation requires active efforts at state-building rather than mere economic decentralization. Moreover, by contesting the voluntarist vision of the state underpinning the market reform paradigm, Woodruff ultimately presents a fundamental critique of the way in which rational choice approaches have been used to evaluate postcommunist economic developments.

Second, Money Unmade occupies a refreshing middle ground in the ongoing debate between "transitologists" and 'area specialists" over the uniqueness of postcommunist transformations. On the one hand, Woodruff actively seeks comparable cases in the book's final chapter, relates the Russian case to familiar historical efforts at state-building and monetary consolidation, and seeks to make generalizable theoretical claims based on the Russian experience. On the other hand, he admits the difficulty of making accurate comparisons given the changed circumstances of states in the twentieth century and the unprecedented institutional legacies left by the command economy. Woodruff encourages us to evaluate postcommunist transformations on their own terms, investigating the evolving institutional development process without presuming to know the outcome ahead of time. As a result, Money Unmade affirms the vital importance of careful case study without retreating into disciplinary

Like most outstanding books, *Money Unmade* contains sins of omission rather than commission. In particular, Woodruff's decision not to address dollarization in Russia or the temporary persistence and subsequent breakup of the ruble zone seems strange for a book about monetary consolidation. Furthermore, his analysis of Russia's exploding nonmonetary economy cries out for a deeper investigation into the parallel monetary economy and for a more explicit look at the role of Western aid and advice in inadvertently facilitating the emergence of barter. These, however, are minor matters. This is simply the best book yet published on postcommunist political economy, and it demands a close and careful reading by any scholar interested in understanding this momentous era of institutional transformation.

Political Change in Eastern Europe since 1989: Prospects for Liberal Democracy and a Market Economy. By Robert Zuzowski. Westport, CT, and London: Praeger, 1998. 176p. \$57.95.

Bartlomiej Kaminski, University of Maryland, College Park

In five core chapters, preceded by an introduction and followed by concluding comments, the author restricts most of his analysis to three countries: Russia, Poland, and the Czech Republic. Each is given a separate chapter with catchy titles that summarize Zuzowski's assessments of developments there: "Carpetbaggers' Country" (Russia); "Spin-Doctors' State" (Poland), and "Czechs Are Different." The other two core chapters are a general review of transformation in Eastern Europe (chap. 2) and a Western approach to the region (chap. 6).

The author believes that a country's precommunist past determines its postcommunist future. Czechs had a working democracy and a relatively well-developed economy (more so than neighboring Austria) before World War II, and therefore they are different. The lack of democratic and market experience makes the task of establishing a market-based democracy difficult, if not impossible: "Czechoslovakia was the only democratic, industrialized country in Eastern Europe" (p. 2). Zuzowski is very optimistic about the Czechs but less so about the Poles and Russians (pp. 145-6). Poles had neither democracy nor a market economy, although some Westerners (not to mention Poles themselves) want to believe otherwise. Their success in conveying this image testifies to their ability to manage perceptions (hence they are spin doctors). Zuzowski argues that Russia has changed a lot over a very short time, but its further transformation hinges on people's tolerance of the pain inflicted and politicians' determination to "pursue revolutionary goals" (p. 65).

Equally crucial to the author's conclusions are his a priori assumptions about East European attitudes and behavioral patterns. These can be summarized as follows. First, "Eastern Europe is a 'street beggar' as a matter of choice, whether conscious of it or not" (p. 3). The implication is that there will be no sustainable economic growth in the region, and all assistance inevitably will be wasted. A beggar will go back to begging once he spends the money. Second, "most East Europeans have a poor perception, if any, of the link between private ownership and democracy" (p. 4). Therefore, external assistance should focus on education, whose task would be "to alter the traditional habits, beliefs, and activities that are inappropriate, ineffective, or counterproductive in attaining a sustainable democracy and a welfare state based on a market economy" (p. 129). Last but not least, Zuzowski believes East Europeans are devoid of ambition. "For many an East European success is tantamount to not being last" (p. 5). With such a huge baggage of behavioral liabilities, can they ever succeed? Zuzowski's optimism about the Czech Republic flies in the face of his assumptions, but he doubts whether that country should be regarded as part of Eastern Europe (pp. 114-5). After all, "the Czechs are more efficient or more pragmatic than the rest of the nations of the region" (p. 115).

But other nations differ as well. Hungarians are different from Poles, Estonians, Slovenians, and so on. Slovaks differ from Czecha, despite sharing the same interwar history. Yet, developments in these countries following the demise of communism have many features in common. All have parliamentary democracies (in contrast to former Soviet republics, excluding the Baltic states, which have presidential systems). They have successfully survived changes in power, thus demonstrating robustness of established institutions. Except for a rather unusual current arrangement between the oppo-

sition led by Klaus and the ruling coalition, which some describe as nondemocratic, there is nothing peculiar about the Czech political system in the context of other Central European countries. Using peaceful change in power as a result of elections and protection of human rights as criteria of consolidated democracy, the Czech Republic is neither worse or better off than nine other countries that signed the Europe Association Agreement (the Baltic states, Bulgaria, Hungary, Poland, Romania, Slovakia, and Slovenia).

Second, there is little if any evidence to support Zuzowski's claim that the Czech approach to economic reform has been "faster, more comprehensive, and ultimately more efficient than approaches taken in other countries of the region" (p. 114). The European Bank for Reconstruction and Development in its 1998 assessment of the progress achieved in establishing market-supporting institutions gives higher scores to Estonia, Hungary, and Poland than to the Czech Republic. It is among the most advanced group but is not a lonely leader, as the author would have us believe.

There is a serious failure in the Czech transformation that Zuzowski ignores. Economic crisis, which began in 1997, has proved that actions did not match free-market rhetoric. Klaus's policy of using state-controlled banks to provide soft credit to privatized large enterprises was "socialist." These de facto subsidies explain the short-lived (but much publicized) Czech miracle, that is, transition without substantial unemployment. The miracle has turned out to be a mirage.

Contrary to what Zuzowski claims, low unemployment was not the result of "the legitimacy and credibility of the Czech government" (p. 111); rather, it demonstrated the lack of microeconomic adjustment. Rapidly growing unemployment since 1996 and the current economic contraction prove that no transition economy can avoid pain when removing distortions inherited from central planning. The Czechs are not different in this respect.

Zuzowski claims that precommunist experience is important to understanding current developments, but this very experience during the interwar period distinguishes Central Europe—the term is not once mentioned in Zuzowski's book—from Russia, Belarus, or Ukraine. Why are European members of the Soviet bloc, including the Baltic states, parliamentary democracies, whereas other former Soviet republics have presidential systems? It may be that one must turn to history to explain it. But wherever one may look for an explanation, one has to notice that the transition paths of the two groups have been different.

The book is rich in generalizations. Some of them may be well taken, others may not. The problem is that careful examination of the evidence rarely supports the author's observations. Even when premises are revealed, their interpretation often misses the point. Richness and diversity in political and economic development in the former Soviet bloc in Europe deserve more sophisticated treatment than offered here.

International Relations

Asian Security Practice: Material and Ideational Influences.
Edited by Muthiah Alagappa. Stanford, CA: Stanford
University Press, 1998. 851p. \$75.00 cloth, \$29.95 paper.

Aaron L. Friedberg, Princeton University

The center of gravity of the international system has shifted in recent decades. In 1960, Asian countries produced only one-fifth of the world's goods and services, versus two-thirds for Europe and North America combined. Thirty years later Asia's share had grown to roughly one-third, while that of the Atlantic community had fallen to around one-half. Some time in the opening decades of the twenty-first century, these relative weights will likely be reversed.

Rapid economic change has been accompanied by a remarkable process of political evolution. After having first been dominated by outside imperial powers and then divided by the Cold War, Asia is finally emerging as a distinct regional state system, a cluster of strong, independent nation-states engaged in intensive and continuous economic, diplomatic, and strategic interaction. What will be the nature of the relations among the newly empowered nations of this increasingly important region? Will the international politics of Asia in the years immediately ahead be characterized more by rising levels of cooperation and stability or by intensifying competition and conflict?

Muthiah Alagappa's edited volume provides an invaluable source of information and insight for anyone wishing to wrestle with these large and urgent questions. To a degree that is unusual for collections of this kind, Alagappa has succeeded in orienting contributors toward a unifying concept ("security practice") and a common set of questions. How, he asks, do elites and central decision makers in various Asian countries think about security issues? Who or what do

they see themselves as trying to secure? What core values are they seeking to protect or enhance? What types of threats do they perceive? And how do they believe that they can best go about meeting them and achieving their objectives?

After setting the stage with a clear introduction, a useful review of recent theoretical debates on the concept of security, and an excellent historical overview of the international politics of Asia from the third century B.C. to the end of the Cold War, Alagappa turns the floor over to his colleagues. The essays explore the evolving security practices of the major Asian powers (China, India, and Japan) and the states of northeastern Asia (North Korea, South Korea, and Taiwan), southern Asia (Bangladesh, Pakistan, and Sri Lanka), continental southeastern Asia (Myanmar, Thailand, and Vietnam), and maritime southeastern Asia (Indonesia, Malaysia, the Philippines, and Singapore). These chapters are admirably rich in empirical detail, and their analytical quality is uniformly high. David Kang makes a persuasive case that there is method in the seeming madness of North Korea's menacing policies toward South Korea and the wider world. Roger Cliff traces the developments in Taiwan that have led in recent years to an intensification of its confrontation with the People's Republic of China. Samina Ahmed explains how Pakistan's precarious internal politics have influenced its policies toward India. It is a tribute to the authors that, in spite of the swiftness with which events have unfolded in Asia during the second half of the 1990s, none of the essays is badly dated or obviously obsolete.

In two concluding chapters, Alagappa attempts to identify, explain, and draw out the larger implications of the sixteen country case studies. He finds that, "unlike in Europe, there is little cause to argue that the state is in decline in Asia" (p. 613). To the contrary, "the state is the primary security referent for security decision makers," and "the nation-state is the most valued form of political organization in Asia" (pp.

612-3). In part because of their comparatively recent emergence from colonialism, Asian elites are preoccupied with internal security concerns (stemming largely from disputes over national identity and political legitimacy) as well as with external or international threats. "The interconnection of the concerns at the two levels is crucial to the understanding of a substantial part of the security behavior of Asian states" (p. 624). Given the multiple challenges with which they feel themselves to be faced, it is not surprising that Asian central decision makers are attracted to notions of "comprehensive security"; they are concerned not only with defending the international autonomy and territorial integrity of the nations they lead but also with preserving what they define as national unity, political stability, social harmony, and, in many cases, their own hold on power. Alagappa finds that Asian security practice is still dominated by self-help strategies, but he sees evidence of an increasing willingness to embrace notions of cooperative security and to explore the potential of multilateral mechanisms to build confidence and contain disputes. Finally, he points out that, "because nearly all Asian states are relatively new . . . and are still in the midst of fundamental transitions . . . and because the material and normative structures affecting them are changing too," their approaches to achieving security are very much in flux (p. 640).

Alagappa's analysis of these common features leads him to two important conclusions. He argues that the findings of the volume's contributors cannot be explained adequately without reference both to material and ideational factors. How leaders define their interests and where they perceive threats (still less how they devise strategies to deal with them) cannot be inferred simply by looking at "objective" factors. Alagappa describes his position as leaning toward constructivism. As he acknowledges, however, the recognition that ideas can play an important causal role is not damaging to realist theories of international politics in general (and especially not to realism's classical and neoclassical variants), only to the rarified and rather desiccated "structural" realism that held sway for a time in the 1970s and 1980s.

For Alagappa the fact that "international politics in Asia, especially during the Cold War, resembled that of the realist paradigm" is "not merely a product of anarchy or of the material structure. It is in considerable part an outcome of past and present ideas and interactions" (pp. 656-7). Things can change, they have begun to do so already, and Alagappa believes they are going to change even more in the future: "International politics in Asia is becoming less Hobbesian; principles and rules are gradually gaining in significance; state interaction is increasingly characterized by conflict as well as cooperation. Cooperation and learning... have contributed to a reconstitution of interests" (p. 660). Alagappa anticipates that "as the international politics of Asia becomes more multidimensional and complex, the realist paradigm will be increasingly inadequate" (p. 660). Perhaps, but the reader is entitled to be skeptical. It seems at least equally likely that lingering mistrust (between Japan and China, for example, or China and the United States, or India and Pakistan), domestic political processes, and shifting balances of power will propel Asia toward a new round of interstate rivalry made more intense, and perhaps more destructive, by the new-found capabilities of the countries of the region. If this happens, Asian political elites will surely revert to the full-blooded realist security practices that are, in most cases, very much a part of their historical traditions. Only time will tell.

Coercive Military Strategy. By Stephen Cimbala. College Station: Texas A&M University Press, 1998. 229p. \$39.95.

David P. Auerswald, George Washington University

Stephen Cimbala describes a world faced with low-intensity conflicts among subnational groups. In this world, the United States will increasingly be tempted to perform peacekeeping and peacemaking duties. Before intervening, however, the United States must develop effective coercive military strategies or risk repeated failure. In Cimbala's words, "mastery of the principles of military coercion is a necessary condition for success in war or diplomacy" (p. 3).

According to Cimbala, coercive military strategies combine the political and military uses of force. We normally think of the former as coercive diplomacy, or more specifically, its component parts: deterrence and compellence. The military use of force is normally thought of as any conventional military action whose goal is battlefield success. Cimbala argues that successful coercive military strategies combine the political and military uses of force in such a way as to ensure flexibility throughout a dispute, vary the pace of any escalation according to circumstances, and maintain domestic and allied political support.

The author then argues that the United States never articulated a coherent coercive military strategy during the Cold War. As examples, Cimbala provides chapter-length treatments of the Cuban missile crisis, the 1991 Gulf War, and the Vietnam conflict. The theme running through these chapters is that the United States failed to learn from past mistakes and instead spent most of the Cold War moving from crisis to crisis, refusing to adapt its coercive military strategy to emerging conditions. Vietnam is Cimbala's classic example of this phenomenon. Even such successes as the Cuban missile crisis and the 1991 Gulf War were qualified exceptions to the rule, caused more by inspirational leadership than by the steady accretion of experience.

Cimbala then discusses how the international environment has changed since the Cold War. He argues that civil wars and chaotic internal conflicts are becoming the norm. Collective security, which relies on knowing who is the aggressor (among other things), may thus be inappropriate for organizing future security arrangements. Operations short of war will also be increasingly difficult in the future, in large part because of growing U.S. domestic opposition to them. Given these trends, understanding the principles of successful coercive military strategy is even more important than it was during the Cold War.

The concluding chapter applies to the future the lessons learned from the Cold War, the American Revolution, and the Civil War. Cimbala argues that successful coercive military strategies have five characteristics: Influence the will of an opponent, maintain the flexibility to revise objectives as necessary, take the perspective of one's opponent before deciding on one's own actions, manipulate the symbols and information associated with a dispute, and maintain support for one's goal both at home and within one's military.

This is an ambitious attempt to broaden the definition of international coercion as more than just coercive diplomacy and to apply that model to emerging international circumstances. To get the most out of this book, however, readers might best be served by first reading the introduction and conclusion and only then turning to the empirical chapters. The reason is that each chapter highlights one or two of the characteristics of successful coercion listed in the conclusions. Putting each chapter into context is thus greatly facilitated by the conclusion's theoretical framework. For instance, one might be tempted to discount a number of

seemingly tangential discussions in the case studies, yet they further our understanding of those cases once their links to the author's overarching theoretical framework are made clear in the conclusions.

Coercive Military Strategy could stimulate a number of creative investigations. Future research, using a larger sample size, might help assess what combination of Cimbala's five characteristics of coercion are necessary for success, or whether a critical mass of one factor guarantees successful coercion. Such a project could demonstrate whether U.S. experiences in Cuba, the Gulf War, and Vietnam were representative of general patterns of U.S. coercion during the Cold War. An alternative approach might be to explore in detail a few U.S. cases involving the Soviet Union or its East European proxies. This is an attractive option given our recent access to Soviet-bloc archives. Using that material, researchers could examine the hypothesized linkages between U.S. actions and changes in an opponent's actual decision-making calculus, which the author was not able to do in the Vietnam or Gulf War cases.

How, then, should U.S. officials adapt policy to ensure future success? Cimbala repeatedly advocates the threat or use of sudden, overwhelming firepower to convince an opponent to capitulate, presumably because overwhelming force contributes to many of the characteristics Cimbala believes lead to successful coercion. This may be sound doctrine, but it may not always be necessary for success. Consider recent events in Kosovo, where NATO's gradually escalating air campaign, according to General Wesley Clark, SACEUR and commander of NATO forces, "was an effort to coerce, not seize." While there is no question that NATO strikes failed to prevent atrocities in the short-term, they arguably achieved NATO's broader goals of maintaining alliance cohesion and regional stability, providing the Kosovars with increased autonomy, and containing Serbia from further aggression.

Moreover, as the author seems to realize, and as the two recent Balkan conflicts demonstrate, overwhelming force may not be feasible when coercion requires maintaining consensus among coalition members. The alternative to multilateral efforts is for the United States to go it alone, but that is increasingly unlikely from a domestic perspective except in the most dure circumstances. One way out of this dilemma would be for the United States to cut back dramatically on its military engagements abroad. This sentiment will surely resonate in some quarters, but American leadership may suffer as a result. Cimbala certainly focuses needed attention on a problem confronting U.S. officials, but his book reinforces the notion that there are no easy solutions to the problem of future military threats and intervention.

The Myth of "Ethnic Conflict": Politics, Economics, and Cultural Violence. Edited by Beverly Crawford and Ronnie D. Lipschutz. Berkeley: University of California at Berkeley (International Area Studies), 1999. 580p. \$28.50.

David B. Carment, Carleton University

The chapters in this volume are drawn from the work of a study group on the political economy of "ethnic and sectarian" conflict organized by Beverly Crawford and Ronnie Lipschutz. The "myth of ethnic conflict" refers to the tautological nature of the term: The label "ethnic conflict" reveals very little about what underlies any conflict, and ethnic differences alone are insufficient to guarantee frustration, political mobilization, and rebellion.

The volume is built around two seminal essays, "The Causes of Cultural Conflict: An Institutional Approach," by

Crawford, and "Seeking a State of One's Own: An Analytical Framework for Assessing Ethnic and Sectarian Conflict," by Lipschutz. Theoretical analyses include "Ethnic Entrepreneurs in the Soviet Successor States," by Philip G. Roeder, "Fall of the First Russian Republic," by Michael Urban; and "Nationalism in the European Context," by Andrew V. Bell-Fialkoff and Andrei S. Markovits.

Complementary case studies provide an in-depth analysis of states and regions at various levels of democratic experimentation, liberalization, and ethnic conflict. There are eight such chapters: an investigation of identity politics in the former Yugoslavia, by Crawford; post-Soviet nationalism in Abkhazia and Ajaria, by Georgi M. Delruguian; "Islamist Responses to Globalization in Egypt, Malaysia and Algeria," by Paul M. Lubeck; "Ethnic Politics in India," by Nirvikar Singh; "Identity Politics in Germany," by John C. Leslie; "Cultural Conflict in the United States," by Lipschutz; "Interethnic Conflict in Great Britain," by Elaine Thomas; and "Identity Transformation in Bulgaria," by Maria Todorova. A summary chapter by Crawford completes the volume.

One risk in compiling an edited volume so broad in scope is that there will be too few conceptual and theoretical threads linking the chapters. The Myth of "Ethnic Conflict" makes an explicit effort to avoid this. The political economy explanation advanced in this volume is that ethnic mobilization is a function of a blend of structural factors (the relative size and location of ethnic groups), ethnic entrepreneurs (the role of elites who have a vested interest in advancing particular agendas), and normative determinants (an appreciation for institutionalized forms of conflict resolution as opposed to coercive measures). The essential argument is that ethnic entrepreneurs who can offer tangible resources to disadvantaged populations are most likely to gain political support. The level of resources available to entrepreneurs is dependent on "(1) the degree to which cultural criteria were historically used to allocate these resources ... and (2) the level of resources provided by international alliances" (p. 36).

The contributors also argue that a state's legitimacy is closely tied to the kinds of ethnic policies it pursues. Narrow policies are usually perceived as less tenable than broad distributive ones. In the absence of strong secular organized parties and strong institutional structures, ideology and culture become the focus for understanding entrepreneurial behavior. In some cases the state does not merely respond to crises, produced by uneven ethnic mobilization and social change, but is itself the dominating force providing differential advantages to regions and ethnic groups. In this case, conflict among elites emerges as a struggle over state power, specifically in newly emerging states. In other cases, when institutions are already weakened after decades of conflict and state suppression, the forces of globalization and liberalization act as catalysts for change by creating political space for ethnic entrepreneurs to emerge.

In the former and perhaps better understood pattern of behavior, conflict arises as a result of the breakdown of the central state, as traditional patterns of authority give way to regional and ethnic forms of political organization. In some cases the breakdown is temporary, although the transition is uncertain. The institutions most deeply affected are federal structures. Whereas power once flowed freely from the top down, there emerges a more narrow base of power controlled by dominant ethnic groups. This partnership is reinforced when the state is challenged by minority groups, itself a response generated by assimilative pressures, policies on in-migration, economic competition, and more direct political threats of secession. The net result is a lethal "policy feedback" process in which the central government's policies

in the form of entitlements for the majority ethnic groups induce minority groups to organize for political action. This challenge in turn generates greater resistance to change from the central state.

In the latter case, exogenous shocks, such as imperatives for rapid economic and political change in response to globalization and liberalization, create a somewhat different and less well understood dynamic. According to Crawford and Lipschutz, broken social contracts and weakened oppressive institutions open political space for ethnic entrepreneurs to mobilize support. If the resources made available to ethnic entrepreneurs are achieved through re-allocation or the disproportionate economic deprivation of one group in favor of another—a result of liberalization, for example—the net result will be the escalation of conflict toward intergroup violence.

The volume succeeds in two areas. First, it highlights the importance of understanding both proximate and underlying causes of conflict. In advancing this argument, the contributors suggest that, among other things, because ethnic conflicts have important regional and international effects, it is important to understand the dynamics of relations that arise as a result of ethnic alliance formation. Second, the volume advances basic understanding of when ethnic conflicts will become violent. Since a knowledge of causes does not necessarily help explain or predict when or how violent interactions will occur, the contributors argue that the probability of violence depends on the opportunities and constraints that present themselves to elites at any given time. Decisions to pursue violence cannot be understood except as part of some larger structural and normative framework. Options available to elites at any moment are constrained by available institutional capabilities and ethnic configurations. These capabilities are themselves a product of processes that occurred during some earlier phase.

Economic Interdependence in Ukrainian-Russian Relations. By Paul J. D'Anieri. Albany: State University of New York Press, 1999. 278p. \$24.95.

Yaroslav Bilinsky, University of Delaware

D'Anieri's monograph is an ambitious and, by and large, successful attempt to apply modern international relations theory to the obstreperous case of independent but vulnerable Ukraine. On the whole, it is well researched and documented, well organized, clearly written, and teachable in courses on the post–Soviet Union. But some lapses should be remedied in its second edition. I will note its flaws first, then pass on to its strengths.

On page 28 we read: "Overall, more than 70 percent of Ukrainians voted for independence [in the December 1, 1991, referendum]." On page 175: "A January 1995 survey reported that 64 percent of Ukrainians continued to support Ukrainian independence, significantly down from the 90 percent of 1991." The correct figure for December vote is 90.32% (Taras Kuzio and Andrew Wilson, Ukraine: Perestroika to Independence, 1994, Table 9.2, p. 189). The Soviet Union invaded Eastern Poland/Western Ukraine and Belarus on September 17, 1939 (p. 25). On September 28, 1939, Hitler's Germany and Stalin's USSR signed a supplementary agreement assigning Lithuania to the Soviet sphere of influence. The correct spelling of the last name of the Soviet general who tried to take over Ukraine for the August 1991 plotters is Varennikov (p. 155); and the correct spelling of Yavlinsky's first name is Grigory (p. 128). A Ukrainian speaker will notice three errors in the Ukrainian title used in

the motto on page 97: the correct title is "Dohovir Ukrainurospne i vona vzhe nikoly ne voskresne" (emphasis added; The [union] treaty will crucify Ukraine, and never will she be resurrected).

Those items are incorrect, and subject to debate are the comparisons of the former Soviet Union with the European Community (EC) and European Union (EU) that the author makes throughout the book, which some Russian politicians such as Grigory Yavlinsky also have made (i.e., in September 1991, Yavlinsky, who has proved a singularly ineffectual Russian political leader, wrote a plan for an economic union based on the European Community's 1957 Treaty of Rome). Russia is in Eurasia, not Europe. After Chechnya I, which cost a minimum of 70,000 deaths (to use the lower estimate of General Aleksander Lebed) and a maximum of 100,000 (according to Dr. Yelena Bonner), and after Chechnya II, which is proceeding as this review is being written, Russia may find it hard to enter some European institutions.

D'Anieri correctly takes quite seriously Russia's Council on Defense and Foreign Policy protoimperial integration plans of April-May 1996 (pp. 7, 202). Later (p. 218) he admits that unlike the EC/EU vis-à-vis the new Germany, the CIS, given the intentions of Russian leaders and the disparity in power among its members, can easily become an instrument for the restoration of the old authoritarian Russian empire. The best strategy for Ukraine, therefore, would not be to try to contain Russia by organizing a coalition of former Soviet states within CIS but to seek entry into EU and transatlantic security structures (NATO), along with Poland, the Czech Republic, and Hungary. But D'Anieri may be right that the bad performance of the Ukrainian economy and Ukrainian political weaknesses militate against achieving those goals in the very near future, unless the United States and European states realize the dangers from a resurgent Russia (p. 212). A Russia, we may add, that plays its own power game in Kosovo, Iran, and Iraq and effectively rewrites the terms of the IMF loans for its own benefit.

I agree with the author that the case of Russian-Ukrainian relations does not fit the liberal strands of hegemonic stability theory of international cooperation of Charles Kindleberger and Robert Keohane, more the realist strand of Stephen Krasner and Robert Gilpin (p. 218). D'Anieri has documented it amply in what is perhaps the best chapter in the book, that on the energy war of 1993–94 (pp. 69–96). The author imaginatively uses Albert Hirschman's analysis of Germany's economic warfare between the wars.

D'Anieri also tries to come to terms with the realist arguments of Kenneth Waltz, John J. Mearsheimer, and Barry Posen that to preserve its power Ukraine should have kept its nuclear weapons (p. 217). This he does less successfully. Chapter 7, "The Direct Economics-Security Connection: Nuclear Weapons in Ukraine" (pp. 147-67), is almost perfunctory. The author also asserts preemptively that the danger from Moscow was not military in nature but economic and political (p. 147). This may have been a rationalization of a faulty strategic decision by the United States and the other powers in NATO (Germany? Britain?) to disarm the strategic missiles deployed on Ukrainian soil and, above all, to press Ukraine to surrender its tactical nuclear weapons to Russia. This was done despite the warnings by Mearsheimer ("The Case for a Ukrainian Nuclear Deterrent," Foreign Affairs 72 [Summer 1993]: 50-66) and some of the authors in John W. R. Lepingwell, ed., RFE/RL Research Report 2 (no. 18), of February 19, 1993, which D'Anieri cites, and by William C. Martel and William T. Pendley, Nuclear Coexistence: Rethinking U.S. Policy to Promote Stability in an Era of Proliferation, 1994, which D'Anieri does not cite. As a result,

Russia still has about 7,000 strategic warheads threatening the United States, and there are about 40,000 tactical nuclear warheads, several thousand of which were yielded under U.S. pressure by Ukraine, that Russia is seriously contemplating using in Chechnya II and that are of very great concern to

D'Anieri partly makes up for his omissions in the security field by his wise general statement toward the end of the book: "Most important, in the former Soviet Union, most of the states are constantly concerned about their survival, and self-preservation preempts other goals. Realism pervades the politics of the former Soviet Union" (p. 222). According to him, Ukraine's glass may be half empty, but partly with belated Western aid, the country survived Russia's energy and political—war of 1993-94. Overall, this is a good, theoretically challenging, if somewhat uneven book.

Forging an Integrated Europe. Edited by Barry Eichengreen and Jeffry Frieden. Ann Arbor: University of Michigan Press, 1998. 340p. \$45.00.

Beth A. Simmons, University of California, Berkeley

In the 1990s, Europeans have had to address issues of deep integration that affect almost every social group to one degree or another. How are we to understand the forces and processes that will contribute to the further integration of Europe as well as those that threaten to undermine it? This volume is a collection of essays by political scientists and economists whose appreciation for interdisciplinary research has done much to develop a political-economic understanding of European integration. With very little editorial coercion toward a particular theme, the essays share an organic compatibility, perhaps because they developed out of a research colloquium on European integration that met over the course of the 1990s. The editors set the agenda by simply suggesting three issues central to European integration: economic and monetary union, the reform of political institutions, and the problem of enlargement. The autonomy apparent in each essay is a source of this volume's strength; there is no stretch to fulfill an editorially imposed mandate. Yet, taken collectively, the contributions shed a good deal of light on who participates in the European Monetary Union, the future membership of the European Union, and the problems associated with the future growth and development of this unique institution.

The first section addresses the EMU. In a stimulating article that blends economic theory with the constraints of domestic institutions, Geoffrey Garrett speculates about the likely membership in the EMU, using optimal currency area (OCA) theory as a point of departure. Is it possible to predict the likely members based on such a theory? Not entirely, Garrett concludes. In addition to the variables central to OCA theory (degree of economic integration, symmetry of economic shocks), joining a scheme for a common currency will only make sense if expected adjustments are small and if wage demands do not spiral out of control. Expect countries with independent central banks and centralized labor institutions to join the EMU, but do not look for Britain to join anytime soon. France's ardent commitment to membership is harder to explain in terms of probable macroeconomic consequences alone. France seems to have neither the economic prerequisites of OCA theory nor the institutions that Garrett argues render benefits to monetary union.

Christian de Boissieu and Jean Pisani-Ferry address the puzzle of French commitment to EMU. France, they argue, has long sought exchange rate stability, a preference unique

among G7 countries. The reason has not been to control inflation as an end in itself but to hold the exchange rate constant while disinflating as a way to remain competitive vis-à-vis Germany (p. 67). Despite the title of the chapter ("The Political Economy of French Economic Policy in the Perspective of EMU"), the answer is clearly grounded in international politics: "The political logic was that only a low-inflation, stable currency France could pretend to leadership in Europe" (p. 63). The authors maintain that French leaders did not view fixed exchange rates as a "commitment technology" to produce price stability, as is often thought; they saw it as a way to maintain the country's rank as a great power. If anything, the "political economy" of the situation is highly permissive: de Boissieu and Pisani-Ferry note that "there is no evidence of strong private sector preferences in favor or against EMU" (p. 82). In combination with an institutional configuration that favors foreign policy over economic policymaking (p. 64), it is not difficult to see why exchange rate policy has transcended political economy and become high politics in France.

The second section is devoted to "political institutions." Apparently not willing to sacrifice a good article for the sake of specificity, the editors have included three contributions that focus on both political and economic institutions, the former at both the European and the national level. Lisa Martin's chapter deals with the crucial question of the legitimacy of European institutions. She notes that once it may have been possible to think of European integration as an elite-driven process. But as European integration has come to affect important policy areas for more and more citizens, Martin notes that institutions are likely to suffer from a crisis of legitimacy for their lack of representation and accountability in decision-making processes. Interestingly, she believes the answer may not lie exclusively or even primarily in the reform of European institutions, and she gives an excellent account of why many of the so-called reforms of the European Council, Commission, and Parliament may not be as "democratic" as they are billed. And empowering the European Parliament may in the end prove incompatible with a "multi-speed" Europe. (The consequences of what institutional form this multispeed Europe takes are examined in the following chapter by Von Hagen and Fratianni.) The answer, Martin argues, is for national parliaments to heed the Danish example and more aggressively constrain negotiators in Council proceedings. This might lead to some inefficiency in reaching agreement, but the Danish record suggests it would improve efficiency with respect to the implementation of directives.

Michael Wallerstein's article points to the consequences of economic integration on the efficacy of an important domestic institution: the structure of industrial relations and wage setting in Europe. But not quite, since Wallerstein rejects any direct effect. Neither of the traditional arguments for integration's corrosive effects on centralized bargaining really hold water, he maintains. The notion that economic integration destroys monopoly labor's ability to raise wages is fallacious, since this has never been the strategy of encompassing labor institutions in Europe; they instead have negotiated reasonable packages of coordinated wage moderation. And the suggestion that integration has eroded the corporatist "bargain" of wage moderation in exchange for discretionary macroeconomic policies aimed at full employment is fallacious, Wallerstein argues, since whatever incentive unions have to solve their collective action problems continnes to be present even in the absence of such an active fiscal policy. Essentially, the correlation between integration and the decentralization of wage bargaining institutions is spurious. "The most compelling relationship between the decentralization of bargaining and economic integration may be that both are responses to a common problem, poor economic performance since the mid-1970s" (p. 203). This chapter has little directly to do with "Forging an Integrated Europe," but it does provide a sound antidote to alarmist claims that integration is to blame for the decay of wage setting institutions in Western Europe.

The final section addresses enlargement. Two chapters analyze the factors influencing support and opposition to joining the EU. Jonathon Moses and Anders Jenssen try to explain why Norway uniquely among the Nordic countries rejected membership, while Sven Arndt compares Austrian acceptance with Switzerland's decision to opt out. In the Nordic case, Moses and Jenssen hypothesize that attitudes toward the EU can be predicted by an entity's economic relationship with the EU. "The more economically integrated an entity is with the EU, the greater the likelihood that the entity [whether a country, county, or individual] will desire membership in the Union." They do not, however, make a traditional political economy argument. They maintain that "a sense of community may be formed by economic exchanges." Furthermore, "political integration might be built on the cognitive foundations of the interactive community" (p. 215). There is moderate support for the hypothesis, but there is none offered for the mechanism of "community." For example, although Norway was the only country to have all its top five export markets in the EU, it rejected membership. Moses and Jenssen attribute this to the specific nature of Norway's export economy, which is dominated by oil, but no reason is offered as to why a sense of community is stunted by the export of a black greasy substance. In fact, traditional arguments that have little to do with a "sense of community" or "cognitive foundations" are explicitly at play: "The specific nature of [Norway's] exports make it less susceptible to economic blackmail" (p. 221). Also, it is not easy to see what the individual data say about community or cognitive foundations: In each country these "show a very strong relationship between support for the EU and the likelihood that one's personal economy would be strengthened by EU membership" (p. 221). That sounds like a straightforward story based on economic interests of the narrowest sort.

Arndt comes to a somewhat different conclusion in comparing Austria and Switzerland. While a plethora of economic studies suggested both countries would face problems as a result of the Single European Act (SEA), these countries made opposite choices. Arndt attributes the difference in attitude to nonquantifiable considerations. In Austria, voters were concerned lest Austria be isolated from Europe and rendered unable to influence EU policy. In Switzerland, a tradition of neutrality, direct democracy, and concerns regarding sovereignty were nearly "insuperable" issues that tipped the balance away from membership. These factors may distinguish Austria from Switzerland, but the pattern of support within Switzerland, especially heavy cleavages along linguistic and urban-rural lines, may belie patterns of economic interest that the chapter does not discuss in any detail.

What of the eastern enlargement of the EU? Peter Boffnger points to tension in the prospects for eastern expansion. Yes, the EU would provide countries in Central Europe with an economic constitution that would help suppress the power of national producers and their demands for protection. The constraint, however, is the EU budget. The cost of providing benefits through the CAP and regional cohesion policies will be prohibitive and certainly will bump up against the willingness of largely German taxpayers to foot the bill. But, Bofinger argues, delay will only give the opportunity for

resistance to liberalization to grow in Central and Eastern Europe. In Olsonian fashion, the passage of time will allow special interests to organize to demand their share of rents from the state. Nationalism will only make resistance that much more difficult. Bofinger argues that the most prudent response would be to multilateralize the current agreements and enhance policy surveillance, while trying to ensure some degree of predictability for foreign direct investment. Institutions must be designed to consolidate the gains of liberalization without stretching EU resources to the political breaking point.

The editors have wisely let a talented group of scholars develop these themes without imposing an unnatural framework to the volume as a whole. The results overall justify this choice, although the cost is less dialogue among the contributors than might be useful. Even so, the essays make a valuable contribution to our understanding of the political economy of European integration. The combination of formal and informal argumentation, quantitative and qualitative evidence, and political and economic perspectives make a good case that there is much to gain from interdisciplinary study of the European Union.

Foreign Policy Decision-Making: A Qualitative and Quantitative Analysis of Political Argumentation. By Irmtraud Gallhofer and Willem Saris. Westport, CT: Praeger, 1996. 276p. \$69.95.

Collective Choice Processes: A Qualitative and Quantitative Analysis of Foreign Policy Decision-Making. By Irmtraud Gallhofer and Willem Saris. Westport, CT: Praeger, 1997. 212p. \$85.00 cloth, \$27.99 paper.

Rose McDermott, Cornell University

The thrill of decision making lies in the inherent freedom it allows to select preferred choices from among selected options. The agony of decision making derives from not knowing which option will most likely result in the desired goal. The challenge of decision making rests in the internal value trade-offs that must be made between valued alternatives. Analyzing these processes in a systematic way presents complications for any scholar who undertakes the formidable task of understanding the mechanisms by which decision makers make these choices.

Irmtraud Gallhofer and Willem Saris, two Dutch scholars, have produced two volumes that combine qualitative and quantitative approaches to the study of foreign policy decision making and collective choice. Gallhofer has conducted research on political decision making and textual analysis for more than two decades. Saris, a professor of statistics and methods at the University of Amsterdam, has worked on structural equation modeling and measurement procedures in the past. They combine a variety of methodological perspectives—archival research, quantitative analysis, and experimental design—to investigate the effect of various forms and combinations of political argumentation on foreign policy and collective decision making.

Foreign Policy Decision-Making analyzes individual decision making from a decision theoretic perspective. The authors examine the arguments used by individual leaders and politicians to convince their colleagues of particular positions on various issues. Collective Choice Processes seeks to examine the process that occurs when groups of decision makers combine to reach a collective choice. Each volume is divided into two parts, the qualitative analysis of cases and the quantitative analysis. Both books are extremely well

organized and provide useful summaries at the end of each chapter as well as each part.

In Part I of both volumes, the qualitative cases overlap, although the emphasis shifts from the more decision-theoretic approach of individual decision making in the former to the more game-theoretic strategic approach of collective choice in the latter. Both examine the following cases: the initiation of World War I in July 1914 by the Austro-Hungarian Common Council of Ministers; the initiation of World War II by Hitler's Germany; the Cuban Mission Crisis here labeled "the world at the brink of nuclear war" in October 1962; and the Dutch decision on whether to take measures against Indonesia in fall 1948. The collective choice volume also adds the Dutch cabinet's decision to carry out military action in Indonesia in December 1948. Through the parallel demonstration of theory across cases, the authors strive to document the operation of several specific hypotheses concerning the nature of argumentation and decision making. The similarity of cases is redundant if both volumes are studied, but the individual cases themselves provide thorough historical and archival documentation.

The quantitative analysis in both volumes examines different factors. Foreign Policy explores the nature of the decision rules used in political argumentation. The quality of the arguments as well as the factors in the specification of strategies, outcomes, and argumentation rules are analyzed. This volume also provides meticulous appendixes on the construction of decision trees, assessing intercoder reliability, and detecting decision rules from interview sources. An innovative method to determine whether decision rules are universal involves self-administered, computer-assisted interviewing, called tele-interview (p. 140). This procedure collected data from 59 Dutch subjects sampled at random. The quantitative analysis in Collective Choice examines the decision-making process and preference aggregation rules.

The results of these studies are interesting if not surprising. The findings from Foreign Policy teach the more systematic and consistent lessons. The authors uncover certain clear biases in the decision-making process across cases. This finding dovetails with established results from social psychological experiments on individual decision making and gains added credibility from this independent source of confirmation. These biases include the universal attempt to simplify the decision problem and the equally common avoidance of combining estimates of utility or probability with any kind of weighting measure of intensity. Indeed, this kind of simplification is one of the editing processes by which individuals frame the choices they later evaluate for decision-making purposes, according to Daniel Kahneman and Amos Tversky's prospect theory model ("Prospect Theory: An Analysis of Decision Under Risk," Econometrica 47 [1979]: 263-91).

Similarly, according to Gallhofer and Saris, decision makers strive to reduce uncertainty to certainty and seek to reduce complexity in any way possible, manifested especially in their desire to translate complex value choices into more simple ones. Kahneman and Tversky (1979) label the former tendency the certainty effect. In general, decision makers tend to restrict both the range of strategies and the number of consequences considered. This failure to examine multiple consequences simultaneously, often called nonconsequentialism in the economics and psychological literature, is pervasive because of the cognitive difficulty required in undertaking such complex considerations (Eldar Shaffr and Amos Tversky, "Thinking through Uncertainty: Nonconsequential Reasoning and Choice," Cognitive Psychology 24 [1992]: 449-74). Nonconsequentialism often results in magical thinking because leaders can only keep a few consequences in mind at a time; Gallhofer and Saris consistently find that the number of consequences typically considered is two and only rarely exceeds three. Because only a limited number of outcomes are considered, politicians come to believe that the consequences they want to have flow from a decision are the only ones possible. Undesired consequences are simply eliminated from contention by disregard. In this way, efforts to reduce cognitive complexity result in oversimplification, which can lead to disastrous results if no one considers important potential consequences, such as the outbreak of war.

The propensity to limit the universe of strategies and consequences contemplated often results in deferred decisions. According to Tversky and Shafir ("Choice under Conflict: The Dynamics of Deferred Decision," Psychological Reports 3 [November 1992]: 358-61), highly conflictual decisions are more likely to be deferred, and deferred decisions are more likely never to be made at all. Such avoidance results in a de facto return to the status quo, which everyone sought to avoid in the first place; indeed, dissatisfaction with the status quo typically instigates the decision-making process. As Gallhofer and Saris state, "political decision problems are in essence complex value problems with a large degree of uncertainty. Since these problems are so complex, decision-makers intuitively apply a variety of simplifications" (p. 124). Historical examples of the tendency toward simplification, provided in the qualitative cases, are often frightening in their horrific, or near horrific, results.

These volumes make real contributions to the study of foreign policy, collective decision making, and political argumentation, especially in the methodological realm. Many scholars pay lip service to the importance of combining methods to investigate and verify findings in political science, but few actually do so. The obvious reasons are the time and difficulty involved in becoming adept in a variety of approaches. Gallhofer and Saris genuinely strive to combine qualitative and quantitative methods to provide a wider body of evidence in support of their hypotheses. In attempting to appeal to the inclinations of all, however, they may end up alienating many. The qualitative cases are well-chosen and engaging examples, but the quantitative analysis and the emphasis on rules of argumentation or aggregation may prove too complex and specific for some to find useful. Although the qualitative analysis is well documented and organized, the format and findings are repetitive, and those with a penchant for quantitative work may find such demonstrations superfluous. This should not detract from the difficulty and complexity of clearly specifying hypotheses and testing them with a wide array of historical case examples as well as quantitative and experimental analysis. By and large, Gallhofer and Saris accomplish this task in a clear, thorough, and accessible manner.

Ironically, the main limitations of their approach may also result from some of this methodological specification. I will mention three issues in particular. First, the complexity of this analysis would require a great deal of time, energy, and resources in order to replicate or extend either the individual or collective set of findings. It is indeed an accomplishment that the authors were able to undertake such a comprehensive project successfully. This level of investment, however, raises the issue of generalization beyond mere replication. The authors freely admit that they seek to understand the decision-making features of politicians in the Western tradition. The all-too-brief extensions to other nations in the West at the end of *Collective Choice*, including Great Britain, France, Germany, and Belgium, do not allow us to extrapolate strong conclusions to the process of political argumen-

tation and decision making in the United States, much less in non-Western nations.

Second, these volumes provide some compelling evidence in support of specific biases in decision making and certain rules and procedures that govern these processes, but many critical questions lie outside the area of analysis. Most important, although the authors are willing to ask some hard questions about the nature of argumentation and the aggregation of preferences, no clear answers emerge about how or why a particular leader would choose one argumentation strategy over another in a particular instance. Obviously, this makes prediction difficult, and it leaves open on-going questions about the origins of such strategies and preferences. The search for specific decision rules or procedures in this manner is a bit reminiscent of the old studies in social psychology on the nature of persuasion, which systematically altered numerous characteristics of the environment, including the nature of the communicator, message, and audience, all to no systematic avail. It took the emergence of Leon Festinger's more unified theory of cognitive dissonance both to explain and predict the nature of persuasive communication in terms of motivation, not cognition.

Third, limits are imposed by a rigorous methodological design that focuses on some facets of the problem to the exclusion of others. Political argumentation, decision rules, and the aggregation of preferences, however important, do not constitute decision making writ large. It may be important to break decision making down into its constituent parts in order to examine various dimensions fully, but it is crucial to remember that the process as a whole is almost certainly greater than the sum of its components. As with motivation and the nature of persuasion, much of decision making may lie outside the realm of reason, in the domain of emotional conviction or moral passion.

Games Advisors Play: Foreign Policy in the Nixon and Carter Administrations. By Jean A. Garrison. College Station: Texas A & M University Press, 1999. 224p. \$34.95.

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University

When applied to foreign policymaking, "game" frequently signals either an elegant and stylized game-theoretic analysis of nation-states' bargaining strategies or the cynical conclusion of a world-weary journalist. Instead, Jean Garrison seeks to classify the varying "power games" (a term she borrows from Hedrick Smith) that senior foreign policy advisors play as they strive to influence U.S. presidents to adopt their preferred policies.

Games Advisors Play fails to breach the almost impregnable wall that continues to separate studies of presidential national security decision making from other kinds of policy decision making. Yet, it focuses on more than policy substance, drawing on the ideas of such scholars as Alexander George, Irving Janis, John French, and Bertram Raven. At the center of the analysis are three kinds of strategies on which senior advisors rely to move the process toward presidential acceptance of their preferred policies: control access ("structural maneuvers"), influence information flows ("procedural maneuvers"), and shape preferences and perceptions ("interpersonal maneuvers"). To illustrate the tactics associated with these strategies and to trace their effects on presidential decision making, Garrison uses four cases involving U.S.-Soviet arms control policy in the Nixon and Carter administrations. She compares the influence dynamics in the more closed Nixon advisory system to those in the

more open Carter system, highlighting the activities of national security advisors Henry Kissinger and Zbigniew Brzesinski.

Although the study uncovers relatively little new empirically, it brings together much diverse evidence from a range of archival, interview, and secondary sources. The work carefully and systematically traces presidential advisory processes as the United States and the USSR moved haltingly from the promise of SALT in the early 1970s, through the ill-considered "deep cuts proposal" of Carter's first year in office, to SALT II's ultimate demise in the U.S. Senate. In doing so, it makes a strong case for the need to pay attention to the influence of individual presidents, foreign policy advisors, and their interrelationships on specific decisions and policy outcomes.

The analysis is a useful example of the promise—and the complexities—of following the advice of Alexander George and Timothy McKeown to examine decision making with the "logic of 'process tracing'" ("Case Studies and Theories of Organizational Decision-Making," Advances in Information Process in Organizations 2 [1985]: 21–58). Quite clearly, this sort of rich description of how particular decision processes unfolded and who (if anyone) helped drive them can contribute to scholarly understanding and citizen assessment of policy decision making and public officials. Meanwhile, Garrison's inventory of influence tactics and her balanced judgments about their evident effect (on achievement of their wielders' policy goals, on other advisors' influence, on presidential policy decisions) provide important insights into the dynamics both of the cases she examines and of top-level U.S. policymaking more generally.

That said, the payoffs of the process tracing in Games Advisors Play might have been even greater if somewhat more attention had been paid to a handful of issues. As is the case in much of the decision-making literature, the conceptual grounding of the analysis could be clearer. For example, Garrison might have delineated more sharply the contours of advisory and decision processes and the distinctions between them. Similarly, she does not explicitly define the boundaries of presidential foreign policy "advisory systems." More important, the key features of such systems are described in varying ways: as "open," "collegial," and "competitive" (Carter) or as "closed" and "hierarchical" (Nixon). Yet, the degree of openness and the extent of hierarchy in these "organizational structures" (p. 144) would seem to be analytically distinct, if rarely orthogonal, dimensions.

In addition, case selection probably deserved more attention. Garrison notes, in a quite helpful if too brief Appendix, that the "foreign policy instances" she examined were "selected to have enough evidence available to illustrate the relationships among individuals and the group" (pp. 144–5). This strategy runs the risk of overstating the amount of advisor influence by excluding presidential advisory/decision processes that are less well documented or for which relevant materials remain closed. The potential selection bias is largely unavoidable, but perhaps it should have been acknowledged and its potential implications explored.

The causal inferences that one can draw from Games Advisors Play are quite limited, since the research design is "indeterminate" (in the terms of Gary King, Robert O. Keohane, and Sidney Verba, Designing Social Inquiry: Scientific Research in Qualitative Research, 1994). The "main differences" among the cases were "advisory structure" and "presidential leadership styles," as well as "the situation, the members of the inner circle, the time frame, and the political context" (p. 144).

The work also routinely notes apparent shortcomings in

each case it spotlights. Often the weakness is in the advisory process itself. Kissinger, for example, frequently insulated Nixon from diverse and conflicting advice, and the decision to propose deep weapons cuts to the Soviets early in Carter's term (in effect, undercutting the 1976 Vladivostok agreement) was reached without careful consideration of its possible risks. On other occasions, Garrison's assessment shifts to poor policy outcomes (such as the refusal of key U.S. senators to support SALT II). Only rarely, however, are the underlying criteria for these sorts of evaluations made explicit or justified.

A final issue is one that social scientists confront continuously. Should efforts focus on enriching empirical understanding by working, for example, to "identify other factors determining what, when, and how influence tactics are used" (p. 140)? Or are energies more fruitfully directed toward narrowing down the number of dimensions considered to a relative few? An approach like Garrison's promises rich and nuanced tales of advisor intrigue, policy complexity, and the volatility and uncertainty of decision making. At the same time, it complicates (and indeed challenges) attempts to generalize or prescribe. In doing so, it provokes and enhances the ongoing debate.

The Second Nuclear Age. By Colin S. Gray. Boulder, CO, and London: Lynne Rienner, 1999. 205p. \$45.00.

Gregory Paul Domin, Mercer University

Colin Gray's book returns nuclear weapons, as well as biological and chemical weapons, to center stage of world affairs. It shares in the tradition and arguments of Gray's other works (House of Cards: Why Arms Control Must Fail, 1992, and Modern Strategy, 1999). Gray, who teaches international politics and is director of the Centre for Security Studies, University of Hull, takes issue with the complacent attitude of American policymakers that weapons of mass destruction (WMD) no longer play a prominent role in world affairs. It is easy for Americans to become complacent: The Iron Curtain is down, NATO has expanded into Eastern Europe, and the economy continues to grow at unprecedented rates. Gray, however, cautions us that the spread of nuclear weapons (and the absence of a dominant political rivalry vis-à-vis the United States-Soviet Union) are still a problem and may ultimately destabilize the international system and lead to nuclear war.

The Second Nuclear Age (which is defined by the absence of a dominant political rivalry, bipolar or otherwise) is about the meaning of nuclear weapons for national and international security today and tomorrow. "The subject here is both the character of an emerging era and a requirement to define how particular kinds of menaces can be defeated, otherwise neutralized, or simply endured in tolerable terms. The problem is how to cope well enough with a persistently, albeit evolving, nuclear (chemical and biological) future" (p. 1). Gray argues that although the Cold War is over, the world may not be as safe a place in which to live. The good news is that the "first nuclear age" was defined by the bipolar struggle between the United States and the Soviet Union, but the bad news is that WMD still exist in this unipolar world and are still just as threatening today (p. 1).

Throughout the six chapters, Gray covers several themes. First, WMD are a permanent fact of life, whether it be the first, second, or tenth nuclear age. Now that the nuclear genie has been let out of the bottle, there is no turning back. The book is about the problems of coping with the largely unfamiliar challenge of a different—hence "second"—nu-

clear security environment from that in which today's policy-makers were educated strategically. Also, it centers around "the process of trying to cope with a new nuclear age when the conceptual, and much of the material, toolkit derives from a previous period" (p. 6).

Second, "the nuclear challenge to our security does not reduce to a simple menace, even though the eventual emergence of a third nuclear age characterized by a dominant axis of nuclear armed antagonism is likely" (p. 10). That is, there are more players on the world stage that possess nuclear weapons, or the threat of manufacturing them, than at any other time in history. As such, there are more threats to global security in this era than at any other time, which may ultimately destabilize the international system.

Third, although nuclear weapons certainly pose a risk to global security, biological, toxic, and chemical weapons pose a "growing array of threats that are in part at least distinctive in profile from nuclear menaces" (p. 6). Global security changes when these weapons can kill, in effect, millions of people. Such threats, obviously, need to be taken very seriously.

Fourth, the twin themes of "change and continuity" are examined. Continuity is used in the sense that there has been a transition from a first (bipolar) nuclear age to a second (unipolar) nuclear age to a possible third and fourth nuclear age. The discussion of such transitions is conducted against the backdrop of the "implementation of the major cumulative changes for security and strategy that are currently being effected by the arrival of new technologies" (p. 12). The theme of change and continuity may seem schizophrenic, but Gray argues that such a compound theme is not handled well by strategic theorists. He remains cautiously optimistic that this work may aid policymakers' understanding.

Fifth, "deterrence is unreliable" (p. 12). Gray goes to great lengths to argue that nuclear deterrence is inherently unreliable, but he does not argue that deterrence does not work. One need only revisit the roughly forty years of stability produced by the Cold War. Yet, although nuclear deterrence "worked" between the superpowers, it may not necessarily work between nonsuperpowers in the current period. Deterrence in theory is one thing, deterrence in practice is quite another.

Sixth, "among the themes explained in this work is the persistence of the distortion that the lenses of both political and strategic culture and national but parochial interest can wreak havoc upon strategic policy" (p. 12). The end of the Cold War meant the demise of a bipolar rivalry and one set of strategies and gave birth to the responsible state, as contrasted with the "rogue" (a state judged by us to be committed to overturn a settled framework of order that we regard as legitimate) (pp. 12-3), and a new set of strategies to deal with such states. Yet, the Cold War pitted one camp against another in clearly defined terms, but the present era does not offer policymakers such a luxury. Gray, quite correctly asks: What is a "responsible" state? What is a "rogue" state? The character of the second nuclear age is not shaped "solely by the negative attitudes toward nuclear armament dominant in an ever more I-war-minded US nuclear defense community" (p. 13). The post-Cold War period lacks the objectively, or at least the perceived objectivity, of its predecessor.

Finally, the "seventh theme is the Clausewitzian revelation that friction can rule" (p. 12). Gray argues that academics, policymakers, public officials, and the like are always tempted to draw tidy pictures of the future world stage and the solutions to its problems. Such naiveté fails to recognize that coping with a persistently nuclear future may indeed be

uncertain, frightening, and full of risks, that the future course of strategic planning may indeed contain some surprises that may or may not affect our national security. The bottom line is that there is no "one size fits all" solution to the problems of the second nuclear age.

Gray's work offers fascinating insight and information, but it has two major shortcomings, which the author freely admits throughout. First, the analysis relies quite a bit on speculation. Due to uncertainty, there "is no way I can demonstrate beyond reasonable doubt the superior merit in my understanding of behavior toward WMD that is prudent for the second nuclear age" (p. 43). Sadly, the arguments in this book do not rest upon any great conceptual or empirical discovery. They are without paradigm, much like the second nuclear age is without a rivalry.

Second, Gray admits to a preference "for a minimalist theory of world politics... this book is only a minimalist exercise in the theory of international relations" (p. 25). Such an approach is not useful in explaining conflict, particularly one that may be characterized by WMD, at the local and regional levels. In a world devoid of a bipolar rivalry, such conflicts will become the norm, and policymakers will need some place to turn in order to devise prudent strategies in order to address the problem.

Given these obvious shortcomings, Gray's work needs to be read by academics and policymakers alike, not so much for its theoretical insights as for the realization that WMD remain a threat to world security, even though the international stage, and its key players, have changed. The Second Nuclear Age is a good primer for reconsidering U.S. nuclear strategy. Its clear and relatively idiom-free prose makes it accessible to students interested in the threat of WMD in the post—Cold War era.

National Collective Identity: Social Constructs and International Systems. By Rodney Bruce Hall. New York: Columbia University Press, 1999. 392p. \$50.00 cloth, \$20.50 paper.

Mlada Bukovansky, Dartmouth College

International relations scholars with a social constructivist orientation have recently begun to produce empirical work substantiating more abstract formulations of the idea that, in the words of Alexander Wendt, "anarchy is what states make of it." Whereas initial empirical forays focused on relatively narrow cases, books offering more sweeping historical interpretations are now emerging; National Collective Identity is one of the latter.

Hall's core argument is that the patterns of international politics are fundamentally shaped by societal collective identities—how societies conceive of themselves as units. Who "we" are shapes how "we" interact. Societal collective identities vary over time, and such a variation in turn shapes political institutions, state interests, and the principles of interaction among actors in a given international system. Hall illustrates his argument with sketches of three distinct European international systems: the dynastic-sovereign system (which he dates from the Reformation to the Peace of Westphalia in 1648); the territorial-sovereign system (from 1648 until roughly the mid-nineteenth century); and the nation-state system (from the mid-nineteenth century until today). He maintains that each was constituted by different forms of collective identity, different notions of state interests, and different systemic principles of legitimacy. Against both classical and structural realism—the main theoretical foils—Hall argues that state interests are not gleaned as the result of some primal will to power or by assessing objective structural conditions. Rather, we must delve into how societies identify themselves as such, an enterprise that requires a foray into the cultural and ideological dimensions of social life, in order to understand state interests and patterns of conflict and cooperation in the international system.

Hall's general theoretical orientation is compelling; it echoes and dovetails nicely with the work of such constructivist scholars as Michael Barnett, Martha Finnemore, and Alexander Wendt. The distinctive character of this book lies in the execution of the argument and the sweeping historical illustrations mustered in support. Analyses of key historical episodes, including the Seven Years' War, the French Revolution, the rise of German nationalism, the character of nineteenth-century imperialism, and World War I, are used to buttress the argument. The emergence of nationalism and the nation-state system is the central empirical focus. There is much to admire in Hall's confident articulation of historical evidence to support his thesis. He also invokes an impressive army of important scholars from a variety of disciplinesincluding Liah Greenfield, Jürgen Habermas, Eric Hobsbawm, Henry Kissinger, and Michael Mann-to substantiate his theoretical and interpretive claims.

Throughout the book, Hall stresses the shortcomings of realist international relations theory. His substitution of the classical realist notion that state interests are derived from a "will to power" with his own notion that they are instead derived from a societal "will to manifest identity" is an original, if somewhat awkward and contestable, formulation. He also makes a compelling case that there is no underlying structural logic to anarchy and provides many examples of how much can ride on the form of collective identity that shapes a polity. But given the fact that some realists have written quite intelligently on the cases Hall covers, he should directly confront realist interpretations of those cases. The best of such confrontations is found in Hall's discussion of Barry Posen's work on the mass army (pp. 147-59); a few more respectful engagements such as this would have strengthened the theoretical effect of the book among the unconverted.

Hall's argument that changes in societal collective identity explain systemic changes raises some thorny theoretical questions. The most pressing is how a particular form of societal collective identity crystallizes. Hall's notion of collective identity comes close to serving as a catch-all category for any historical contingencies the author finds relevant for a particular case. He notes, for example, that "by a highly complex and historically contingent set of interactions, classes and nations emerged as dominant social actors in the nineteenth century" (p. 22). This gives him the freedom to tell a rich story about nationalism, but there is some sacrifice of conceptual rigor insofar as the forces that crystallized nationalism are "complex and historically contingent."

Furthermore, Hall's model lets him sidestep the collective action problem. It is, he argued, that collective identity produces collective action through socialization of individuals (pp. 37–8), but as rationalist accounts of collective action problems have shown, this proposition can be highly problematic. Just because there is a "we" in the world does not necessarily mean that "we" are going to do anything. Hall's "will to manifest identity" idea suggests that, once societies find their mode of collective identification, social action will follow automatically, but I remain skeptical. Overall, since so much rides on the notion of societal collective identity, it is troubling that the criteria by which we know a collective identity when we see it are poorly specified.

The elasticity of the concept also allows Hall to sidestep

important empirical issues. For example, he argues that he French Revolution generated a national collective identity (pp. 134-51) but neglects the counterrevolution that occurred throughout the period. Large portions of the French population continued to identify themselves in strongly local and particularly Catholic terms, and the aristocracy continued to put forth alternative conceptions of French identity, conceptions which saw a revival under Napoleon (who sought dynastic legitimacy by marrying an Austrian archduchess) and under the Restoration. Thus, collective identity can be contested (a fact that Hall acknowledges but does not pursue far enough), and it is an open question as to how one form as opposed to another becomes the dominant explanatory factor required by Hall's model. In short, the book makes a strong theoretical contribution to the conceptualization of the institutional and systemic consequences of collective identity, but it is less informative about how collective identities are determined or change. The latter issue is treated in a richly descriptive but not theoretically precise manner.

Along similar lines, the theoretical framework does not always generate a convincing explanation of why some systemic legitimacy principles become dominant over others. Presumably, systemic principles are derived from dominant forms of societal collective identity. I was confused to find that "Old Regime statesmen" (p. 269) still had a strong hand in running international affairs immediately before World War I, since Hall earlier stated that the Old Regime was dead and that national sovereignty had emerged victorious as a systemic legitimating principle. Also contestable is Hall's assertion that national self-determination emerged as the victorious principle in the Versailles settlement (p. 274), since its application was limited mostly to existing territorial entities, which suggests that territoriality was still firmly entrenched as a systemic principle. In fairness, Hall's understanding of history is sophisticated enough to recognize these tensions, but his theoretical model is too blunt an instrument to guide us in sorting them out. Closer attention to intellectual history and the ideological debates of the periods studied would yield further insight into how societal and systemic legitimating principles were articulated at particular times, which would then allow us to model tensions and variations in societal collective identity and systemic legitimacy more precisely.

Readers versed in the historical literature of any of the eras covered in this book will undoubtedly find things to quibble about in terms of interpretation, emphasis, and choice of sources. The writing style is somewhat bombastic at times and is peppered with colorful indictments of hapless aristocrats, incompetent Bourbons, and greedy capitalists. This can be lively or irritating, depending on one's disposition toward the targets. Readers without a solid background in social theory will find the theoretical chapters difficult going. None of the criticisms raised here should overshadow the fact that this is a major contribution to international relations scholarship. Hall has broken new ground in establishing the empirical credentials of constructivist approaches. His historical accounts resonate with compelling insights, and one can only hope that others will follow his pioneering lead.

Theories of International Regimes. By Andreas Hasenclever, Peter Mayer, and Volker Rittberger. New York: Cambridge University Press, 1997. 248p. \$59.95 cloth, \$18.95 paper.

Lisa L. Martin, Harvard University

As the authors explain in their acknowledgments, they began writing this book as a review article on modern theories of

international regimes and institutions. It grew well beyond the bounds of an article-length treatment of this vibrant area of study. The result is a comprehensive review of alternative approaches to studying regimes, one that should prove highly useful for teaching graduate seminars or for researchers who want a quick source on recent developments. In some ways, an article would have been more effective, as the luxury of pages allows the authors to draw distinctions that at times are too fine and to discuss at length less worthy contributions. Nevertheless, this is a valuable survey of the state of the field.

The analysis of international regimes is divided into three bodies of work: interest-based, power-based, and knowledgebased theories. Interest-based theories focus on problems of market failure or strategic dilemmas attached to particular issues in order to specify the role and influence of regimes. Power-based theories, which concentrate on problems of multiple equilibria, bargaining, and the threat of conflict, argue that power determines the choice of regime. Knowledge-based theories depart from or add to the rationalist assumptions of the other two approaches, according to the authors, to illuminate how ideas, persuasive arguments, and identities interact with regimes. The analysis of each approach focuses on its assumptions and internal logic rather than empirical studies of regimes. One of the most valuable contributions is the identification of logical problems within each approach, although serious logical inconsistencies are not always adequately distinguished from relatively minor problems of imprecision.

The primary value of this book is its thorough and detailed summary of existing approaches to the study of international regimes. Those familiar with the field may find that the summary of knowledge-based approaches contains the most new information. The authors distinguish between weak and strong versions of cognitivist theory. The weak perspective shares much with the interest-based and power-based approaches, such as the assumption of rationality and a positivist approach to designing research, but endeavors to add perceptions, ideas, and learning to the standard rationalist accounts. In this respect the potential for theoretical progress seems high, since rationalist models of incomplete information provide a central role for ideas and beliefs. Moving to a more careful analysis of the sources of such ideas and beliefs therefore requires no fundamental changes to the rationalist approach and could yield substantial new insights.

Strong cognitivists, in contrast, reject the assumption of rationality. They may see states as role-players rather than expected-utility maximizers, relying instead on a "logic of appropriateness" (p. 155). From this perspective, institutions do not merely regulate actors; they also constitute them. Strong cognitivists therefore focus on how institutions construct identities at a deep level. The discussion of strong cognitivism, however, reveals one weakness of the analysis. The difficulty is that rationalism and positivism are conflated. According to the authors, a strong cognitivist rejects both the assumption of rationality and positivism, but they suggest implicitly that the two are inseparable, or perhaps even identical. Yet, many analysts of identity in international relations—those who go by the label "constructivists"—are committed to challenging the rationality assumption while continuing to use a positivist approach to develop and test hypotheses. Similarly, it is possible to imagine the use of game-theoretic models to interpret behavior rather than as a source of testable hypotheses. Keeping rationalism and positivism distinct is essential to a clear analysis of alternative theories, and conflation of the two could lead to substantial confusion.

The original contribution to the study of regimes comes in

the final chapter, where the authors argue (somewhat tentatively) in favor of a synthetic approach, as opposed to continuing to sharpen differences among theories. Interest-based and power-based theories could be synthesized by using the analysis of power to make claims based on interests more contextual. Rationalism and weak cognitivism might be complements, with cognitivism providing some of the necessary inputs, such as preferences and beliefs, for a rationalist model. Rationalism and strong cognitivism, in contrast, show little potential for synthesis. This seems particularly true if the authors are correct that strong cognitivists reject positivism; not all of them do, however.

Few would deny that the clash of paradigms that has characterized much work on regimes has been less than fruitful. The desire for a synthetic approach is strong, and pluralism among those who study regimes is surely a good idea. Nevertheless, careful analysis of the underlying assumptions of alternative theories is necessary before making a firm commitment to synthesis. Theories that rest on incompatible underlying assumptions cannot productively be combined, as the authors acknowledge in their treatment of strong cognitivism. For example, interest-based and power-based theories both assume rationality, but beyond this common bond the assumptions often diverge significantly. Interest-based theories focus on the problem of incomplete information in international politics, the possibility of market failure that this introduces, and the role of regimes in enhancing transparency. Power-based theories, in contrast, see information as not very problematic. The role of regimes, therefore, is to institutionalize a particular pattern of behavior that works to the benefit of the most powerful state rather than to provide information. Any attempt to meld theories that have such radically different assumptions about information is as likely to lead to incoherence as to a unified, progressive body of

Studies of international institutions and regimes have flourished over the last two decades. Ongoing theoretical and empirical research, now reaching beyond international political economy into the study of international security and social issues, indicates that this agenda will continue to be productive. Differences in assumptions and implications of alternative theories are just now being specified in such a way that they can be directly tested. In this setting, the authors provide a good guide for those new to the field about where it now stands. Their agenda for future directions, which focuses on attempts to synthesize existing theories, is worthy of consideration but requires more skeptical analysis.

Identities in International Relations. Edited by Jill Krause and Neil Renwick. New York: St. Martin's, 1997. 220p. \$65.00.

Resolving Identity-Based Conflict in Nations, Organizations, and Communities. By Jay Rothman. San Francisco: Jossey-Bass, 1997. 195p. \$28.95.

Martin O. Heisler, University of Maryland

To say that these two books deal with identity tells very little about them. In the 1990s alone, literally thousands of books and a vast number of articles were published with such a focus in English alone. The term is common in the vocabularies not only of social scientists, psychoanalysts, and students of literature but also of political activists and politically and culturally engaged people in most of the West; and it is increasingly common in political and psychological discourse elsewhere. In addition to the myriad scholarly formulations of the term (is it a concept?), more and more people are

becoming conscious of "identity" in their lives, talking about it, and sometimes basing their actions on such consciousness. Each discipline, theoretical posture, political orientation, and cultural situation gives it a different twist; indeed, it would seem that each user can choose her or his meaning. Identity is essentially subjective, both as a phenomenon and as a concept, and, therefore, there are few criteria for judging the legitimacy of any particular usage. In sum, two facts about identity have become clear in recent years. First, it is increasingly prominent in academic work and general political and cultural discourse. Second, and not unrelated, it exemplifies conceptual stretching of the sort Giovanni Sartori cautioned about in this *Review* 30 years ago: Using it more and more widely leads to saying less and less with it.

But if the situation is serious, it is not hopeless. There is some intersubjective agreement on conceptual boundaries. A growing number of scholars have been meeting recently, in print if not in person, in diverse but overlapping circles of intellectual exchange. Conversations based on mutual understanding and even efforts to work cumulatively are increasingly evident. Some of this work, especially in international relations (IR), is empirically grounded or at least suggestively illustrated, notwithstanding the tendency of many writers to use postpositivist and/or postmodern language. The areas of overlap include emphases on the growing importance of collectivities based on ethnicity or nationality, religion, or other cultural or ascriptive traits in IR. Not all writers argue that the significance of states as actors in world politics is declining, but most accord them relatively less central roles.

Jay Rothman and the contributors to the volume edited by Jill Krause and Neil Renwick demonstrate ways identity can be harnessed for research and theory and, for Rothman, intervention in the "real world." In both books, identity denotes a collectivity or solidarity group. Such collectivities are treated most commonly as actors in politics; in much of Rothman's work and in all the essays in *Identities in International Relations*, these are actors in regional or general world politics and are seen as increasingly important challengers of the primacy of states as actors. The two books share the virtues of relative conceptual clarity, theoretical relevance, and connection to issues that matter for people, peace, and order, but they have little else in common. Their purposes and styles are fundamentally different, and they reflect very different ways of thinking.

The Rothman book is a graceful amalgam of his extensive and varied experience in conflict-management, theoretical consciousness, and personal values. He wants to advance prospects for cooperation and peace across a wide range of conflict-ridden situations, from small groups and localities to societies and international relations. Threats to identity are at the root of the general insecurities and hostility that energize parties to the most intractable conflicts. If the deep underlying elements in such conflicts can be identified and brought to the surface, then perhaps some of their elements can be harnessed for cooperation. Rothman plots the path to cooperation through what he terms "the ARIA framework": antagonism, resonance, invention, and action. He uses musical metaphors throughout and discusses the dynamics of his framework in terms of "the ARIA Quartet." (This often tedious, extended, and not always consistently used metaphor is one of two serious stylistic flaws in an otherwise wellpresented argument and well-written book. The other is a penchant for relegating important conceptual points and qualifications to endnotes.)

Rothman differentiates identity-based conflicts from those centered on resources and argues that such conflicts are not only very difficult to manage but also often difficult to recognize. They may be misdiagnosed as disputes over material or other issues. Identity revolves around concerns related to dignity, the need for recognition, opportunities for participation, and cultural distinctiveness. (He recognizes that this is a forced dichotomous distinction appropriate only for heuristic purposes.) Surfacing the roots of antagonisms beneath perceived threats to individual and collective concerns with identity is a crucial first step in the ARIA process. The next step involves "inventing" ways of transforming elements that have given rise to antagonism into cooperative dispositions. This is followed by the "reflexive reframing" of the conflict, so that "the identity needs of all sides" can be taken into account, leading to "resonance." Finally, once reframed, the conflict can be directly tackled through appropriate action. Facilitators are crucial at all stages but particularly in the invention phase.

All this may seem both commonsensical and very idealistic. But what Rothman is attempting to do, if I read him correctly, is to move confrontations to negotiations and negotiations to the explicit confrontation of conflicting cognitive and cultural perspectives. It is then that intelligent and well-meaning intervention (facilitation, rather than mediation) can manage conflicts. If there is a flaw in this strategy, it follows from an implicit assumption that what appears to be a logical progression from the first to the last A in the ARIA process can be translated into a progression in action. Like Marx's assumption that moving from class consciousness to class-based collective action is likely because it is logical, for Rothman the temptation to harness a compelling desire to manage conflict to lessons derived from some specific successes in conflict management to a general formula may well bump up against disconnections between intellectual awareness and practical calculations of interest. Unfortunately, it is hard to know under what conditions the ARIA process is likely to work or not.

The volume edited by Krause and Renwick is a coherent, harmonious collection that shares a theoretical focus but is remarkably varied in content and point of view. The focus is on identity at the level of nation. Beginning in the editors' introduction and evident in most of the ten chapters that follow, national identity is transmuted into, or used interchangeably with, nationalism. This would be a source of puzzlement or irritation were it not smartly made into a theoretical tool at the very outset. The argument begins with the problematization of society in an era of penetrated boundaries and globalization. If the boundaries of society and state can no longer be assumed to be coextensive, how do we find the body politic? Most of the contributors test one or another iteration of the notion that national identity and the bounding of the polity in national(istic?) terms is the emerging answer.

It is not possible to render the arguments of the contributors in a fair and discrete fashion in a brief review. The concerns with forms and meanings of large-scale identity in contemporary IR theory and practice are addressed in both general thematic chapters (e.g., the "inside/outside" divide, globalization, ethnicity, gender, and citizenship) and essays with specific area or regional focus (the Middle East, Nicaragua, Europe, sub-Saharan Africa). Every contribution tests fresh ideas about the identitive or ascriptive foundations of current or emerging polities. These are interesting and often provocative but seldom without difficulties. Most of the difficulties stem from the sometimes casual treatment of existing theoretical arguments. Thus, before we can demote the nation-state as an entity privileged in IR theory and practice and supplant or even supplement it with large-scale collectivities based on identity, we have to dismantle international law, organizations, and much else. And before we can put such collectivities on center stage, we have to come to grips with the manifest tendency of many or most of them to strive for nation-state or nation-state-like qualities.

Some normative and practical problems lurk beneath the surface of schema that rely on identity-based group actors. How is membership in the group determined? Who chooses its spokespersons or representatives? What, if any, opportunities do members have to exit? How and by whom is solidarity inside the group enforced? Such problems are endemic to institution-design approaches that rely on group actors based on collective identity or cultural affinity. Thus, the ARIA process may produce agreements, but the stability of those agreements may be jeopardized by divisions inside the group actors. This has clearly been the case in the "settlements" reached in the 1990s in the Palestinian-Israeli, Northern Ireland, and Basque conflicts. In each instance, (predictable) divisions inside one or more of the parties challenged the legitimacy not only of the outcome but also of the process by which it was achieved and, in some cases, the legitimacy of those who bargained on behalf of-in the name of—the collectivity. These concerns are also apposite for the identity-based solidarity contemplated by the contributors to Identities in International Relations.

On balance, these are both worthwhile contributions to the growing literature on cultural and identitive aspects of IR. They are thoughtful, informative, readable, and, perhaps most valuable, arguable.

International Relations in a Constructed World. Edited by Vendulka Kubálková, Nicholas Onuf, and Paul Kowert. Armonk, NY: M.E. Sharpe, 1998. 214p. \$59.95 cloth, \$25.95 paper.

Mark Peceny, University of New Mexico

A decade ago, Nicholas Onuf introduced constructivism to the international relations field in his path-breaking book, World of Our Making (1989). Since then, constructivism has established itself as a serious challenger to the rationalist approaches that dominate the discipline. Many scholars have traveled the constructivist path in recent years, but they have not followed the specific trail cleared by Onuf. International Relations in a Constructed World attempts to place Onuf's vision at the center of the disciplinary debate by making it more accessible, situating it as an approach that avoids the extremes of positivist rationalism and postmodernism, and demonstrating its utility for explaining contemporary issues. The book accomplishes these tasks. Yet, because the contributors have not systematically engaged the most prominent mainstream constructivist scholarship in the contemporary debate, this volume misses an opportunity to demonstrate the value of Onul's approach relative to other constructivists.

The most important contribution is Onul's "User's Manual" for constructivist scholarship (pp. 58–78). His original formulation in *World of Our Making* is a rich yet demanding work of abstract political theory that many scholars of international relations have found difficult to understand and/or use. Onul's user's manual provides an excellent summary of the central points raised in his book and presents the approach in a transparent and accessible fashion.

The editors' introduction and theoretical chapters by Kubálková and Gould join Onuf in clearly positioning constructivism as a third path between mainstream international relations and radical accounts. Kubálková's fine essay adds the nice twist of claiming E. H. Carr, a member of the realist canon, as a founder of constructivism (pp. 25–39). Hamman

(pp. 182-91) argues that constructivism is compatible with modern natural science theories that emphasize emergent properties of systems, nonlinearity, indeterminacy, conditionality, complexity, and chaos.

All these theoretical contributions reflect a central tendency of Onur's original formulation. In staking out the middle ground, the authors in this volume are as concerned with postmodernists and other radical theorists as with scholars in mainstream international relations. Indeed, they are in many ways more completely engaged with the participants in the "third debate" than with mainstream realist and neoliberal approaches (pp. 13-4). Since Onuf was crafting a broad treatise drawing from and commenting upon political philosophy, childhood development, linguistics, and other fields in addition to international relations, this connection with a variety of "post-" approaches places his work in a unique position in the center of a broad debate about social theory. In the international relations field, as practiced in the United States at least, postmodern and other radical approaches have been more marginalized in the scholarly debate. For this reason, the "centrist" position of this volume may not be recognized as centrist by mainstream scholars unconcerned about how constructivism allows the adoption of the insights of postmodernist thinking without abandoning the concept and practice of social science.

This way of framing the debate becomes somewhat problematic in the context of the evolution of constructivist thinking in mainstream international relations. Instead of Onuf and his collaborators, it is Finnemore, Katzenstein, Ruggie, Sikkink, Wendt, and others who have brought the constructivist approach into the mainstream journals and debate. These scholars examine puzzles that realist or neoliberal theories cannot explain. They generally do not seek to overturn the established traditions in their entirety. Indeed, many hope to construct a more liberal international system. Their scholarship places greater emphasis on such nonmaterial variables as culture, national identity, and international norms than is the case in most mainstream scholarship. Many are explicitly positivist in their methods. In sum, constructivism has become a more visible and influential approach precisely because it has become more mainstream.

The most visible contemporary advocates of constructivism receive little systematic attention in this volume. Kubálková, for example, distinguishes among hermeneutics, postmodernism, poststructuralism, dialectical approaches, critical theory, world systems theory, and numerous branches of Marxist thought, but he does not distinguish among different types of constructivist thought (pp. 42–3). Gould discusses the relationship between Onuf's work and that of Wendt (pp. 84–91), but he does not examine how Onuf's work speaks to a wider range of mainstream constructivist accounts.

The failure to engage more mainstream constructivist approaches becomes especially problematic in questions of method. This volume contains chapters by Kowert (pp. 101-22), Prugl (pp. 123-46), and Simon (pp. 147-69) that clearly demonstrate how, in contrast to postmodern approaches, constructivism lends itself to systematic investigation of the empirical world. It is difficult to know from these chapters, however, how Onur's brand of constructivism generates a method distinct from positivism, given that all the case chapters seem to follow in the best positivist traditions of Alexander George's structured-focused case study method. This is especially true of Kowert's essay on British national identity and the Suez Crisis, which spells out clear, explicit, and falsifiable hypotheses and then systematically examines the documentary record to provide confirming and/or disconfirming evidence. Constructivism has become popular in the

discipline, in part, because it privileges the use of case studies to understand politics, but should these case studies reflect a unique interpretivist method or "small n" positivism? This volume leaves the question unanswered.

Onul's vision of constructivism contains many insights not fully appreciated by more mainstream constructivist accounts. One is the importance of linguistic theories that show how the rules governing agents and social arrangements are fundamentally shaped by the language we use to express them (pp. 65-9). Another is how "rules yield rule" that inevitably privileges some actors over others in all social arrangements (pp. 74-7). The failure to address systematically the differences between Onul's vision of constructivism and mainstream varieties leaves these and other valuable contributions somewhat hidden. This will limit the influence of the book to some degree. Nevertheless, the essays do an excellent job of making constructivism accessible, of demonstrating that both rationalist and postmodern approaches miss crucial aspects of the social and political world, and of revealing that constructivism provides a systematic, scientific method for understanding the world.

The Enigma of 1989: The USSR and the Liberation of Eastern Europe. By Jacques Lévesque, trans. Keith Martin. Berkeley: University of California Press, 1997. 267p. \$35.00 cloth, \$19.95 paper.

Mark Kramer, Harvard University

Few developments in the twentieth century were more dramatic than the abrupt and largely peaceful collapse of East European communist regimes in 1989. For roughly forty years, the Soviet Union had maintained a bloc in Eastern Europe through the use or threat of military force. When Mikhail Gorbachev took office as general secretary of the Soviet Communist Party (CPSU) in March 1985, he displayed no sign of wanting to make fundamental changes in Soviet-East European relations. Yet, not five years later, when it became clear that Gorbachev would not rely on military force to prop up orthodox communist rulers in Eastern Europe, the whole Soviet bloc disintegrated, and noncommunist governments came to power. Soon thereafter, in October 1990, Germany was reunified as a member of NATO. This sudden transformation was all the more remarkable because it occurred with almost no violence.

The upheavals of 1989 have been chronicled and analyzed at great length. A vast number of books and articles on the subject have been published, and many more are likely to appear. Most of the literature covers events in a particular East European country (especially East Germany), tracing the emergence and growth of popular unrest and the downfall of the communist regime. This literature has given us many insights into the political and social developments that undermined communism in the region. What is not as well understood is the fundamental change that occurred in Soviet policy. Although numerous scholars and journalists have attempted to explain why Gorbachev ended Soviet hegemony in Eastern Europe, much of this work is inconclusive. Some argue that the Soviet Union was forced to give up its hold on Eastern Europe because of economic difficulties, relentless pressure from the United States, domestic political maneuvering, or some combination of these factors. Others contend that the reason was a desire to improve relations with the West. For the most part, these explanations have been inadequate or unconvincing. The motivations behind Gorbachev's actions are more complex than often alleged.

Jacques Lévesque's book, which first appeared in French

(1989, La fin d'un empire: L'URSS et la libération de l'Europe de l'Est, 1995), provides a far more complete and nuanced analysis of Soviet policy in Eastern Europe at the end of the 1980s. Lévesque, a political scientist at Quebec University who has written widely about the former Soviet Union, rightly points out that the collapse of communism in Eastern Europe was not at all surprising once it became clear that Soviet leaders would no longer use military force to keep orthodox regimes in power. The periodic revolts against communist rule and Soviet hegemony—in 1953, 1956, 1968, 1970, 1976, and 1980–81—left little doubt about the fragility of the East European regimes. What was greatly surprising, however, was the drastic change in Soviet policy. "Soviet foreign policy [until 1989] had always been at its most rigid and intolerant in relations with Eastern Europe" (p. 1). The Soviet Union's "permissiveness" in 1989, Lévesque argues, is therefore a "great enigma" that must be explained.

In a sophisticated analysis drawing in part on Kenneth Jowitt's characterization of Leninist regimes, Lévesque persuasively argues that when Gorbachev took office he had no intention of presiding over the dissolution of the Eastern bloc. His main priority was to redress his country's domestic problems. Over time, as Gorbachev increasingly realized that economic growth would require diverting resources from the military, he began to carry out major changes in Soviet foreign policy, including toward Eastern Europe. By 1989 Gorbachev's aim was to "transform the international order in Europe by a controlled overcoming of the continent's division," a process that "demanded a gradual democratization of Eastern Europe" (p. 3, emphasis in original). Lévesque overstates his case here somewhat—the main rationale for Gorbachev's policy was simply to forestall violent upheavals in Eastern Europe, which, if they had erupted, would have derailed his whole reform program—but Lévesque correctly emphasizes that "Gorbachev and his team believed that a reformed socialism in Eastern Europe was viable. They were convinced that by taking the initiative in democratizing these regimes, reform-minded Communist leaders could retain control of the process" (p. 3).

Lévesque's analysis is admirable not only because of its depth and richness but also because it gives due weight to the role of contingency ("improvisation, contradictions, and the absence of clear choices") in the events of 1989. Lévesque rejects the simplistic notion that the changes in Soviet policy were all inevitable. Everything seems inevitable in retrospect, but the reality is always more complex. The book captures that complexity very well.

The author is especially cogent in his depiction of Gorbachev's ambivalence in 1989. On the one hand, Gorbachev pressed for and countenanced drastic reforms in Eastern Europe that went far beyond anything that would have been tolerable under previous Soviet leaders. At each critical stage when the Soviet Union could have halted the changes engulfing Eastern Europe, Gorbachev chose instead to expedite events. By staunchly ruling out any use of force in the region, even when communism began disintegrating, the Soviet leader ensured himself a favorable place in history. On the other hand, Gorbachev often tried to back away from the full implications of the processes he unleashed. This was especially noticeable in late 1989, when he realized that the upheavals in Eastern Europe had sparked the outright collapse of the socialist bloc, not just a transition to reform communism. According to Lévesque, "Gorbachev's team first tried to deny the depth of the changes that were taking place and then tried to rationalize them in terms of their own emerging social-democratic view of the world" (p. 3). When Gorbachev finally tried to reassert control of events, he found it was too late. Having been deprived of "the levers he needed" to achieve the "greatest benefits for the USSR" (p. 3), he reluctantly acquiesced in the dismantling of both the Warsaw Pact and the Council for Mutual Economic Assistance.

The Engma of 1989 is impressive, but a few important problems arise. Chief among these is the author's vagueness about certain key concepts, especially his references to the "social democratization" of the CPSU. Lévesque never clearly defines this process or indicates how far he believes it would have gone. Because the notion of social democratization plays such an important role in what he calls the "ideology of transition" (a concept used to explain Gorbachev's new policy in Eastern Europe), one would have expected a far more careful and sustained treatment.

Another shortcoming is the limited range of sources consulted. Good use is made of a handful of crucial Soviet documents and a number of important memoirs by former Soviet officials, and the author also interviewed many of the former policymakers in Moscow. Yet, he neglected to use the large volume of other declassified materials in Moscow that are now available at the former CPSU archives and at the Gorbachev Foundation. He also failed to draw on some extremely important items that have been published in Russian journals (Istochnik, Svobodnaya mysl', and so on) and in document anthologies. Furthermore, Lévesque made almost no use of memoirs by former East European officials and no use at all of the huge collections of declassified materials in the East European archives. (The tiny number of documents and memoirs that he does cite from Eastern Europe were translated for him by research assistants.) It is a shame that Lévesque was unable to make much greater use of the rich archival holdings and memoir literature in Eastern Europe, which would have contributed a great deal to his analysis, especially his chapters about Soviet policy toward individual countries.

Fortunately, these drawbacks are outweighed by the book's many strengths. Lévesque provides a level of detail and nuance impossible to achieve before key documents (the ones he consulted) were released and memoirs were published. Although several earlier works on the subjectnotably Charles Gati, The Bloc That Failed: Soviet-East European Relations in Transition, 1990; Ronald Asmus et al., Soviet Foreign Policy and the Revolutions of 1989 in Eastern Europe, 1992; and Alex Pravda, ed., The End of the Outer Empire: Soviet-East European Relations in Transition, 1985-90, 1992—offered valuable preliminary assessments, Lévesque goes well beyond these books. Much more can still be done, especially with the continued outpouring of memoirs from both Eastern Europe and Russia and the enormous amount of declassified documentation that Lévesque failed to examine. Some of his comments about Soviet policy toward specific countries, as well as a few of his broader arguments, are bound to be contested. Even so, this book is, and will remain, an excellent starting point for anyone seeking to explain (or at least understand) one of the most momentous political events in modern history.

Fences and Neighbors: The Political Geography of Immigration Control. By Jeannette Money. Ithaca, NY: Cornell University Press, 1999. 248p. \$39.95.

Leah Haus, Vassar College

How do we explain variations in immigration control policy both among advanced industrialized countries and within these countries over time? Jeannette Money takes on this question in her ambitious book, Fences and Neighbors. Much of the literature that considers the sources of immigration policy is country specific and looks at factors at the national level of analysis (e.g., concepts of citizenship and national identity, economic recession) to explain patterns of immigration policy. Money criticizes this established literature and goes farther. She seeks to develop a generalizable argument to explain variations in immigration control policy across space and time. Pointing to the very uneven geographic concentration of immigrants within receiving countries, Money stresses that to explain a country's immigration control policy one needs to begin at the local rather than the national level.

Money develops two related arguments to address variations in immigration control policy. First, she seeks to explain societal preferences, which she argues are determined by conditions in local communities, such as unemployment rates, capital mobility rates, and rates of immigration increase. Second, she explains why and when these local societal preferences become translated into a country's national immigration policy. A local community's preferences will be added to the national political agenda when the local region is a swing district and perceived to be critical to maintaining or retaining a national electoral majority. Thus, economic distress and unemployment in a local community with a significant portion of immigrants will generate a move to restrictionism at the national level if the district is a swing district. Money's overall argument is that "local conditions drive local preferences, but national politicians are driven by national electoral incentives to play or not to play the 'immigrant card' " (p. 10). She tests this argument on three countries the United Kingdom, France, and Australia and focuses on the 1960s and 1970s.

Fences and Neighbors makes an important contribution to our understanding of immigration policymaking and presents a refreshing addition to the established scholarship. The book is an admirable attempt to move beyond the unique and develop a generalizable framework for analysis. Money presents an original argument on why and when local community preferences for restrictionism are translated into policy at the national level. She provides empirical data from the three case studies to support this part of the argument.

The difficulties in developing and testing a generalizable argument that can simultaneously explain variations in policy between and within countries over time are apparent in the book. The focus is on policy changes in the direction of restrictionism, and the arguments on the sources of restrictionism are tested on a quite limited period (the 1960s and 1970s). One is left to wonder whether the causes of restrictionism may have changed during the course of the twentieth century.

The book gives much less attention to developing and testing arguments that explain policy changes in the directions of openness. For Money, the sources of openness are limited to business firms (pp. 53-6). Her argument leads one to expect a policy change in the direction of openness when there are tight local labor markets in swing districts, but the book does not rigorously test this argument. A number of pertinent case studies in the twentieth century could have been explored, and one is left to wonder whether business firms that were not in swing districts also might have sometimes influenced national immigration policy. Moreover, Money's focus on business firms to explain openness sounds plausible for earlier decades in the twentieth century, but it seems inadequate for a full explanation of changes in the direction of openness in the late twentieth century. For example, France has instituted various amnesty programs,

most notably in 1981-82 and 1997-98, and the United States instituted an amnesty program in 1986. People regularized in amnesty programs are documented in national statistics under the "annual per capita gross flow of legal resident aliens," the measure Money uses to operationalize immigration control policy (pp. 22-4). At the local community level, societal preferences in New York City have continued to be relatively open to immigration in comparison with Los Angeles. Although these openings are narrow, they are nonetheless significant, and it is likely that an explanation needs to look beyond business firms to other actors, such as civil rights groups and immigrant associations. To balance the arguments on the sources of restrictionism, as the author concludes, future research could give more attention to the sources of openness so as to address thoroughly variations in immigration control policy. Fences and Neighbors is an ambitious book that gives a thought-provoking argument on why and when countries move to restrictionist immigration control policies. It is clearly required reading for scholars of immigration policy.

Ethics and Authority in International Law. By Alfred P. Rubin, Cambridge: Cambridge University Press, 1997. 206p. \$59.95.

Kenneth W. Thompson, University of Virginia

Historically, international law has participated in most of the great debates in normative thought. Yet, surprisingly, in the last 25 years the role of lawyers has been marginal. The dominating thinkers tend to be political scientists, such as Hans J. Morgenthau, the theologian Reinhold Niebuhr, and diplomats, such as George F. Kennan. As for international lawyers, Charles de Visscher, author of *Theory and Reality in Public International Law* (1957), stands out.

Alfred Rubin of the Fletcher School of Law and Diplomacy joins this group. His starting point is a view with which Judge de Visscher would have agreed. International law must comprehend the obstacles to no less than the arguments for international law. Universal crimes do not assure universal jurisdiction for war crimes or human rights. Despite Nuremberg, there has been no shortage of wars in the past half century. Intellectual models or ontologies of academics are often based on law that is unconnected with the realities of authority and predictable state practice.

Having stated his assumptions, Rubin goes on to evaluate the historical forces and major philosophies, such as naturalism and positivism, and the interrelations of reality and theory. These sections knit together broad concepts and practical realms of international law. The last three chapters concentrate on theory and practice, interdependence, the twenty-first century, and conclusions. They bring to completion a rather admirable review of a large and complex subject.

The great strength of the volume is the author's single-minded effort to explain the purposes and problems of moral and legal reasoning. Rubin seeks to clarify what each school of thought comprehends. His pedagogy sometimes inhibits clear and simple prose, and the book is not easy reading for the uninitiated. Rubin sometimes falters in doing justice to contending viewpoints, and translating complex ideas from one sphere to another, from law to political theory, is a problem. Finally, this is a textbook on types of law—positive, divine, natural, and eternal—that bespeaks a familiarity with classical and medieval theorists. It may be that Rubin's mastery of different concepts of law in their historical setting is his single most valuable contribution.

Rubin's approach assumes that the reader is at least

minimally familiar with the earlier periods in the history of international law. The author gives an account of his presentation in 1986 to a group convened at Notre Dame University by the Carnegie Council on Ethics and International Affairs. In that paper, he set out his conclusions in a "positivist" model that was meant to reflect the realities of international society addressed in particular to human rights. At the same time, he sought to explain the jurisprudential assumptions on which his paper was based, which were at odds with those of many in his audience. His model conflicted with the moral "naturalist" model dominant in most of the scholarly literature and in the minds of the majority of his audience. When the question of publication came up, it was clear that his paper would not suit the editor of the projected conference volume. After a similar experience at another conference, Rubin concluded that his subject required book-length treatment, and the present volume is the result.

As one reviews the international law literature on human rights as well as on intervention, divisions of this type are all too familiar. Scholars approach the subject with crusading zeal either to substantiate the legal basis for their conclusions or to overturn the argument that the realities of international society support an interventionist approach to human rights. In this respect, violations of human rights are reminiscent of crimes against humanity in the international law of Nuremberg. While Rubin may exaggerate his position as a lone voice defending the positivist viewpoint, his circumstances are familiar in the history of international law. In the 1930s and 1940s, dissenters to what was then the dominant reformist view of international law made similar complaints. They included Morgenthau and Nicholas Spyknard of Yale. Thus, precedents exist for the beleaguered defender of a minority viewpoint in international law scholarship. Although the situations in the 1930s differ from those of 1986 to 1987, the criticisms in both instances are of the failure of the reformers to come to terms with the realities of international societythe more things change, the more they remain the same.

Ethics and Authority makes a contribution to the history and theory of international law, especially in reviewing widely held assumptions that cry out for reexamination. Neither general international law nor the pertinent treaties qualify the United States, for example, to apply such rules to foreigners acting within their own countries. Yet, intellectual models for twentieth-century world order invoke these assumptions not only as they reflect different aspects of culture and definitions of law but also whether they relate to the realities of "authority." Rubin finds this practice demeaning to the law, its influence on civilized behavior, and subversive to the Yugoslavia and Rwanda cases. As for international criminal courts, the U.N. Security Council, when it fails to invite independent analysis for judging their legal capacity, repeats the same error. None of the international tribunals so constituted have jurisdiction for applying the rules and standards of universal law to the countries that most enthusiastically support them. The Nuremberg precedent of victors trying defeated Nazi leaders is recapitulated. (The second author of the Molotov-Ribbentrop pact was tried for conspiring to commit aggression, but no Soviet officials were defendants.) Rubin finds that practice morally one sided and legally dubious. Nevertheless, the Nuremberg example can be useful and significant for the future.

A second contribution Rubin makes is his defense of legal positivism and a "positivist" model that reflects the realities of the current international society better than the "naturalist" model. Given Rubin's attention to law as moral principle, his concluding summary reveals the weight of the evidence undergirding legal positivism. He connects the two in a

discussion of the objective basis of natural law with reference to Aristotle and Vitoria.

Finally, Rubin concludes with a discussion of the relationships of law, morality, and the international political order. In almost 20 tightly written pages, he explores the role of the positivist and naturalist views and examines the claim that "force is the bottom line" versus the concept that justice is determining. He finds no evidence on this question and others that the modern generation is more insightful than earlier thinkers, beginning with Plato and Aristotle.

The Challenge of Fundamentalism: Political Islam and the New World Disorder. By Bassam Tibi. Berkeley: University of California Press, 1998. 262p. \$29.95.

Vali Nasr, University of San Diego

Two decades have passed since the Iranian revolution underscored the centrality of Islam to politics in Muslim societies from Malaysia to Morocco. Since then much has been written on the origins and development of political Islam, more popularly referred to as Islamic fundamentalism or Islamism. Through these works we have come to learn much about the directives of Islamic ideology, the social base of political Islam, the dynamics of its politics, and its relation to state authority. Still, there is much about this phenomenon that needs be explained; more important for comparative politics, the rise of ideational politics in modernizing societies must be properly theorized. The Challenge of Fundamentalism promises to address these concerns. It sets out not only to explain political Islam but also to do so in a theoretical language, using the heuristic tools of the social sciences to shed light on the root causes and development of this phenomenon.

The book does not fulfill its promise. Although there is some discussion of the origins and development of political Islam, for the most part Tibi is concerned with the recent public discussion surrounding the implications of the rise of political Islam for globalization and civilization dialogue (pp. 15–9, 64–113, 199–211). He analyzes how the end of the Cold War—and, hence, the West's search for a new bugbear—as well as the groping for a new world order and the Persian Gulf War are relevant for Western characterizations of Islam. In so doing Tibi revisits arguments that have been put forward by John L. Esposito (The Islamic Threat: Myth or Reality? 1995), Fred Halliday (Islam and the Myth of Confrontation: Religion and Politics in the Middle East, 1995), and Dale Eickelmand and James Piscatori (Muslim Politics, 1994), among others.

What is perhaps new here is the importance Tibi places on Samuel Huntington's "clash of civilizations" thesis (The Clash of Civilizations and the Remaking of World Order, 1996) in constructing his framework of analysis, but Tibi appears to be less concerned with the conceptual implications of that thesis and more interested in debunking the shibboleths that lie at its foundation (pp. 44, 180-3). Consequently, despite the theoretical language, the book deals very little with theory. There also is not much in the form of factual details, history, and case study analysis. Instead, the author remains content with a rhetorical debate about the Huntington thesis. Rather than a social scientific study of political Islam, The Challenge of Fundamentalism is Tibi's entry into the public discussions that Huntington has generated.

Tibi's central argument is as follows. Islam as a religion must be distinguished from political Islam. The former is open to interpretation and can have liberal, mystical, and apolitical manifestations, whereas the latter is a rigid political ideology. There is nothing new in this argument. The distinction between religion and political ideology has been amply discussed in social science inquiries into the role of religion in the politics of Muslim societies, and it has been generally accepted by all observers of the political scene in those societies. It goes without saying that the public discussion of political Islam is less sensitive to this distinction, but this is a book for specialists who will not benefit from belaboring the obvious. Indeed, the distinction is posed here not so much as a conceptual problem but as a straw man. Tibi's elaborate arguments in the end resolve a problem that does not really exist for his audience. The author would have done better to go beyond merely distinguishing between Islam and political Islam to grapple with empirical evidence so as to shed light on the relevance of political Islam to issues of significance to both the social sciences and the study of politics in Muslim societies—patterns of authority, regime stability and change, and prospects for economic reform.

Tibi writes in the language of the social sciences, but what begins as social scientific inquiry quickly becomes his interpretation of Islam and the religion's views on society and politics (pp. 20-32, 139, 158-77). At the least, this creates methodological confusion. Furthermore, it is apparent that the author is not at home with the Islamic sources on which he bases his interpretation of the fundamental directives of the Islamic faith in contradistinction to the prescriptions of Islamic ideology. For instance, he draws on Islamic mystical positions to present a humanistic reading of Islam and to challenge the validity of Islamic ideology (p. 158). Yet, Tibi does not discuss what Islamic mysticism is or, more important, how in theory as well as in practice it can supplant the dominance of Islamic law and the orthodox interpretation of it to foster humanism and tolerance. This point is all the more important since Tibi proffers Islamic mysticism as a solution in the face of the fact that a number of Islamic leaders and activists, Ayatollah Khomeini and Mawlana Mawdudi among others, were students of Islamic mysticism.

Little in this book ensures that it will be central to the study of political Islam. Its contribution at the theoretical level is rather limited, and there is not much that is new in terms of facts and information. The study of political Islam—its ideology, social base, and politics—has already moved far beyond the conceptual framework and conclusions of this book.

Industrial Crists and the Open Economy: Politics, Global Trade, and the Textile Industry in the Advanced Economies. By Geoffrey R. D. Underhill. New York: St. Martin's, 1999, 280p. \$65.00.

Sherry L. Bennett, Rice University

The politics and process of trade policy formation have captured the research interest of many scholars. A growing number use formal models to examine the behavior of interests groups, institutions (e.g., Michael J. Gilligan, Empowering Exporters, 1997; Sharyn O'Halloran, Politics, Process, and American Trade Policy, 1994), and the interaction of these factors at the domestic and international level (e.g., Helen V. Milner, Interests, Institutions, and Information, 1997; Beth A. Simmons, Who Adjusts? 1994). Industrial Crisis takes a traditional approach to explain, in particular, the rise and decline of protection in the clothing and textile industry. Underhill's study is motivated by two principal questions: Why did the Multi-Fibres Arrangement (MFA) for the textile and clothing sector, signed in 1973, emerge during a period when liberalization of manufactured goods trade was generally expanding and increasing in scope? What factors explain why states that expended great efforts to secure international cooperation on the MFA subsequently led the way to dismantle it during trade negotiations in the Uruguay Round? To answer these questions Underhill provides a detailed account of the textile industry that is an extensive case study, beginning in the early 1970s and extending roughly twenty years.

In response to the first question, Underhill explains the reasoning behind the policy preferences among textile and clothing interests. Not surprisingly, in the face of an industrial crisis that began to emerge in the early 1970s, inefficient clothing producers and textile manufacturers, unwilling or unable to adapt to a highly competitive, dynamic marketplace, demanded a variety of subsidies and import restrictions to ease the pain of adjustment. Both readers who are unfamiliar with and interested in the technical complexities of the textiles and clothing industry will be engaged from the beginning. Chapter 1 gives an intricate description of the complex web of interrelationships between producers of raw fibers and manufacturers who assemble clothes and related finished products. Although each stage of the process uses different technologies and has distinct factor mixes, Underhill convincingly demonstrates the interdependencies. Dislocation at any stage is shown to have negative repercussions on production and manufacturing in the entire sector, which forms the logical basis for the convergence in preferences among the various interests in this industry. Chapter 2 delves into the particulars of domestic demand for clothing and textiles in the French market. Within only ten years, demand for finished products dropped, growth slowed, and the level of foreign competition increased. As the analysis of the French case demonstrates, many firms were unwilling to take advantage of their competitive factor mixes or seize opportunities created by the changing market, hence the emergence of a crisis that unified the industry's demand for protection and the eventual passage of the MFA

Although Underhill does a thorough job of establishing the basis for the policy preferences of textile firms, he falls short of demonstrating how these interests successfully organized and later interacted with institutions to influence policy. Chapter 3 examines the pattern of state-industry relations in France with the intended purpose of showing how the textile industry captured the domestic trade policy agenda, but unlike Mancur Olson's The Logic of Collective Action (1968) and the works cited above, there is no convincing account for the logic associated with these interests organizing. Instead, Underhill describes the sector's development in postwar France in an effort to elicit its economic and political salience to the overall economic well-being of the state. Although the regional distribution of the industry was concentrated in particular communities, this alone does not translate into effective political pressure or sufficiently explain the responsiveness of policymakers. Missing in the details of the French experience is a framework that effectively delineates the formation of coalitions, political pressure, and responsiveness by policymakers, all of which can be generalized to the experience of other states.

Important textile groups in Britain and the United States are discussed in some detail, but the extent of their influence is demonstrated through a series of interviews with people involved in the MFA negotiations. Noticeably absent is a sense of how these groups successfully infiltrated the policy-making agenda. This is important because a number of manufacturing interests were competing to influence the trade policy agenda in favor of liberalization. For example, manufacturing exporters in the United States, which had gained political power over time, were able to influence the

passage of the 1962 Trade Expansion Act and other trade policies designed to increase free trade. Their success was due in part to the 1934 Reciprocal Trade Agreement Act and its various extensions, which served to lower the costs to these interests associated with collective action and create the conditions necessary to shift legislative preferences away from protection toward liberalization. These arguments have recently been expressed by Gilligan (Empowering Exporters, 1997) and Michael Bailey, Judith Goldstein, and Barry Weingast ("The Institutional Roots of American Trade Policy: Politics, Coalitions, and International Trade," World Politics 49 [1997]: 309–38). In light of this work, Underhill is unable to answer sufficiently a principal question motivating his research.

Underhill extends his conjecture in chapter 4 to the activities of a transnational coalition of textile interests and their purported capture of the policy agenda in the international domain. One might expect a discussion about the dynamics of bargaining at both the domestic and international level, but there is little explanation of either the formation of an international coalition (e.g., the mobilization of these groups across states) or how it could exert political pressure on policymaking in multilateral institutions. In this sense, one strength of this book becomes one of its greatest weaknesses. Most of the discussion centers on France. Al-

though this provides immense detail about the variety of actors and interests involved in the policymaking process associated with the MFA, it is not clear how generalizable the conjectures are to other states involved in the bilateral negotiations. For example, although Britain and the United States are democratic states, they have different political institutions. How interest groups forge linkages and exert political pressure on policymaking varies among the states in Underhill's study. As expressed by Milner (Interests, Institutions, and Information, 1997) and by George Tsebelis ("Decision-Making in Political Systems: Veto Players in Presidentialism, Parliamentarianism, Multicameralism and Multipartyism," British Journal of Political Science 24 [1995]: 289-325), variations in constitutional authority, electoral incentives, and the distribution of political authority affect decision making on policy across variable political systems.

Although Underhill does not adequately appreciate how variations in political institutions across states influence the formation of trade policy, *Industrial Crisis and the Open Economy* amply illustrates that explanations of policy focusing solely on structural determinants are inadequate. This is demonstrated time and again by Underhill's thorough analysis of interests associated with the French textile and clothing industry.

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Increasing Returns, Path Dependence, and the Study of Politics

PAUL PIERSON Harvard University

It is increasingly common for social scientists to describe political processes as "path dependent." The concept, however, is often employed without careful elaboration. This article conceptualizes path dependence as a social process grounded in a dynamic of "increasing returns." Reviewing recent literature in economics and suggesting extensions to the world of politics, the article demonstrates that increasing returns processes are likely to be prevalent, and that good analytical foundations exist for exploring their causes and consequences. The investigation of increasing returns can provide a more rigorous framework for developing some of the key claims of recent scholarship in historical institutionalism: Specific patterns of timing and sequence matter; a wide range of social outcomes may be possible; large consequences may result from relatively small or contingent events; particular courses of action, once introduced, can be almost impossible to reverse; and consequently, political development is punctuated by critical moments or junctures that shape the basic contours of social life.

t is increasingly common for social scientists to describe political processes as "path dependent." Claims of path dependence have figured in both classic works of comparative politics, such as Lipset and Rokkan's (1967) analysis of European party systems, and more recent analyses on topics such as labor incorporation in Latin America (Collier and Collier 1991), the outcome of state-building processes in Europe (Ertman 1996), and the comparative development of health care systems (Hacker 1998). The notion of path dependence is generally used to support a few key claims: Specific patterns of timing and sequence matter; starting from similar conditions, a wide range of social outcomes may be possible; large consequences may result from relatively "small" or contingent events; particular courses of action, once introduced, can be virtually impossible to reverse; and consequently, political development is often punctuated by critical moments or junctures that shape the basic contours of social life (Collier and Collier 1991; Ikenberry 1994; Krasner 1989). All these features stand in sharp contrast to prominent modes of argument and explanation in political science, which attribute "large" outcomes to "large" causes and emphasize the prevalence of unique, predictable political outcomes, the irrelevance of timing and sequence, and the capacity of rational actors to design and implement optimal solutions (given their resources and constraints) to the problems

that confront them. If path dependence arguments are indeed appropriate in substantial areas of political life, they will shake many subfields of political inquiry. This essay argues that they are.

The analysis begins with a general discussion of path dependence that seeks to clarify some important ambiguities surrounding the concept. I then outline and investigate the distinctive characteristics of social processes subject to what economists call "increasing returns," which could also be described as self-reinforcing or positive feedback processes. For some theorists, increasing returns are the source of path dependence; for others, they typify only one form of path dependence. The focus here is on increasing returns processes, both because they are of great social significance and because (in contrast to broader conceptions of path dependence) social scientists are beginning to develop rigorous arguments about the causes and consequences of increasing returns. Increasing returns dynamics capture two key elements central to most analysts' intuitive sense of path dependence. First, they pinpoint how the costs of switching from one alternative to another will, in certain social contexts, increase markedly over time. Second, and related, they draw attention to issues of timing and sequence, distinguishing formative moments or conjunctures from the periods that reinforce divergent paths. In an increasing returns process, it is not only a question of what happens but also of when it happens. Issues of temporality are at the heart of the analysis.

The following section reviews the development of increasing returns arguments in the social science discipline in which they have received the greatest attention: economics. This review suggests the wide sweep of potential applications, even in a field that might be expected to be hostile to the idea. More important, these economic applications provide the most analytically developed discussions of increasing returns. Economists not only have clarified the principal implications of path dependence but also have identified many of the specific aspects of a particular social environment that generate such processes.

The discussion of economics prepares the way for an exploration of the distinctive characteristics of politics.

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Rather than simply apply extant arguments in economics to political phenomena, we need to consider the features of the political world that require modifications in the use of path dependence claims. I will demonstrate that increasing returns arguments are at least as relevant to an understanding of politics as they are in other areas of the social sciences. Indeed, factors such as the prominence of collective activity in politics, the central role of formal, change-resistant institutions, the possibilities for employing political authority to magnify power asymmetries, and the great ambiguity of many political processes and outcomes make this a domain of social life that is especially prone to increasing returns processes.

The final section considers what these arguments can contribute to political analysis. They provide an important caution against a too easy conclusion of the inevitability, "naturalness," or functionality of observed outcomes. Given the ubiquity of claims about efficient or functional elements in politics, this alone would be an important corrective. More significant, increasing returns arguments justify efforts to stretch the temporal horizons of political analysis. They can redirect the questions political scientists ask, which will contribute to a richer appreciation of the centrality of historical processes in generating variation in political life. They can also direct attention toward particular variables and suggest promising hypotheses about the sources of both political stability and political change in certain common political settings. For instance, increasing returns arguments highlight the need to consider hypotheses based on temporal ordering—the possibility that the particular sequencing of events or processes may be a key part of the explanation for divergent outcomes. Finally, grasping the implications of widespread path dependence can help orient political scientists to a realistic, which is to say modest, set of aspirations regarding the possibilities for achieving parsimony and predictability in the study of politics.

PATH DEPENDENCE AND INCREASING RETURNS

Analysts are increasingly inclined to invoke the concept of path dependence, but clear definitions are rare. In practice, usage tends to fluctuate between a broader and narrower conception. In the broader version, path dependence refers to the causal relevance of preceding stages in a temporal sequence. William Sewell (1996, 262-3), for instance, suggests path dependence means "that what happened at an earlier point in time will affect the possible outcomes of a sequence of events occurring at a later point in time." This usage may entail only the loose and not very helpful assertion that "history matters," although it may also be presented with more rigor. To take an example offered by Andrew Abbott (1983, 131), the contemporary significance of a group's organization of 40% of its potential membership depends greatly on whether that membership used to be 100% or 10%. Note that Sewell's definition involves no necessary suggestion that a particular path is difficult to exit. Rather, the claim is that

we cannot understand the significance of a particular social variable without understanding "how it got there"—the path it took. Previous events in a sequence influence outcomes and trajectories but not necessarily by inducing further movement in the same direction. Indeed, the path may matter precisely because it tends to provoke a reaction in some other direction.

An alternative, narrower definition has been suggested by Margaret Levi (1997, 28):

Path dependence has to mean, if it is to mean anything, that once a country or region has started down a track, the costs of reversal are very high. There will be other choice points, but the entrenchments of certain institutional arrangements obstruct an easy reversal of the initial choice. Perhaps the better metaphor is a tree, rather than a path. From the same trunk, there are many different branches and smaller branches. Although it is possible to turn around or to clamber from one to the other—and essential if the chosen branch dies—the branch on which a climber begins is the one she tends to follow.

This conception of path dependence, in which preceding steps in a particular direction induce further movement in the same direction, is well captured by the idea of increasing returns. In an increasing returns process, the probability of further steps along the same path increases with each move down that path. This is because the *relative* benefits of the current activity compared with other possible options increase over time. To put it a different way, the costs of exit—of switching to some previously plausible alternative—rise. Increasing returns processes can also be described as self-reinforcing or positive feedback processes.

We face, then, a choice between a broader and a narrower conception of path dependence. Frequently, authors are not explicit about which of these two meanings they intend. To establish greater clarity, this essay employs the term in the narrower sense, that is, social processes that exhibit increasing returns. The fuzziness that has marked the use of this concept in social science suggests that the greater range offered by the broader definition has come at a high price in analytical clarity. Path dependence has been a victim of what Sartori (1970) called concept stretching. Different types of temporally linked sequences are generated in different ways and have different implications (Abbott 1983, 1990; Pierson n.d.b). These distinctive kinds of social processes, which have been bundled together, must be disentangled and systematically explored. Limiting the concept of path dependence to self-reinforcing processes in no way precludes the investigation of other ways in which sequences can matter in explaining social outcomes. It does encourage clear argument about distinct claims.1

¹ Another strategy would be to use a broader conception of path dependence and then distinguish subtypes, exploring how these are generated in different ways, with different consequences (Mahoney n.d.). Because path dependence is often invoked without further clarification, however, this option seems more problematic. Ultimately, such definitional disputes remain intractable. What is critical is that researchers be clear and consistent about what they mean when they employ the concept and recognize the importance of differentiating among distinct processes.

However such issues of concept formation are ultimately resolved, there are two compelling reasons for focusing special attention on processes that exhibit increasing returns. First, such processes characterize many important parts of the social world. Second, social scientists are developing theory that makes the investigation of the causes and consequences of increasing returns a particularly promising area of inquiry. These two reasons are developed in later sections of this essay.

The basic logic of increasing returns processes can be captured in a simple mathematical illustration.² Imagine a very large urn containing two balls, one black, one red. Remove one ball, and then return it to the urn, accompanied by an additional ball of the same color. Repeat this process until the urn fills up. What can we say about the eventual distribution of colored balls in the urn? Or about a series of trials in which we fill the urn and then start over again one hundred times?

- In each individual trial we have no idea what the eventual ratio of red to black balls will be; it could be 99.9% red, or 0.01% red, or anything in between. If we were to run 100 trials, we would probably get 100 different outcomes.
- In any particular trial, the ratio will eventually reach an equilibrium. Later draws in a series contribute only minutely to the distribution of balls in the urn. Thus, the distribution settles down onto a stable outcome.
- Sequence is thus crucial. Early draws in each trial, which have a considerable random element, have a powerful effect on which of the possible equilibria will actually emerge.

Mathematicians call this a Polya urn process. Its characteristic qualities stem from the fact that an element of chance (or accident) is combined with a decision rule that links current probabilities to the outcomes of preceding (partly random) sequences.³ Polya urn processes exhibit increasing returns or positive feedback. Each step along a particular path produces consequences which make that path more attractive for the next round. As such effects begin to accumulate, they generate a powerful virtuous (or vicious) cycle of self-reinforcing activity.

Increasing returns processes have quite intriguing characteristics, which Arthur (1994, 112-3) has summarized as follows.

 Unpredictability. Because early events have a large effect and are partly random, many outcomes may

² The following discussion relies heavily on Arthur 1994, a collection of his ground-breaking essays on increasing returns and path dependence.

- be possible. We cannot predict ahead of time which of these possible end-states will be reached.
- 2. Inflexibility. The farther into the process we are, the harder it becomes to shift from one path to another. In applications to technology, a given subsidy to a particular technique will be more likely to shift the ultimate outcome if it occurs early rather than late. Sufficient movement down a particular path may eventually lock in one solution.⁴
- Nonergodicity. Accidental events early in a sequence do not cancel out. They cannot be treated (which is to say, ignored) as "noise," because they feed back into future choices. Small events are remembered.
- 4. Potential path inefficiency. In the long-run, the outcome that becomes locked in may generate lower pay-offs than a forgone alternative would have.

To this one can add a general point of particular interest to social scientists: These are processes in which sequencing is critical. Earlier events matter much more than later ones, and hence different sequences may produce different outcomes. In these processes, history matters.

If these characteristics are common in politics, then they carry major implications, both for the kinds of questions we should ask and the kinds of answers we should expect to find. Most important, they suggest the need to focus on the temporal dimensions of social processes. In searching for explanation, we need to think about causes and effects that are often separated in time, rather than focus exclusively on synchronic explanations (Harsanyi 1960; Stinchcombe 1968). Exploring these implications requires a review of recent work on increasing returns in economics.

INCREASING RETURNS ARGUMENTS IN ECONOMICS

Economics has traditionally focused on the search for unique equilibria. The goal is attractive because it suggests a world of potential predictability and efficiency. Given knowledge of existing factor endowments and preferences, equilibrium analysis will point to a single optimal outcome. Moreover, because economists assume a context of decreasing marginal returns, this goal is potentially achievable. With decreasing returns, economic actions will engender negative feed-

³ This case depicts a specific type of increasing returns process, in which the probability of a particular draw precisely equals the ratio between the two alternatives in the existing population. Arthur (1994) shows that many of the features of this case have a greater range of application, but not all of them. It is easy to model path dependent processes with only two equilibria (e.g., Hill 1997), which probably comes closer to capturing the essence of many path dependent processes in the social world.

⁴ This emerging stability represents a critical distinction between increasing returns processes and chaotic processes, which may generate no equilibrium. For an interesting discussion of this quite different framework, with applications to politics, see Fearon 1996. ⁵ Some social scientists, such as Stephen J. Gould, also have been drawn to arguments about path dependence, critical junctures, and punctuated equilibria in evolutionary biology (see especially Krasner 1989; Spruyt 1994). Without denying the relevance of this literature, I find it a less useful point of departure than the economists' focus on increasing returns. Most aspects of politics lack anything like the mechanism of natural selection that drives Darwinian theory (international relations and certain characteristics of electoral systems constitute important exceptions). Furthermore, socially created constructs of norms and formal institutions have no real analog m evolutionary theory. These constructs, however, are crucial features of politics and, as we shall see, are a critical element in social processes subject to increasing returns.

back, which will lead to a predictable equilibrium. A sharp rise in oil prices prompts increased conservation, exploration, and exploitation of other sources of energy, which will lead to a fall in oil prices. Each step away from equilibrium is more difficult than the one before. As Arthur (1994, 1) summarizes, negative "feedback tends to stabilize the economy because any major changes will be offset by the very reactions they generate.... The equilibrium marks the 'best' outcome possible under the circumstances: the most efficient use and allocation of resources."

During the past fifteen years, however, this decreasing returns tradition has faced a mounting challenge. Economists have exhibited a growing interest in the idea of increasing returns. On a wide range of subjects—including the spatial location of production, the development of international trade, the causes of economic growth, and the emergence of new technologies—path dependence arguments have become prevalent. The ideas developed in this research are not entirely new, but they have been embraced and developed by prominent mainstream economists. This work has received considerable attention in leading journals. Douglass North, who places great emphasis on such arguments in his analysis of the development of modern capitalism, was awarded the Nobel Prize for economics.

The study of technology has provided the most fertile ground for arguments based on increasing returns. As Arthur (1994) and David (1985) have stressed, under conditions often present in complex, knowledge-intensive sectors, a particular technology may achieve a decisive advantage over competitors. although it is not necessarily the most efficient alternative in the long run. This occurs because each technology generates higher payoffs for each user as it becomes more prevalent. When a new technology is subject to increasing returns, being the fastest out of the gate (if only for reasons of historical accident) becomes critical. With increasing returns, actors have strong incentives to focus on a single alternative and to continue down a specific path once initial steps are taken in that direction. Once an initial advantage is gained, positive feedback effects may lock in this technology, and competitors are excluded. Path dependence arguments have been applied to the development of the "QWERTY" typewriter keyboard, the triumph of the light-water nuclear reactor in the United States, the battles between Betamax and VHS video recorders and between DOS-based and Macintosh computers, early automobile designs, and competing standards for electric current.6

Not all technologies, however, are prone to increasing returns. Arthur (1994) addresses not only the characteristics of such processes but also the conditions that give rise to them. Understanding these conditions

is essential, as we shall see, because analytically similar circumstances occur frequently in the world of politics. Arthur's characteristics provide a foundation for developing hypotheses about when increasing returns processes are likely to operate in the social world.

Arthur (1994, 112) argues that four features of a technology and its social context generate increasing returns.

- Large set-up or fixed costs. These create a high pay-off for further investments in a given technology. With large production runs, fixed costs can be spread over more output, which will lead to lower unit costs. When set-up or fixed costs are high, individuals and organizations have a strong incentive to identify and stick with a single option.
- 2. Learning effects. Knowledge gained in the operation of complex systems also leads to higher returns from continuing use. With repetition, individuals learn how to use products more effectively, and their experiences are likely to spur further innovations in the product or in related activities.
- 3. Coordination effects. These occur when the benefits an individual receives from a particular activity increase as others adopt the same option. If technologies embody positive network externalities, then a given technology will become more attractive as more people use it. Coordination effects are especially significant when a technology has to be compatible with a linked infrastructure (e.g., software with hardware; automobiles with an infrastructure of roads, repair facilities, and fueling stations). Increased use of a technology encourages investments in the linked infrastructure, which in turn attracts still more users to the technology.
- 4. Adaptive expectations. If options that fail to win broad acceptance will have drawbacks later on, then individuals may feel a need to "pick the right horse." Although the dynamic here is related to coordination effects, it derives from the self-fulfilling character of expectations. Projections about future aggregate use patterns lead individuals to adapt their actions in ways that help make those expectations come true.

This discussion of technology is important primarily because it clarifies a set of relationships characteristic of many social interactions. New social initiatives—such as the creation of organizations or institutions—usually entail considerable start-up costs; individuals, as well as organizations, learn by doing; the benefits of our individual activities or those of an organization are often enhanced if they are coordinated or "fit" with the activities of other actors or organizations; it is frequently important to bet on the right horse, so we adapt our actions in light of our expectations about the actions of others.

Although arguments about technology are probably the best known, economists have applied similar analyses of increasing returns processes in a striking range of economic contexts. Both Krugman (1991) and Arthur (1994, 49–67) point to the role of increasing returns in the spatial location of production. Given the

⁶ Many of these examples have been contested by critics who deny the empirical claim that superior technologies lost out. Since these criticisms raise broader issues about the usefulness of increasing returns arguments, I will postpone discussion until the end of this section.

importance of physical proximity in many aspects of economic life, agglomeration effects are widespread. That is, initial centers of economic activity may act like a magnet and influence the locational decisions and investments of other economic actors. Established firms attract suppliers, skilled labor, specialized financial and legal services, and appropriate physical infrastructure. The concentration of these factors may in turn make the particular location attractive to other firms that produce similar goods. So do social networks, which allow for easy exchange of information and expertise. Increasing returns arguments help explain the prevalence of pockets of specialized economic activity, from Silicon Valley to the high-end textile manufacturers of northern Italy. Krugman (1991, 80) concludes: "If there is one single area of economics in which path dependence is unmistakable. it is in economic geography—the location of production in space. The long shadow cast by history over location is apparent at all scales, from the smallest to the largest—from the cluster of costume jewelry firms in Providence to the concentration of 60 million people in the Northeast Corridor."

These claims closely parallel recent analyses of international trade, an area in which arguments about increasing returns have gained wide acceptance. Researchers began by focusing on economic trends that appeared anomalous from the perspective of traditional trade theory—most notably, the explosion of intraindustry trade after World War II (Krugman 1996). If comparative advantage results from "natural" features of different countries, then one would expect most trade to occur between quite different countries, such as North-South trade of manufactured goods for raw materials. Yet, most trade is North-North, including extensive exchanges within particular industries. This pattern suggests a puzzle: Why have broadly similar countries developed highly specialized niche comparative advantages?

Increasing returns provide an answer. Knowledgeintensive sectors are prone to positive feedback. Countries that gain a lead in a particular field, for whatever reason, are likely to consolidate that lead over time. The result is a high degree of specialization. Even countries with similar initial endowments develop divergent areas of economic strength. Comparative advantage is not simply given, it is often created through a sequence of events over time.

It is worth noting that this research on trade has been used to derive some controversial policy implications. If first-mover advantages are significant, then free trade may not be an optimal policy for a country whose trade partners are willing to subsidize emerging sectors. Under certain (restricted) conditions, a policy of picking winners may make economic sense (Krugman 1996; Tyson 1993). There remains considerable dispute about the significance of such opportunities for strategic intervention. Krugman, for instance, maintains that they will appear relatively infrequently, not so much because path dependence is rare, but because governments will not be able to identify winners ex ante. Whatever the appropriate policy implications

may be, however, the relevance of increasing returns processes to the economics of trade is now widely accepted.⁷

Economists also have applied increasing returns arguments to economic change more broadly. The most prominent development in recent discussions of economic growth centers on "endogenous growth" theory (Romer 1986, 1990). Economists in the 1980s became puzzled by growth rates (notably in developed countries after World War II) far greater than could be explained by measured increases in inputs of capital and labor. Romer and others argue that increasing returns associated with economic applications of knowledge help account for the anomaly. Unlike capital and labor, many aspects of knowledge are nonrival—their use in one firm does not prevent their use in another. A single gain in knowledge can be applied in many settings and can lead to dramatic improvements in productivity. Economic growth generates the positive feedback that defines increasing returns processes. A somewhat different analysis of growth based on increasing returns emphasizes the importance of complementarities (Milgrom and Roberts 1990). Various economic activities (e.g., in information technology) are complementary to other related activities. Improvements in a core activity can spill over by improving related parts of the economy (lowering costs or increasing productivity). These improvements in turn may increase the attractiveness of the core activity.

Economists are now applying increasing returns arguments to a wide range of important economic phenomena, but Douglass North's application to issues of institutional emergence and change is perhaps most important for students of politics. North (1990a, 95) argues that all the features identified by Arthur in investigations of increasing returns in technology can be applied to institutions. In contexts of complex social interdependence, new institutions often entail high fixed or start-up costs, and they involve considerable learning effects, coordination effects, and adaptive expectations. Established institutions generate powerful inducements that reinforce their own stability and further development.

North emphasizes that not just single institutions are subject to increasing returns. Institutional arrangements induce complementary organizational forms, which in turn may generate new complementary institutions. For social scientists interested in paths of development, the key issue is often what North calls "the interdependent web of an institutional matrix." This matrix, he emphasizes, "produces massive increasing returns" (North 1990a, 95). Path dependent processes will often be most powerful not at the level of individual organizations or institutions but at a more macro level that involves complementary configurations of organizations and institutions (Hall and Soskice 2000; Katznelson 1997).

⁷ As Krugman (1996, 109-10) notes, in the American Economic Association's classification system for journal articles, one now finds "models of trade with increasing returns and imperfect competition" alongside the category for "conventional trade models."

This argument provides the core to North's sweeping reinterpretation of economic history. The central puzzle motivating North's inquiry is the limited convergence of economic performance across countries over time. Neoclassical theory suggests that laggard countries should readily adopt the practices of high performers, which would induce fairly rapid convergence, but this does not happen. According to North, path dependent development of institutional matrices explains the anomaly of continued divergence in economic performance. Once in place, institutions are hard to change, and they have a tremendous effect on the possibilities for generating sustained economic growth. Individuals and organizations adapt to existing institutions. If the institutional matrix creates incentives for piracy, North observes, then people will invest in becoming good pirates. When institutions fail to provide incentives to be economically productive, there is unlikely to be much economic growth.

For political scientists, North's insight is crucial for two reasons. First, he highlights the parallels between characteristics of technology and certain characteristics of social interactions. In this context, it is worth noting that Arthur's arguments about technology are not really about the technology itself but about the characteristics of a technology in interaction with certain qualities of related social activity. Second, North rightly emphasizes that institutional development is subject to increasing returns. Indeed, it is the role of path dependence in explaining patterns of institutional emergence, persistence, and change that may be of greatest significance for political scientists.

The dialogue surrounding increasing returns in economics is the impassioned discourse of an emerging paradigm. Economists talk of "new" growth theory, "new" trade theory, and so on—all based on arguments involving increasing returns. Yet, despite the prevalence of such arguments and the intellectual excitement associated with them, there are excellent reasons to believe that the range of application should be at least as wide in politics as in economics. To understand why, it is helpful to consider the major objections to increasing returns arguments that have recently surfaced in economics. The discussion will clarify the sources of path dependence and identify social mechanisms that might offset such processes. This clarification provides a useful analytical bridge to an investigation of path dependent processes in politics.

In a forceful critique, Liebowitz and Margolis (1995) raise some tough questions about the literature on increasing returns. Two aspects of their argument are relevant here. They emphasize that only "remediable" path dependence is really of theoretical significance, and market mechanisms ensure that remediable path dependence is rare. I will take up each argument in turn.⁸ Following Williamson (1993), Liebowitz and

Margolis (1995) distinguish remediable and nonremediable path dependence. The latter occurs if there are no feasible improvements in the path, either now or in the past. Nonremediable path dependence "stipulates that intertemporal effects propagate error" (p. 207). With hindsight, we wish that some other alternative had been chosen. Yet, Liebowitz and Margolis question whether this type of path dependence has profound implications. If we acted as best we could with the information available at the time, then the mistake was unavoidable, and we cannot reasonably describe the outcome as inefficient. Liebowitz and Margolis argue that the only kind of path dependence with major ramifications is path dependence that is potentially remediable.

Is their dismissal of nonremediable path dependence convincing? As Williamson (1993) notes, for policy purposes remediability is likely to be an appropriate standard. Recognizing the existence of path dependence may not help policymakers much if they do not know how to identify it ex ante. But this objection loses its force if our purpose is instead to understand—perhaps ex post—why aspects of societies move in particular directions and the consequences of such movements. And, of course, it is precisely these questions about causality that are the central preoccupation of most social scientists.

The second part of the Liebowitz and Margolis analysis is the claim that remediable path dependence is rare. Their argument is straightforward. If one of two options is superior in the long run but not in the short run, then market arrangements will generally assure the adoption of the superior path. The ability of private actors to capture the returns from long-term investments prevents bad choices. Institutions of property rights, provisions for patents, and extensive capital markets mean that options with low short-run pay-offs will nonetheless receive the support they deserve. Economic actors, in short, calculate in the shadow of the future and are thus unlikely to indulge in myopic, short-term maximizing behavior at their own long-term expense.

This argument has considerable merit, 10 but how much merit depends on the strength of these mechanisms for overcoming short-term thinking or free-riding. Although Liebowitz and Margolis are more than a little complacent about the capacity of these various market mechanisms, it is wise to leave those issues to economists. Two objections, however, are critically important. First, arguments about the far-sightedness of markets seem to apply to only some types of path dependence in the economy. The Liebowitz and Margolis critique focuses on the decisions of firms to invest in particular technologies or products. In most of the illustrations discussed earlier (e.g., spatial agglomorations, trade specialization, endoge-

⁸ Note that the Liebowtiz and Margolis critique depends on both parts of the argument being true. The significance of path dependence for social scientists can be sustained if either the relevance of nonremediable path dependence or the prevalence of remediable path dependence can be sustained.

⁹ As noted before, precisely for this reason Krugman and others question those who make broad claims about the implications of increasing returns arguments for trade policy.

¹⁰ Indeed, Arthur (1994, 28, fn 11) explicitly recognizes this possibility, although as far as I know he does not systematically pursue the implications.

nous growth), however, many of the benefits of increasing returns are external to individual firms and cannot be fully captured by individual investors and entrepreneurs. Thus, the mechanisms identified by Liebowitz and Margolis are unlikely to ensure that the best long-term outcome will be selected.

Perhaps more important, the Leibowitz and Margolis (1995) argument has little relevance to the development of institutions, which are also subject to increasing returns. Private actors cannot obtain patents or venture capital to capture the long-term economic gains from constructing key economic institutions. Indeed, the Leibowitz and Margolis argument simply assumes the presence of institutions that support market mechanisms. Also, their argument does not seem to have much relevance for North's argument about the presence in particular polities of networks or matrices of institutions and organizations. The fact that they do not even cite North's work is telling. North maintains that path dependent processes of institutional development are crucial to the evolution of particular market economies. Far-sighted financial markets are of limited help in triggering such institutional development; to a large extent, they are its product.

The failure of Leibowitz and Margolis to address issues of institutional development in economies points to a more fundamental objection. Even if one accepts their analysis regarding the economic sphere, their arguments still have limited relevance for political scientists. However strong market mechanisms for "far-sightedness" may be, they are almost certainly far weaker in politics. I explain why in the next section.

MOVING FROM ECONOMICS TO POLITICS: THE APPLICABILITY OF INCREASING RETURNS ARGUMENTS

Microeconomic theory illuminates important features of the political landscape in fields ranging from the study of party competition, to the formation of interest groups and social movements, to voting and legislative behavior. The value of economists' theoretical exports is greatly enhanced, however, if the political science importers take careful account of the distinctive features of the "local" environment. As Terry Moe (1990, 119) states in a related context: "The real problem is to try to identify those essential features of politics that might serve as a foundation for theory, a foundation that can take advantage of the new economics without being overwhelmed or misdirected by it." Arguments drawn from economics must be sensitive to the quite different nature of the political world (Lindblom 1977; Moe 1984, 1990; North 1990b).

Politics differs from economics in many ways. The key is to specify which aspects are most relevant to an investigation of the sources and consequences of path dependence. Following a brief summary of the distinctive tasks of the political arena, this discussion is divided into two parts. The first considers four prominent and interconnected aspects of politics that make this realm of social life conducive to increasing returns processes: (1) the central role of collective action; (2)

the high density of institutions; (3) the possibilities for using political authority to enhance asymmetries of power; and (4) its intrinsic complexity and opacity. After briefly explicating each, I will discuss their relevance to path dependence. Each of these features makes increasing returns processes prevalent in politics.

Second, I explain why the ameliorative mechanisms that Liebowitz and Margolis identify in economic systems are often ineffective in offsetting path dependence in politics. Three characteristics of politics change the picture considerably: the absence or weakness of efficiency-enhancing mechanisms of competition and learning; the shorter time horizons of political actors; and the strong status quo bias generally built into political institutions. Each of these features makes increasing returns processes in politics particularly intense. They increase the difficulty of reversing the course down which actors have started. Increasing returns processes are now central to economic theory and the argument here is that these dynamics will be very widespread and often more difficult to reverse in politics.

For my purposes, the fundamental feature of politics is its preoccupation with the provision of public goods.¹¹ Such goods are distinguished by jointness of supply (the production costs for the good are unaffected or only modestly affected by the number of those consuming it) and nonexcludability (it is very costly or impossible to limit consumption to those who have paid for a good). These features, which are extremely widespread in modern life, make public goods—from national defense to environmental protection—difficult to provide through markets. Nonexcludability creates free-riding incentives, since individuals will receive the benefits of a public good whether or not they contribute to its production. Jointness of supply means that private markets will underproduce the good in question, since private actors tend to consider only the benefits to themselves.

These characteristics of public goods help explain why the central features of political systems are compulsory rather than voluntary. The exercise of authority, generally combined with a complex array of complementary institutions designed to circumscribe and legitimate that authority, is necessary to generate collective provision. Legally binding rules are not just a foundation for political activity (like property rights in the economy). They are instead the very essence of politics (Lindblom 1977; Moe 1990). This key quality of politics has a number of repercussions for the character of political life, each of which is relevant for an assessment of tendencies toward path dependence.

The Collective Nature of Politics

A quick contrast with economic markets can highlight the prevalence of collective action in politics. Suppose

¹¹ In most cases, the goods in question are not "pure" public goods—a fact that complicates the analysis but does not alter my basic claims. For discussions, see Mueller 1989, chap. 2, and Cornes and Sandler 1996.

you work for a firm with an annoying boss and bad pay. You have a clear option: Seek work elsewhere, either at one of a large number of other firms or by setting up business on your own. Your ability to move depends on the state of the labor market, but the existence of competitive options sets clear limits on how annoying your boss can afford to be and on how bad the pay can get.

Or suppose you invent a great new product. Assuming that you can get financial backing (which you should be able to do—it is a great idea, and the market generates a ready supply of venture capitalists), your prospects are good. Nothing stops you from going into business or selling the idea to someone. Either way, the new, superior product gets to see the light of day, and you reap considerable benefits from your innovation.

The setting of consumers, at least in the textbook case, is similarly atomistic. My decisions are essentially independent of my expectations regarding the choices of other consumers.¹² There is no need for explicit attempts to coordinate behavior; the market simply aggregates the isolated decisions of individuals.

These highly stylized examples illustrate the flexibility, fluidity, and atomization of economic markets. In contrast, political "markets" are generally far from flexible and fluid. In politics, the consequences of my actions are highly dependent upon the actions of others. What I get depends not just on what I do, but (mostly) on what others do. Following Olson's (1965) path-breaking work, students of politics have long recognized the "logic of collective action." Most of the "goods" produced in politics are public goods; it is difficult to limit their consumption to those who helped provide them. As a result, individuals will have a strong tendency to free-ride. Creating conditions favorable to collective action is a principal issue in political life.

The problem is not limited to the fact that the public sector produces public goods. Given the reliance of politics on mechanisms of collective decision backed by authority, laws themselves have the character of public goods for those who benefit from them. In the words of Marwell and Oliver (1993, 42), "influencing government policy almost always has very high jointness of supply." These circumstances generate major collective action problems.

There is another reason political action frequently requires coordination. Many of the goals pursued by political actors have a "lumpy" or "winner-take-all" quality (politicians seeking reelection, coup plotters, and lobbyists either win or lose; legislation either passes or is rejected). Unlike economic markets, in which there usually is room for many firms, in politics finishing second may not count for much. Indeed—the Menshiviks in 1917 come to mind—it can be extremely problematic. Again, the effectiveness of my actions

depends heavily on the actions of others. This is less true of some aspects of politics—such as answering an opinion poll question or voting—than others. Even in voting, however, the lumpiness of election outcomes (in the absence of a pure system of proportional representation) means that the actions of a person who does not want to "waste" her vote may well turn on what she expects others to do.

A crucial feature of most collective action in politics is the absence of a linear relationship between effort and effect. Instead, collective action frequently involves many of the qualities conducive to positive feedback (Marwell and Oliver 1993). A central reason is the prevalence of adaptive expectations. When picking the wrong horse may have very high costs, actors must constantly adjust their behavior in the light of how they expect others to act. Whether you put energy into developing a new party, or join a potential coalition, or provide resources to an interest group may depend to a considerable degree on your confidence that a large number of other people will do the same. In addition, many types of collective action involve high start-up costs, which reflects the fact that considerable resources (material or cultural) need to be expended on organizing before the group becomes self-financing.

That collective action processes in politics are very often subject to increasing returns explains why social scientists are often struck by the considerable stability of patterns of political mobilization over time. Lipset and Rokkan's (1967) work on political parties in Europe exemplifies this dynamic: Key historical junctures produced major political cleavages. These political divisions became organized into political parties. Once they have surmounted initial start-up costs and fueled processes of adaptive expectations, these parties are reproduced through time, which generates "frozen" party systems.

Recent work by Skocpol (1999) on extensive voluntary associations in the United States provides additional strong evidence of the organizational persistence that can result from positive feedback. Skocpol identified all such organizations that had ever enrolled more than 1% of the American population (or half that amount for single-gender groups) and tracked them over time. The results, which cover 58 groups since the 1830s, reveal striking organizational continuities. Although some crossed the 1% threshold only for a relatively short period, 26 remain above it today. Of these, 16 had reached the 1% mark by the 1940s, and a number of them stretch back much farther. A large number have fallen from the list, but they managed to stay on it for many decades. Among the 40 organizations founded before 1900, 19 stayed above the 1% mark for at least five decades. Ten of the 40 are still above that threshold, a century or more after their founding.¹³ In short, despite massive social, economic,

¹² This represents a critical difference between economics and politics, but a number of important qualifications need to be made. For instance, consumption often involves significant externalities, which make consumer choices interdependent. As already noted, conditions of independent consumption often do not apply to high-tech products, which frequently involve network externalities. For a good discussion of some of these complications, see Hirsch 1977.

¹³ It should be emphasized that this evidence understates organizational persistence, since many groups that fall below the demanding 1% threshold nonetheless continue to have a very large membership. Also, they may have existed as quite large organizations for long periods before initially crossing the threshold.

and political changes over time, self-reinforcing dynamics associated with collective action processes mean that organizations have a strong tendency to persist once they are institutionalized.

The Institutional Density of Politics

As much recent work in political science stresses, efforts to coordinate actors in the pursuit of public goods often require the construction of formal institutions. Once established, these institutional constraints can apply to all—those who do not approve as well as those who do-and they are backed up, ultimately, by force. The exit option, so central to the workings of the market, is often unavailable (or prohibitively costly) to actors who feel poorly served by existing political arrangements. In politics, institutional constraints are ubiquitous. Politics involves struggles over the authority to establish, enforce, and change the rules governing social action in a particular territory. In short, much of politics is based on authority rather than exchange. Both formal institutions (such as constitutional arrangements) and public policies place extensive, legally binding constraints on behavior.

Although unorthodox, the inclusion of public policies as well as formal institutions in this formulation is important (Pierson 1993). Policies are generally more easily altered than the constitutive rules of formal institutions, but they are nevertheless extremely prominent constraining features of the political environment. Policies, grounded in law and backed by the coercive power of the state, signal to actors what has to be done and what cannot be done, and they establish many of the rewards and penalties associated with particular activities. Most policies are remarkably durable (Rose 1990). Especially in modern societies, extensive policy arrangements fundamentally shape the incentives and resources of political actors.

That such institutions are prone to increasing returns is implicit in much recent research on institutions. Scholars emphasize how institutions can help actors overcome various dilemmas arising from collective choice situations—especially the need to coordinate their behavior by disciplining expectations about the behavior of others. What is absent or downplayed, however, is a recognition that these characteristics render processes of institutional development path dependent.

As already discussed, North highlights how institutions induce self-reinforcing processes that make reversals of course increasingly unattractive over time. In contexts of complex social interdependence, new institutions and policies are costly to create and often generate learning effects, coordination effects, and adaptive expectations. Institutions and policies may encourage individuals and organizations to invest in specialized skills, deepen relationships with other individuals and organizations, and develop particular political and social identities. ¹⁴ These activities increase

the attractiveness of existing institutional arrangements relative to hypothetical alternatives. As social actors make commitments based on existing institutions and policies, their cost of exit from established arrangements generally rises dramatically.

Political Authority and Power Asymmetries

In the famous community power debate of the 1960s and 1970s, Bachrach and Baratz (1962) and Lukes (1974) argued persuasively that power asymmetries are often hidden from view; where power is most unequal, it often does not need to be employed openly. Pluralist critics essentially countered that a systematic evaluation of such claims was impossible (Polsby 1963; Wolfinger 1971). Although he does not frame the issue quite this way, Gaventa (1980) demonstrates that power asymmetries can reflect the operation of positive feedback processes over substantial periods. Increasing returns processes can transform a situation of relatively balanced conflict, in which one set of actors must openly impose its preferences on another set ("the first face of power"), into one in which power relations become so uneven that anticipated reactions ("the second face of power") and ideological manipulation ("the third face") make open political conflict unnecessary. Thus, positive feedback over time simultaneously increases power asymmetries and renders power relations less visible.

The allocation of political authority to particular actors is a key source of this kind of positive feedback. Indeed, this represents a source of path dependence quite distinct from those discussed by Arthur and North. When certain actors are in a position to impose rules on others, the employment of power may be self-reinforcing (Mahoney 1999). Actors may use political authority to generate changes in the rules of the game (both formal institutions and various public policies) designed to enhance their power. Relatively small disparities in political resources among contending groups may widen dramatically over time as positive feedback sets in.

The Complexity and Opacity of Politics

Economic theory is built in large part around the useful and plausible assumption that actors seek to optimize and are relatively good at it. Firms operate to maximize profits. The metric for good performance is relatively simple and transparent. Prices send strong signals that facilitate the analysis of how various features of the economic environment affect firm performance. Observable, unambiguous, and often quantifiable indicators exist for many of these features. Workers can

to mean previous outlays that cannot be recovered and should be regarded as irrelevant to current choices among options. The whole point of path dependence, however, is that these previous choices often are relevant to current action. In cases of increasing returns, social adaptations represent investments that yield continuing benefits. Actors may be locked into a current option because massive new investments may be required before some theoretically superior alternative generates a higher stream of benefits.

¹⁴ It is common to refer to such consequences as sunk costs. Although intuitive, this terminology is unfortunate. Economists use it

obtain fairly good information on the wages and working conditions on offer from different firms. Consumers, too, are reasonably adept at navigating most aspects of the economic world. Links between choices and outcomes are generally clear. Take a new job and your income rises; buy a car and your savings account balance shrinks. The quality of goods is usually evident in relatively short order, and repeated purchases allow consumers to sample alternatives.

Of course, one can add many complications to this simple picture of the economic realm. The market is often highly complex and confusing. Yet, the clarifying role of prices, the prevalence of repeated interactions, the absence of a need to coordinate many of one's economic decisions with those of other actors, and the presence of relatively short causal chains between choices and results greatly facilitate the efforts of economic actors to correct mistakes over time.

Politics is a far, far murkier environment (Moe 1990; North 1990b). It lacks anything like the measuring rod of price. Political actors pursue a range of goals. Furthermore, it is often very hard to observe or measure important aspects of political performance. And, if we believe that a system is not performing well, it is still more difficult to determine which elements in these highly complex systems are responsible and what adjustments would lead to better results. The reliance on elaborate procedures to handle collective choice situations in politics is inescapable, but it undermines transparency, that is, it greatly increases transaction costs (Cornes and Sandler 1996; Mueller 1989). The complexity of the goals of politics as well as the loose and diffuse links between actions and outcomes render politics inherently ambiguous.

Even if mistakes or failures in politics are apparent, improvement through trial-and-error processes is far from automatic. Many participants in politics (voters, members of interest groups) engage in activities only sporadically. Their tools of action are often crude, such as the blunt instrument of the vote, and their actions have consequences only when aggregated. There may be long lags and complex causal chains connecting these political actions to political outcomes. The result is that mistaken understandings often do not get corrected.

The point is not that learning never occurs in politics. Rather, learning is very difficult and cannot be assumed to occur. Instead, understandings of the political world should themselves be seen as susceptible to path dependence. Drawing on work in both cognitive psychology and organizational theory, researchers argue that actors who operate in a social context of high complexity and opacity are heavily biased in the way they filter information into existing "mental maps" (Arthur 1994; Denzau and North 1994). Confirming information tends to be incorporated, and disconfirming information is filtered out. Social interpretations of complex environments like politics are subject to positive feedback. The development of basic social understandings involves high start-up costs and learning effects; they are frequently shared with other social actors in ways that create network effects and adaptive expectations. The need to employ mental maps induces increasing returns. This is true both at the individual level and at the group level, as "communities of discourse" often come to share and reproduce a similar ideology (Wuthnow 1989).¹⁵

This recent work converges with the long-standing views of those who study political culture as well as the recent contributions of cognitive science. ¹⁶ Once established, basic outlooks on politics, ranging from ideologies to understandings of particular aspects of governments or orientations toward political groups or parties, are generally tenacious. They are path dependent. ¹⁷

There are, then, compelling reasons to believe that political life will often be marked by dynamics of increasing returns. Tendencies toward positive feedback characterize four processes central to political environments: collective action, institutional development, the exercise of authority, and social interpretation. In each case, there are reasons to anticipate that steps in a particular direction can trigger a self-reinforcing dynamic. This conclusion should be underlined. By itself, it suggests why increasing returns is a critical concept for those who seek to understand the sources of political stability and change. A recognition that self-reinforcing processes are significant is shaking up economics, and political scientists have at least as great a need to consider their implications.

There is also reason to believe that these effects in politics are often particularly intense. In the remainder of this section I consider why it is frequently more difficult to reverse course in politics than it would be in economics. Economists argue that the market provides two powerful mechanisms for exiting problematic paths: competition and learning. Competitive pressures in a market society mean that new organizations with more efficient structures will develop and eventually replace suboptimal organizations (Alchian 1950). Learning processes within firms also can lead to correction. According to Williamson (1993, 116–7), one can rely on

¹⁵ Wuthnow's (1989) subtle analysis of the comparative development of ideologies, with its emphasis on relatively brief periods of historical openness followed by processes that select and then institutionalize a particular track of ideological development, is broadly consistent with the framework suggested here

¹⁶ Consider the statement by Mannheim (1952, 298) in his famous cassay on generations: "It is of considerable importance for the formation of consciousness which experiences happen to make those all-important 'first impressions.' . . . Early impressions tend to coalesce into a natural view of the world."

¹⁷ Indeed, as marketers know well, path dependent cognitive effects are evident even in the less ambiguous world of consumption. This is why advertisers covet the attention of youngsters, who have yet to make definitive (and resilient) choices. A telling recent example is the marketing effort of the National Football League, which is alarmed by indications that youngsters are increasingly drawn to basketball and soccer. A former MTV executive now working on special events speaks the language of increasing returns: "It's all about getting a football... into a kid's hands as soon as you can. Six years old, if possible. You want to get a football in their hands before someone puts a basketball in their hands, or a hockey stick or a tennis racquet or a golf club" (Seabrook 1997, 47).

the "far-sighted propensity" or "rational spirit" that economics ascribes to economic actors.... Once the unanticipated consequences are understood, these effects will thereafter be anticipated and the ramifications can be folded back into the organizational design. Unwanted costs will then be mitigated and unanticipated benefits will be enhanced. Better economic performance will ordinarily result.

Of course, neither mechanism represents a guaranteed corrective in the context of increasing returns. Options that gain a head start will often reinforce themselves over time, even if they have serious shortcomings. What I wish to stress, however, is that Williamson's corrective mechanisms are even less effective when one shifts from firms in private markets to the world of political institutions (Moe 1984, 1990; Pierson n.d.a). This is clearest for mechanisms of competition. Political institutions rarely confront a dense environment of competing institutions that will instantly capitalize on inefficient performance, swooping in to carry off an institution's "customers" and drive it into bankruptcy. Models of competition may be helpful for understanding some important aspects of politics (such as international relations and elections), but there can be little doubt that political environments are typically more "permissive" than economic ones (Krasner 1989).

As just discussed, the complexity and ambiguity of politics create serious problems for learning arguments. It may be appropriate to argue that politics sometimes involves learning processes, in which responses to public problems proceed in a trial-and-error fashion (Hall 1993; Heclo 1974). There is little reason, however, to think that this acts as a selection mechanism with anything like the efficiency-enhancing properties of market competition in economics or Darwinian natural selection in biology. Because political reality is so complex and the tasks of evaluating public performance and determining which options would be superior are so formidable, such self-correction is often limited.

Even when learning does occur, it faces additional hurdles. In Williamson's (1993, 117) words, learning must still be "folded back into the organizational design." All the barriers to change in systems subject to increasing returns become relevant: Long movement down a particular path will increase the costs of switching to some previously forgone alternative. Furthermore, in politics the pursuit of such change faces two additional obstacles: the short time horizons of political actors and the strong status quo bias associated with the decision rules that govern most political institutions. These factors often make path dependent effects particularly intense in politics.

Time Horizons. A statement attributed to David Stockman, budget director during the Reagan administration, is unusual among political decision makers only for its candor. Asked by an adviser in 1981 to address Social Security's severe long-term financing problems, Stockman dismissed the idea out of hand. He explained that he had little interest in wasting "a lot

of political capital on some other guy's problem in [the year] 2010" (quoted in Greider 1982, 43).

Many of the implications of political decisions especially complex policy interventions or major institutional reforms—only play out in the long run. Yet, political actors, especially politicians, are often most interested in the short-term consequences of their actions; long-term effects tend to be heavily discounted. The principal reason is the logic of electoral politics. Because the decisions of voters are taken in the short run, elected officials generally employ a high discount rate. They will pay attention to long-term consequences only when these become politically salient or when they have little reason to fear short-term electoral retribution. As John Maynard Keynes once noted, in the long run we are all dead; politicians in democratic polities have special reason to take that message to heart.

Political scientists have paid limited attention to the issue of time horizons. An interesting literature is developing on "credible commitments"—the attempt of political actors to create arrangements that facilitate cooperation by lengthening time horizons (North 1993; North and Weingast 1989; Shepsle 1991). We know relatively little about the time horizons of different political actors or about the institutional arrangements conducive to lowering their discount rates (i.e., increasing the political relevance of the future). Recent research suggests that particular institutional designs (such as independent central banks), which empower particular kinds of political actors (e.g., bankers), may succeed in lengthening time horizons in politics.

In general, however, such mechanisms are less effective in politics than in economics. As noted, the marketplace possesses some strong mechanisms for lengthening time horizons—especially property rights and capital markets. The mechanisms in politics are generally far weaker. Monitoring political behavior over time is difficult because indicators of performance are typically so limited. It is no accident that much of the generally optimistic rational choice discussion of "credible commitments" in politics has focused on relatively transparent economic issues (e.g., budget deficits, monetary policy). In these instances, performance indicators are clear, and behavior is easy to monitor. Although these issues are clearly important, it must be stressed that for reasons already noted they are fundamentally atypical of the kinds of matters dealt with in politics. Not only is monitoring often exceptionally difficult in politics, but also the relatively rapid turnover of key positions makes it hard to hold actors accountable. Politics, in short, lacks the characteristic property rights that facilitate the linkage of actors' decisions over time in the economic sphere. In many cases, the long term is essentially beyond the political horizon. A statesman, Bismarck is said to have pronounced, is a politician who thinks about his grandchildren.

The different nature of time horizons in politics and in economics matters a lot. This can be seen by revisiting the Liebowitz and Margolis (1995) critique of path dependence. They properly point to key market institutions as a (partial) protection against (certain

kinds of) remediable path dependence. If it is believed that one option (say, Amazon.com) has greater longterm benefits, then investors should gravitate toward that option even if in the short term it will perform more poorly than an alternative. They argue that market mechanisms should allow the more efficient result

In politics the outcome may well be different. Assume that the crucial decision maker is a politician up for reelection in two years. In this context, effects after the election cycle may not count for much. A politician who focuses on the short-term pay-off will choose a different option from that of far-sighted economic investors. This difference in time horizons has profound consequences. If time-horizons tend to be short, then we can expect that long-term costs and benefits will have a limited effect on the chosen path. Furthermore, once on a particular path, political actors will generally have powerful incentives to stay on it. Switching costs are typically borne in the short run, and the benefits will generally only accrue in the long run, that is, to someone else.

The Status Quo Bias of Political Institutions. Political arrangements are unusually hard to change. In the economic realm, an individual with a new idea for a product need only secure the financing to put it on the market. If enough consumers (choosing independently) find it sufficiently appealing, the product will be a success. Change can be engineered through competition against existing products. Similarly, those with property rights over a firm are generally in a strong position to remake their organizations as they choose. Lines of authority are clear, and the relevant decision makers are likely to share the same broad goal of maximizing profits.

By contrast, the key features of political life—public policies and (especially) formal institutions—are change-resistant. Both are generally designed to be difficult to overturn for two broad reasons. First, those who design institutions and policies may wish to bind their successors. According to Moe (1990), this reflects the problem of "political uncertainty." Unlike economic actors, political actors must anticipate that their political rivals may soon control the reins of government. To protect themselves, they may create rules that make preexisting arrangements hard to reverse. As Moe (1990, 125) puts it, designers "do not want 'their' agencies to fall under the control of opponents. And given the way public authority is allocated and exer-

cised in a democracy, they often can only shut out their opponents by shutting themselves out too. In many cases, then, they purposely create structures that even they cannot control."

Second, in many cases, political actors also are compelled to bind themselves. The key insight of the "credible commitments" literature is that actors can often do better, in the short run as well as the long run, if they remove certain options from their future menu. The economy of a country will grow faster, for instance, if a monarch can credibly commit himself to refrain from expropriating an excessive amount of the hard-earned wealth of his subjects (North and Weingast 1989). This can be done if he accedes to parliamentary control over the power to tax.

To constrain themselves and others, designers create large obstacles to institutional change. The barriers to reform may be extremely high, such as unanimity requirements in the European Union and multiple supermajorities to alter the U.S. Constitution. Of course, these obstacles facilitate forms of cooperation and exchange that would otherwise be impossible. The relevant point here is that this status quo bias characteristic of political systems reinforces the already considerable difficulties of moving off an established path.²⁰ Combined with the lack of competitive mechanisms, the weakness of learning processes, and the short time horizons characteristic of politics, the bias means that increasing returns tendencies in political development are often particularly intense.

Politics differs from economics in many ways. Applying tools of economic analysis to politics is treacherous unless these differences are systematically considered. In the case of arguments about path dependence, attention to the character of politics suggests a striking result. The political world is unusually prone to increasing returns. Both the prevalence and intensity of increasing returns processes suggest that path dependence arguments offer important insights for understanding political dynamics.

¹⁸ These long-term effects will count if an actor with longer time horizons (such as an interest group) can make them relevant to politicians, such as through campaign contributions or votes. The question is whether such mechanisms are anywhere near as effective as the capital markets operative in the economic sphere. In my view, there are strong reasons to be skeptical, but it is an issue that deserves considerable attention.

¹⁹ This assumes that the actors involved care about what the government does after their faction loses an election. For reasons just noted (and as Moe observes) the problem of long-term political uncertainty is likely to be of greater concern for interest groups than for politicians.

²⁰ An important characteristic of political systems runs counter to this line of argument. Because politics is a powerful system for mobilizing coercive power, governments may at times be in a position to orchestrate a "jump" from one path to another. By employing sanctions, they can coordinate adjustments in a way that markets might never be able to achieve. For instance, the British government enacted a shift to the metric system that would have been difficult or impossible to engineer through the more atomistic mechanisms of the market. Governments are clearly capable on occasion of mobilizing resources for more dramatic changes in course. Such possibilities, however, should not be exaggerated. The metric example represents a modest instance of reversing path dependence. Switchmg costs were relatively low; the principal problem was one of coordination, of inducing everyone to make the switch at the same time. For this task, the authoritative rule-setting capacities of government are of great assistance. For reasons already discussed, it is much less evident that governments will generally be willing or able to engineer shifts to a different path when switching costs are high. Cases of fundamental or revolutionary reform in well-institutionalized political systems attract our attention precisely because they are

PATH DEPENDENCE AND THE STUDY OF POLITICS

To summarize briefly, in settings in which increasing returns or path dependent processes are at work, political life is likely to be marked by four features.

- Multiple equilibria. Under a set of initial conditions conducive to increasing returns, a number of outcomes—perhaps a wide range—are generally possible.
- Contingency. Relatively small events, if they occur at the right moment, can have large and enduring consequences.
- 3. A critical role for timing and sequencing. In increasing returns processes, when an event occurs may be crucial. Because earlier parts of a sequence matter much more than later parts, an event that happens "too late" may have no effect, although it might have been of great consequence if the timing had been different.
- Inertia. Once an increasing returns process is established, positive feedback may lead to a single equilibrium. This equilibrium will in turn be resistant to change.

There are also good reasons to think that increasing returns processes are widespread in politics, since they will be characteristic in institutional development, collective action, the exercise of authority, and the emergence of our understandings of the political world.

If increasing returns processes are prevalent in politics, then there are fundamental theoretical implications. We need to change both the kinds of questions we ask about politics and the kinds of answers that we generate. With respect to questions, the most important implication is the need to focus on branching points and on the specific factors that reinforce the paths established at those points. Students of comparative politics and American political development have long been interested in critical moments. The dynamics of increasing returns lay out what Collier and Collier (1991, 31) term the "mechanisms of reproduction," which carry and often amplify the effects of a critical juncture through time. Discussions of path dependence and critical junctures are often conflated, but as Hacker (1998) has emphasized, it is important to keep them distinct. Arguments about path dependence explain why particular historical junctures have lasting consequences. Yet, although claims about critical junctures seem to rest on assertions of increasing returns processes (otherwise it is not clear why the juncture is "critical"), the inverse does not hold. Path dependent arguments based on positive feedback suggest that not only "big" events have big consequences; little ones that happen at the right time can have major consequences as well.

A focus on increasing returns processes justifies a turn to history. At one level, of course, all social scientists agree that history matters. Current conditions, which influence current social outcomes, came into being in some way. Those earlier processes are relevant to a full understanding of contemporary social

events. Yet, the standard assumption is that for most purposes we may safely put such issues aside. Looking back leads to the familiar problem of infinite regress. An exploration of each preceding event leads to the conclusion that some other previous occurrence was also part of the chain of necessary events, and so on. Social scientists need to break through the seamlessness of history somewhere, and the present is as good a place to do so as any. Homans (1967, 92-3) compares the situation to that faced by navy minesweepers, who need to know the magnetic charge of a ship. Such a charge results from an infinite range of small factors accumulated over the ship's lifetime. For practical purposes, however, a simple expedient can be used: The current charge of the ship can be measured. If the task is to understand the ship's vulnerability to mines, one can simply cut through the Gordian knot of historical regress.21

For many purposes, this is an appropriate approach. Social scientists often have good reason to focus on synchronic causality—to try to understand how variations in current variables affect present social outcomes. When increasing returns processes are significant, however, such a strategy may be problematic. Increasing returns arguments rest on a conception of "historical causes" (Harsanyi 1960; Ikenberry 1994; Stinchcombe 1968, 103-18), that is, some original ordering moment triggered particular patterns, and the activity is continuously reproduced even though the original event no longer occurs. Under conditions of path dependence it is true that current circumstances in some sense "cause" current outcomes, but a focus on these simultaneous occurrences is highly misleading. It provides a "snapshot" explanation for what should be seen as a moving picture. The necessary conditions for current outcomes occurred in the past. The crucial object of study becomes the critical juncture or triggering events, which set development along a particular path, and the mechanisms of reproduction of the current path—which at first glance might seem commonplace or at least analytically uninteresting.

An awareness of increasing returns processes can change not only the questions we ask but also the answers we provide. Put differently, an understanding of increasing returns can be a fruitful source of hypotheses about the sources of social outcomes. One merit of increasing returns arguments is that they provide a plausible counter to functionalist explanations in political science, which often go unchallenged. Although not always explicitly stated, functionalist arguments are prevalent among political scientists. They are common, for instance, among those who emphasize the rational choices of individual actors that underlie political activity and the reasonably efficient nature of collective responses to social needs (Keohane 1984; Shepsle 1986; Weingast and Marshall 1988).

Functionalist arguments take the following form: Outcome X (e.g., an institution, policy, or organization) exists because it serves the function Y. In a world of purposive actors, it may indeed be the case that the

²¹ For a discussion of Homans's argument, see Knapp 1984, 43-5.

effects of an institution have something to do with an explanation for its emergence and persistence. Arguments about increasing returns, however, suggest the large dangers in any assumption that an institution arose because it serves some particularly useful purpose. Thinking in functionalist terms about an institution, policy, or social organization may be a good way to derive causal hypotheses, but functional accounts are far from being the only plausible ones. Many alternatives to the outcome in question might have been possible, and a dynamic of increasing returns may have locked in a particular option even though it originated by accident, or the factors that gave it an original advantage may have long since passed away. Rather than assume relative efficiency as an explanation, we have to go back and look. Thus, recognizing the possibility of path dependence necessarily draws social scientists to an investigation of history, if only to evaluate the validity of functionalist assertions.

More positively, an investigation of path dependence can provide a basis for developing important hypotheses about the sources of political stability and change. To repeat, Arthur's work on increasing returns is ground-breaking not simply because it describes the characteristics of these processes; it identifies conditions conducive to path dependence. The major ambition of this essay, building on North's work, is to begin the process of adapting these arguments to the study of politics. Doing so requires careful attention to the distinctive features of the political world, such as its intrinsic ambiguity, the prevalence of change-resistant institutions, the prominence of collective action problems, and the prospects for using political authority to amplify asymmetries of power. Not all aspects of political life are subject to increasing returns. Furthermore, this article has highlighted the more specific features of political environments that are likely to influence the initiation and reinforcement of increasing returns processes. In short, this is fertile territory for developing new propositions about the conditions that facilitate or impede various types of political change.

Consider one example. A prominent theme in recent research in comparative political economy is "varieties of capitalism." Despite increasing international economic interdependence, which seems to generate pressures toward convergence, the advanced industrial societies continue to exhibit fundamental differences in their core institutional structures (Berger and Dore 1996; Hall 1999; Hollingsworth and Boyer 1997; Soskice 1999). To date, this literature has done a better job of identifying and describing the diversity than it has of explaining what generates and sustains it. Hall and Soskice (2000) have made an important step forward by emphasizing the role of institutional complementarities. The benefits of particular economic institutions and organizations are increased if they operate in an environment populated by specific kinds of institutions and organizations.

The "varieties of capitalism" analysis persuasively illuminates distinct equilibria in different economies, but it does not address how these distinct equilibria emerge. From the current analysis, one can easily see

why the elaborate production systems of modern economies would be subject to increasing returns. Start-up costs, not just for new firms but more fundamentally for the key organizations and institutions that link private actors, are enormous. Organizations, and the formal and informal arrangements (both public and private) that help structure their interactions, create densely linked institutional matrices. Economic and social organizations and political institutions (both basic constitutional arrangements and public policy frameworks) have coevolved over extended periods. Coordination effects are widespread; particular courses of action make sense because of anticipated actions of others in the system. Firms have developed sophisticated strategies suitable to the particular institutional matrix they confront, that is, tremendous amounts of learning by doing have occurred over time in these complex systems. In short, national economic systems are highly path dependent. They are likely to exhibit substantial resilience, even in the context of major exogenous shocks, such as recent changes in the global economy.

In addition to highlighting particular causal processes that generate or sustain positive feedback, increasing returns arguments also direct attention to hypotheses explicitly based on timing and sequence. Under conditions conducive to path dependence, the same event (e.g., an exogenous shock such as depression or war) may have a different effect depending on when in a sequence of events it occurs (Collier and Collier 1991; Ertman 1996). Skowronek (1993) persuasively argues that we cannot understand the opportunities, constraints, and demands a president faces without placing him within a sequence of presidencies that support or oppose the dominant coalition of a particular period. Path dependence arguments provide a stronger foundation for Tilly's (1984, 14) claim that "when things happen in a sequence affects how they happen."

This highlights the broader theoretical significance of path dependence arguments: They can help political scientists think more clearly and explicitly about the role of time, and history, in social analysis. This is crucial because some claim to be witnessing a "historic turn" in the social sciences (McDonald 1996), but there is much confusion about what such a turn might mean. For some, particular historical outcomes are of intrinsic interest. For many, historical analysis is essentially a method, a way to expand the universe of cases that one can use to illustrate supposedly general theoretical models. The claim here is quite different. We should turn to history because important aspects of social reality can best be comprehended as temporal processes. It is not the past per se but the unfolding of processes over time that is theoretically central.

The main properties of increasing returns processes provide considerable support for many of the key claims of "historical institutionalist" analyses in political science. The phrase is a fortunate one, as it captures two critical themes explored here. This work is historical because it recognizes that political development must be understood as a process that unfolds over time.

It is institutionalist because it stresses that many of the contemporary political implications of these temporal processes are embedded in institutions—whether formal rules, policy structures, or norms.

Of these two elements, the institutional side generally has received greatest attention. Despite notable exceptions (Katznelson 1997; Orren and Skowronek 1994; Skocpol 1992; Skowronek 1993; Thelen 1999), the significance of temporal processes in historical institutionalist analysis is often left implicit or downplayed. Nevertheless, empirical work in this tradition highlights the need to examine temporal processes in order to explain important political outcomes. Historical institutionalist scholarship often emphasizes critical moments in politics, distinctive developmental sequences, and the rigidities that make it difficult for social actors to escape from established paths.

Of course, recent works of historical institutionalism build on a tradition of attention to history in the social sciences. Particularly for those who want answers to critical questions that grow out of the experiences of real polities, the turn to history has been common. Issues of timing, sequence, and critical junctures figure prominently in this body of work. Among many such studies, Gerschenkron's (1962) study of industrialization and state-building and Lipset and Rokkan's (1967) analysis of the formation of party systems are two classic examples. Indeed, it is fair to ask whether incorporating the concepts of increasing returns and path dependence into the study of politics is akin to the man who discovered that he had been speaking prose all his life. Is path dependence merely a trendy name for old ideas?

Discussions of path dependence would be worthwhile even if they did no more than focus the attention of a fad-prone discipline on the insights and continuing relevance of this earlier body of work. Yet, there is every reason to believe that the concept can do more. Knowledge of the dynamics of increasing returns processes can greatly sharpen our understanding of why particular junctures (and which aspects of them) are critical and why timing often counts for so much in politics. Most of the earlier work was vague on this point (Pierson n.d.b), although a detailed literature review would be needed to document that claim. The specific characteristics of positive feedback provide a key to making sense of the complex mix of stability and bursts of change that characterize so many political processes. As just discussed, an investigation of increasing returns processes can generate sharper hypotheses, based on more explicit social mechanisms, about the sources of divergent paths and social inertia.

There are, of course, important difficulties with increasing returns arguments. Two require at least brief attention. The first is methodological, and it concerns the difficulty of testing hypotheses based on complex, path dependent arguments (Geddes 1997).²²

The "many variables, few cases" problem is worsened in path dependent arguments, which require one to evaluate sequences of several variables over time.

This need not pose particularly acute problems for studying outcomes when it is possible to generate many cases (e.g., the formation of interest groups). Collective action and the development of actors' mental maps of politics seem to be promising areas of study. The "few cases, many variables" problem does pose difficulties, however, for increasing returns arguments that operate at a more aggregated level. The need to generate more cases helps explain why comparative politics has always been a field that emphasizes critical junctures (Collier and Collier 1991). Counterfactual analysis is also emerging as an important tool for such studies (Tetlock and Belkin 1996). Furthermore, analysts can use our growing theoretical understanding of path dependent processes to generate more observable implications, for instance, by focusing on intermediate stages in the processes. As Geddes (1997) argues, there are ways to deal with the "small n" problem, but they demand careful research designs. Even careful designs may well be inadequate for anything but fairly simple increasing returns arguments.

A second problem concerns the danger that the increasing returns concept suggests an overly static view of the social world. To take the starkest illustration, Arthur's Polya urn processes all settle on a particular equilibrium and then essentially stop. Increasing returns processes seem to generate only brief moments of "punctuation" in a largely frozen social landscape. To many, the significance of path dependence is belied by the evident dynamism of social life.

This is a sensible and useful challenge (Thelen 1999). But path dependent analyses need not imply that a particular alternative is permanently locked in following the move onto a self-reinforcing path. Identifying self-reinforcing processes helps us understand why organizational and institutional practices are often extremely persistent—and this is crucial, because these continuities are a striking feature of the social world. Asserting that the social landscape can be permanently frozen hardly is credible, and that is not the claim. Change continues, but it is bounded change—until something erodes or swamps the mechanisms of reproduction that generate continuity. North (1990a, 98–9) summarizes the key point well: "At every step along the way there [are choices]—political and economic—that provide . . . real alternatives. Path dependence is a way to narrow conceptually the choice set and link decision making through time. It is not a story of inevitability in which the past neatly predicts the future." The claims in path dependent arguments are that previously viable options may be foreclosed in the aftermath of a sustained period of positive feedback, and cumulative commitments on the existing path will often make change difficult and will condition the form in which new branchings will occur.

Indeed, as recently emphasized by Mahoney (n.d.) and Thelen (1999), identifying the particular feedback loops (or "mechanisms of reproduction") at work often provides key insights into the kinds of events or

²² It should be noted, however, that mainstream statistical research will also face difficult challenges if path dependent processes are common, since many quantitative techniques rest on the assumption that they are not (Jackson 1996).

processes that might generate major subsequent change points. Such junctures are usually attributed, often ex post, to "exogenous shocks." We should expect, however, that these change points often occur when new conditions disrupt or overwhelm the specific mechanisms that previously reproduced the existing

Increasing returns arguments open up an exciting research agenda for political science. In addition, an understanding of these processes can make another contribution to political scientists: a healthy dose of humility. Since the rise of behaviorism, many political scientists have had lofty aspirations about developing a science of politics, rooted in parsimony and generalization and capable of great predictive power. Despite modest achievements over four decades, these aspirations remain. Setbacks are shrugged off with calls for more time or more sustained application of the proper methods, but the inability to generate powerful generalizations that facilitate prediction remains a puzzle. If the prevalence of increasing returns processes is indeed a defining feature of politics, then we have been looking in the wrong place for an explanation. The main problem lies in the character of the political world itself.

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Institutional Change in the House of Representatives, 1867–1998: A Test of Partisan and Ideological Power Balance Models

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examine competing explanations for House rules changes with significant partisan overtones. I sought to identify all rules changes adopted from 1867 to 1998 that were intended either to advantage or to undermine the majority party and its leaders in their efforts to shape the House agenda. I test a majority party cartel model of institutional change against a model that focuses on the ideological balance of power on the floor, that is, on the closeness of the median voter to the median member of the majority and minority parties. I also evaluate the conditional party government approach. The data analysis suggests the preeminent unportance of shifts in the ideological balance. Two variables identified by the conditional party government theory, party polarization and party capacity, obtain limited support, but their effect is neither as robust nor as large in magnitude as that of change in the median voter's position.

representatives. The rise of Newt Gingrich (R-GA) to the speakership, the machine-like control exercised by House Republicans for much of the 104th Congress, and the loud complaints of minority party Democrats (echoing the protests of House Republicans when they were a frustrated minority) all contribute to a growing sense that the House is aptly characterized in terms of majority party government.

The Republican experiment with strong party government buttresses Davidson's (1986) observation that Congress is a "moving target." Core features of legislative organization continually change, such that the House of the mid- to late 1990s, with its centralized party leadership, weak standing committees, and modest respect for seniority, is a very different institution from the House of the middle years of this century, which featured weaker party leaders, powerful standing committees, and considerable deference to seniority.¹

This variability raises the question of how to understand change in legislative institutions. In recent years, political scientists have developed important new the-

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¹ For important characterizations of the mid-twentieth-century Congress, see Polsby 1968; Fenno 1965, 1973, and Shepale 1989. The difficulties experienced by House Republicans and their leaders in the 105th Congress (1997–98), when a coup attempt and other manifestations of internecine warfare embarrassed the party, suggest that the current revival of vibrant party leadership may turn out to be short lived (Cohen and Baumann 1998, Connelly and Pitney 1997; Mitchell 1998).

ories that highlight the role of the majority party as a procedural coalition governing House organization.² Two leading theories are the majority party cartel model elaborated by Cox and McCubbins (1993, 1994) and the conditional party government perspective developed by Rohde (1991), Aldrich and Rohde (1995, 1998), Binder (1996, 1997), and Dion (1997).

Cox and McCubbins argue that legislative institutions—at least in the case of the House—are designed and controlled by the majority party. They write (1993, 278) that the majority party manages to "usurp the rule-making power of the House" and uses that power to create institutions that best achieve party members' shared interests. Their thesis (1994, 218) is that majority party caucus rules "dictate that all members of the caucus are bound, if they wish to retain their membership, to support caucus decisions in the House on a variety of key structural matters—such as the election of the Speaker and the design and staffing of the committee system." As a result, "constitutional change of the House rules" cannot be obtained without the support of a majority of the caucus (1994, 223). The Cox and McCubbins theory leads to the substantively important prediction that the majority party will design rules and procedures that generally allow it to pull policy outcomes away from the floor median and toward the majority party's median position.3 Their work points to a dramatic reassessment of the so-called textbook Congress of the mid-twentieth century, suggesting that majority party Democrats exerted far more influence over legislative organization and policy outcomes throughout this period than most scholars have recognized.

Conditional party government theory also emphasizes the majority party's ability to shape legislative institutions, but advocates of this theory highlight variation in the degree of partisan control. Aldrich and

² See Jones 1968 on the concept of a procedural coalition. See also Froman and Ripley 1965 on the distinction between procedural and substantive votes.

³ As Groseclose and King (1997) point out, Cox and McCubbins never claim that the equilibrium policy in a one-dimensional framework will equal the median of the majority caucus, but they strongly suggest that policies often will be pulled toward the majority party median.

Rohde (1998) contend that the majority party's influence varies as a function of the party's internal homogeneity and the degree of polarization in the majority and minority parties' positions. An internally unified majority party that favors policies markedly different from those favored by the minority will be the most likely to enact rules changes that strengthen its agenda control (see also Cooper and Brady 1981). Binder (1996, 1997) agrees with Aldrich and Rohde that the majority party's capacity to change the rules varies across time. She argues that the relative unity and size of the two parties determine the ability of each to adopt rules changes that alter the legislative process in its favor. A large, unified majority party will be most likely to tighten its agenda control. Dion (1997), by contrast, contends that small majorities tend to be more cohesive and therefore are most likely to adopt rules changes that limit minority obstruction.

Although these models differ in significant respects, Cox and McCubbins (1997, 1376) rightly observe "something of a consensus" among scholars who have elaborated party-based theories: As the majority party becomes more homogeneous, rules changes that enhance its leaders' agenda control become more likely. When majority party members agree on basic policy stands—that is, when they have homogeneous preferences—they will have stronger incentives to give the party and its leaders increased control over the legislative agenda and decision making. By contrast, when there is disagreement on major policy issues, they will have incentives to give fewer tools to the party and its leaders.4

In this article, I develop and test an alternative explanation for House rules changes. I argue that shifts in the ideological balance of power on the floor, rather than change in the internal characteristics of the majority party, is the key determinant of changes in House rules. In this model, the median legislator on the floor is the pivotal decision maker. When she moves closer to the median member of the majority party, she will favor rules changes that enhance the majority party's agenda control. By contrast, when she moves closer to the median member of the minority party, she will favor rules changes that limit the majority party's agenda control and that instead create more opportunities for the minority party and cross-party coalitions.

The ideological balance of power approach draws upon Krehbiel's (1991, 1996, 1998) nonpartisan model of legislative institutions in emphasizing the critical influence of the median voter on legislative procedures and outcomes (see Black 1958). Yet, the ideological

power balance model is not entirely nonpartisan. Instead, it accepts the notion that some allocation of agenda power to political parties is necessary for a legislature to function effectively (see Cooper 1975, 1981). The key question is how much of that power will be allocated to the majority party and to the minority party and other competing power centers. I argue that changes in rules that determine the allocation depend on the ideological balance of power on the floor, that is, on the closeness of the median voter on the floor to the two parties. Dynamics on the floor, rather than the interests and deliberations of the majority party caucus, determine House rules changes.⁵

The median voter is pivotal for decisions about House rules because the Constitution gives the House as a whole the power to make its own rules. By longstanding precedent, a set of rules must be adopted at the start of each Congress.6 This gives a majority of members on the floor the opportunity to alter the rules to suit that majority's preferences. The ideological balance of power model is premised on the idea that the floor median will oppose rules changes that pull outcomes away from her favored policies and will instead seek changes that bring outcomes more closely in line with her preferences. Cross-party ideological coalitions therefore have the opportunity, and at times the incentive, to unite and pursue their policy goals through House rules changes that undermine the agenda control of the majority party and its leaders. Numerous significant rules changes, such as those in 1910, 1924, 1945, and 1967, were the work of crossparty coalitions operating against the wishes of most majority members.

Proponents of partisan models readily acknowledge that the floor median constrains what the majority party can accomplish (see, e.g., Aldrich and Rohde 1998; Cox and McCubbins 1994), but the distinctive contribution of party-based models to our understanding of House rules changes is their focus on majority party homogeneity. Given the widespread agreement on the homogeneity hypothesis, if it turns out that change in majority party homogeneity has little effect on adoption of House rules, then the explanatory power of these theories would be called into question. Furthermore, if change in the floor median has a substantial effect on adoption of House rules, then we would have new evidence of the applicability of the

⁴ An exception to this consensus is Flnk and Humes (1997), who argue that rules changes are least likely when there is a cohesive majority party, because it will not need rules changes to enact its policies. Their model makes predictions about the frequency of changes but not about their direction (in terms of strengthening or weakening majority leaders). Since the dependent variable in this article codes the direction of changes rather than their frequency, it does not provide a fair test for Fink and Humes. For similar reasons, I do not test the claim by Ainsworth and Sened (1998) that rules changes are most likely when turnover is high. They also make predictions about the frequency but not the direction of rules changes

⁵ The ideological power balance model thus departs from Krehblel by treating the allocation of agenda power to the majority party as a key variable that can affect policy outcomes. One might ask why the median voter does not create a new, centrist party and allocate agenda power to it. But creating a third party is likely to be costly for centrist members, particularly in the United States, where the first-past-the-post electoral system disadvantages smaller parties. Altering legislative rules is likely a less costly method for centrist legislators to shift the balance of agenda power in the desired direction.

From 1860 to 1890, House rules stated that the rules of the preceding Congress remain in force until amended by the new Congress. Even during this early period, it was generally understood that the Constitution gave the incoming Congress the right to adopt new rules without being bound by any preexisting rules (Alexander 1916; Damon 1971). Furthermore, House members still had the opportunity to consider new rules at the start of each Congress.

unidimensional spatial model (Black 1958; Downs 1957) to the choice of legislative institutions. Contrary to the fear that legislative institutions are likely to inherit the potential for "endless cycling" inherent in a multidimensional policy space (see Riker 1980), the ideological power balance model suggests that changes in House rules follow a predictable and orderly pattern: As the floor median (measured along a single, ideological dimension) moves toward the majority party, rules changes in favor of that party are more likely; when the median moves away from the majority party, rules changes that favor the minority party are more likely.⁷

Tests of competing models of legislative organization confront two major problems: It often proves difficult to operationalize the causal variables identified by each approach, and it is not always clear what each theory implies for many specific facets of legislative organization (Groseclose and King 1997). As a result, in the often fierce debate among advocates of the competing theories (Cox and McCubbins 1993, 1997; Krehbiel 1993; Rohde 1991, 1994; Schickler and Rich 1997a, 1997b; Sinclair 1994, 1998), few studies develop quantitative measures of change in the key variables of interest over a long period and relate those variables systematically to changes in legislative institutions. Binder (1996) is an important exception, but she does not test her conditional party government model against either the party cartel theory of Cox and McCubbins or against an approach based on the floor median (see also Fink and Humes 1997).

After clarifying the implications of the party cartel model, conditional party government theories, and the ideological balance of power model for understanding House rule changes, I devise measures that allow for a systematic test of how well each approach explains this feature of legislative organization. In constructing the dependent variable, I sought to identify all rules changes with significant partisan overtones adopted by the House from 1867 to 1998. I focus on this particular facet of institutional development not only because rules changes with partisan overtones have played an important role in congressional history but also because each theory makes relatively clear predictions about when such changes will occur. Furthermore, partisan rules changes provide a difficult test for the ideological balance of power approach, since one might well expect a party government model to fare best in explaining changes that are conspicuously partisan.

The data analysis shows that the ideological power balance model fares quite well in explaining partisan rules changes. By contrast, the key explanatory variable derived from partisan theories, change in the homogeneity of majority party members' policy views, does not have a statistically significant effect on rules changes. Similarly, the ideological balance of power approach

outperforms more elaborate conditional party government models, although changes in party polarization have had a significant effect as well, particularly in recent years.

On the whole, these findings are an important counterpoint to the increasing scholarly emphasis on majority party government that was sparked by the revival in party voting in the 1980s–90s (Cox and McCubbins 1993; Rohde 1991; Sinclair 1995). The results suggest that change in the ideological balance of power on the floor, as measured by the floor median, offers a better explanation of House rules changes than do the internal characteristics of the majority party. Notwithstanding this relatively straightforward finding, median voter models do not provide the complete story when it comes to institutional change. Therefore, I conclude by noting some limitations of the ideological balance of power model.

HYPOTHESES AND MEASURES

Between 1867 and 1998 there were several dramatic changes in House rules: the moves to eliminate minority obstruction in the 1890s; the destruction of the Speaker's so-called Czar Rule in 1909–10; the adoption of the 21-day rule in 1949 and again in 1965, which helped liberals overcome Rules Committee opposition; and the reforms adopted by the Democrats in the 1970s that bolstered majority leaders' agenda control.

The data set analyzed in this article includes any alterations in rules that were intended either to advantage or to undermine the majority party and its leaders in their efforts to shape the House agenda. Appendix A lists these changes. An example of a rules change intended to benefit the majority party is the Republican move in the 43d Congress (1873-75) to limit the use of dilatory motions. This paved the way for the Civil Rights Act of 1875, a top priority for the majority party (Damon 1971). The 1924 decision to liberalize the discharge rule is an example of a reform intended, at least in part, to undermine majority party leaders' agenda control. That decision made it easier for minority party Democrats and dissident Republicans to force the consideration of measures opposed by GOP leaders (Cooper [1960] 1988). By contrast, the Legislative Reorganization Act of 1946, which was championed by a broad, bipartisan coalition, exemplifies a change that, although important, does not qualify for consideration here because there is no evidence that members viewed the act in even remotely partisan terms (Cooper [1960] 1988; Davidson 1990). I excluded nonpartisan changes because neither party-based theories nor the ideological balance of power model make clear predictions about when these should occur.8

⁷ Earlier studies have demonstrated the importance of the median voter for congressional decisions concerning specific legislation (see, e.g., Krehbiel 1996; Krehbiel and Rivers 1988), but previous work has not assessed the explanatory power of the floor median with respect to changes in House rules

Nonpartisan rules changes do not shift the balance of agenda control in favor of one party or the other. As a result, the central premise of the ideological power balance model—that shifts in the floor median toward the majority or minority party lead to rules changes in that party's favor—does not translate into a prediction about nonpartisan rules changes. Extending the ideological power balance model (and party-based models) to address nonpartisan rules changes would be a worthwhile next step.

Each Congress between 1867 and 1998 is coded to reflect the pro- or antimajority party tenor of its rules changes. The resulting dependent variable is scored as follows:

- +1, if rules changes were adopted in a given Congress that, on balance, were intended to advantage the majority party and its leaders in their efforts to shape the House agenda;
- -1, if rules changes were adopted in a given Congress that, on balance, were intended to undermine the majority party and its leaders' efforts to shape the House agenda; and
- if no rules changes were adopted in a given Congress, or if the changes adopted were not, on balance, intended either to strengthen or to undermine the majority party and its leaders' agenda control.

I relied on leading sources on congressional rules and procedures to identify and classify rules changes (Alexander 1916; Cooper [1960] 1988; Damon 1971; Galloway and Wise 1976; Hasbrouck 1927).9 I also consulted the debate in the Congressional Record on specific rules changes when evidence from these secondary sources proved inconclusive. In classifying each change, I focused on the intent of its advocates, along with the composition of the coalition that adopted it.¹⁰ I classified rules changes in 21 of 66 Congresses as favoring the majority party and 8 of 66 as disadvantaging the majority party; in the remaining 37 cases there were no rules changes or changes that promised no net alteration in the majority party's degree of agenda control. Although Appendix A only lists Congresses in which partisan rules changes were made, the data set includes all Congresses in this period.

The rules changes analyzed in this article had a substantial influence on the degree of agenda control exercised by the majority party and its leaders. Advocates of partisan theories rightly claim that House rules since the mid-nineteenth century have always included some advantages for the majority party (Cox and McCubbins 1997),¹¹ but this by no means minimizes the significance of the variations in majority party agenda control during this period. For example, the prerogatives available to majority party leaders were greatly enhanced by the adoption of Reed's rules in 1890, just as they were significantly diminished in the aftermath of the 1910 revolt against Speaker Joseph Cannon (Cooper [1960] 1988; Peters 1997). These were

⁹ The Congressional Quarterly Almanac also proved helpful for the 1945–98 period. not simply minor changes that only marginally affected agenda control. Instead, they are just two of many cases that significantly altered the balance in control between the majority party and competing coalitions. Indeed, the changes listed in Appendix A were typically the subject of fierce floor fights that highlighted the considerable stakes involved. ¹² It is therefore appropriate to expect a partisan theory of legislative organization to account for these changes. ¹³

To check the robustness of the results, I replicated all the analyses using Binder's (1996) list of rules changes in her article on minority obstruction. Binder focuses on the creation and suppression of minority rights, so her case selection criteria are somewhat narrower than mine. She omits changes intended to advantage one party that are unrelated to questions of minority rights. An example is the 1945 rule granting standing committee status to the House Un-American Activities Committee. Rather than add to the official list of minority prerogatives, this rule created a new committee power base. Nonetheless, it undermined the ability of majority party leaders to submerge the issue of communist subversion, which provided clear benefits for the minority Republicans.

My case selection criteria, therefore, have the advantage of encompassing a broader range of rules changes than do Binder's criteria. At the same time, by combining a relatively diverse set of changes, I risk undermining the extent to which each of the coding categories constitutes a single class. It turns out that the results obtained from analyzing Binder's list of changes are extremely similar to the estimates obtained from analyzing the list in Appendix A. This makes it unlikely that the findings reported here are compromised by the coding scheme. (See Appendix A for a description of the coding differences between Binder's variable and the dependent variable employed here and for a discussion of a handful of difficult coding decisions.)¹⁴

¹⁰ The antimajority party changes did not always grant new minority rights. For example, repeal of the 21-day rule in 1951 strengthened the Rules Committee, rather than vesting new powers in the minority, but the committee was a power base for the cross-party conservative coalition in this period (Lapham 1954). Members believed that strengthening Rules would weaken majority leaders and help minority party Republicans. More generally, changes that weaken majority leaders' agenda control tend to open up more opportunities for cross-party coalitions. I code such a change as antimajority party when floor debate and specialized sources suggest that members believed it would advantage minority party members at the expense of majority party leaders.

¹¹ For example, with few exceptions, majority party members have monopolized committee chairmanships during this period.

¹² See, for example, the floor debate on the Reed rules (Congressional Record, January 29, 1890, pp. 948–60), the overthrow of Speaker Cannon (March 17, 1910, pp. 3292–334), the creation of the House Un-American Activities Committee (January 3, 1945, pp. 11–4), the repeal of the 21-day rule in 1951 (January 3, 1951, pp. 9–18), and the "packing" of the Rules Committee in 1961 (January 31, 1961, pp. 1573–90).

¹³ According to Cox and McCubbins (1994, 221), their theory suggests that "caucus members are obliged to vote with the majority of the caucus on specified House votes, including . . . the adoption of key House rules . . . Key house rules are (i) those designating the powers of the [speaker and committee charmen], such as the speaker's agenda-setting powers, the agenda-setting powers of committee and subcommittee charpersons, and the jurisdictions of the committees, and (ii) those specifying how House rules can be changed." The rules changes listed in Appendix A typically had important effects on the agenda powers of the Speaker and of committees or on committee jurisdictions. Therefore, these changes clearly fit within the domain of the Cox and McCubbins theory.

¹⁴ One limitation of the dependent variable is that changes that differ in importance—such as adoption of Reed rules in 1890 and a less monumental 1925 change curbing the discharge process—are each coded as +1 (or −1, in the case of prominority changes). Thus limitation makes it impossible to know whether the higher number of promajority than prominority changes observed in the data set means that the net advantage of the majority party has increased over time. Previous work on House rules has shared this problem (see, e.g.,

Both versions of the dependent variable exclude changes in party rules as opposed to House rules. For example, neither dependent variable includes the changes in party rules adopted by the Democratic Caucus in the 1970s. I exclude party rules because there is little reason to expect the same dynamics to characterize changes in these and in House rules. The latter must be approved by a majority on the House floor; the former require only a majority of the caucus. The finding that the floor median is the key determinant of House rules changes leaves open the question of whether the majority party is able to use caucus rules changes as an alternative mechanism to achieve its goals (see Rohde 1991, and discussion below). Again, this study is intended to test the usefulness of partisan and ideological power balance explanations for one important domain of legislative institutions, the adoption of House rules. It is not intended to demonstrate that a single model can explain all elements of legislative organization.15

To construct the independent variables, I rely heavily on Poole and Rosenthal's DW-NOMINATE scores (see McCarty, Poole, and Rosenthal 1997). NOMINATE scores are the most commonly used measure for members' ideological positions. They are based on all nonunanimous roll call votes taken in each Congress. DW-NOMINATE scores have the virtue of comparability across time. Although critics have pointed out several shortcomings (Heckman and Snyder 1996), NOMINATE scores offer a reasonably good measurement of members' ideological positions. Therefore, I use first-dimension DW-NOMINATE scores to calculate the location of the floor and party medians as well as party homogeneity.

The first hypothesis that I test from partisan theories reflects the near-consensus position articulated by Cox and McCubbins (1997, 1385–6): "If one wants to examine rules changes from the perspective of a partisan theory, one has to look systematically at changes in the homogeneity of the majority caucus. One expects decreasing homogeneity to lead to fewer institutional powers for majority party leaders, increasing homogeneity to lead to greater powers." I infer from Cox and McCubbins:

HYPOTHESIS 1: CHANGE IN HOMOGENEITY. Rules changes that enhance the agenda control of the majority party and its leaders are more likely as the majority party becomes increasingly homogeneous. Rules changes that reduce such majority party advantages are more likely as the party becomes more heterogeneous.

Binder 1996). A promising direction for future research would be to ask a group of experts to rank the importance of each rules change, which could yield more refined measures.

Following Aldrich and Rohde (1998, 13), I operationalize majority party homogeneity as the standard deviation of majority party members' first-dimension NOMINATE scores divided by the standard deviation of all House members' first-dimension NOMINATE scores (see Appendix B for a full definition of this and the other independent variables). I then compute change in that variable. Consistent with Cox and McCubbins (1997, 1381), when party control of the House changes hands, the homogeneity of the new majority party is compared to that of the outgoing majority party. Thus, the change in homogeneity variable for the 104th Congress, in which the GOP took control of the House, would be calculated as follows:

Δ HOMOGENEITY = GOP HOMOGENEITY_(104th)

- DEMOCRATIC HOMOGENEITY (103d)

where each party's homogeneity is the standard deviation of its members' first-dimension DW-NOMINATE scores divided by the standard deviation of all House members' first-dimension scores. Since a high standard deviation is associated with a low level of party homogeneity, this variable is expected to have a negative sign when predicting partisan rules changes.¹⁷

Figure 1 shows the majority party homogeneity scores for 1867–1998. This graph looks quite similar to what a close observer of congressional history might expect. For example, the majority party standard deviation is fairly high in the 1920s, an era of Republican majorities beset by significant internal divisions. After a brief fall during the early New Deal years, when Democrats voted with unusual unity, the standard deviation begins a substantial rise in 1939 and peaks in 1967, when majority party heterogeneity appears to have reached an historical high. This corresponds to the era of deepest divisions among southern and nonsouthern Democrats. The steadily declining standard deviation after 1970 reflects the well-documented easing of Democratic divisions in the 1970s and 1980s (Rohde 1991). This variable seems, therefore, to be a reasonably good measure for majority party homogeneity.¹⁸ It is plausible, however, that homogeneity ought to be considered in terms of multiple dimensions (Cooper and Young 1997; Fink and Humes 1997). In response to this potential concern, I replicated each analysis substituting a measure of homogeneity based on both NOMINATE dimensions (see Appendix B). This substitution had little effect on the results.

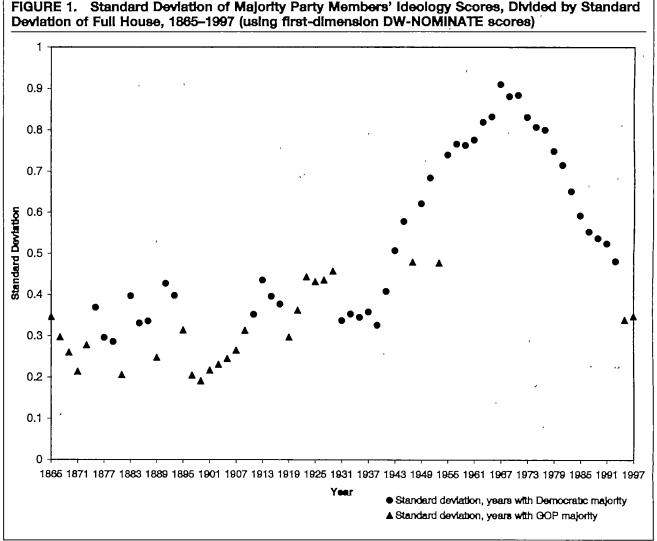
A second prediction that emerges from the Cox and McCubbins model is that when party control of the House changes hands, partisan rules changes are likely.

¹⁵ The most noteworthy changes in caucus rules in recent decades were adopted by the majority Democrats in 1971–75 (Rohde 1991) Interestingly, all those Congresses are coded as having promajority changes (see Appendix A). This suggests, albeit tentatively, that extensive use of the majority party caucus and promajority rules changes tend to go together, rather than primarily substituting for one another.

¹⁶ More precisely, votes are excluded in which fewer than 2.5% of the members voting are on the loging side.

¹⁷ A simpler definition of homogeneity uses the majority party's standard deviation without dividing by the standard deviation for the full House. When this alternative measure is used, the results are substantively identical to those reported below. I choose the more complicated operationalization because it corrects for changes in the scale as the entire rank-and-file become more or less spread out (see Aldrich and Rohde 1998).

¹⁸ This is not to say that DW-NOMINATEs are a perfect measure for homogeneity (see Appendix B for a discussion of some limitations of this measure).



If the Democrats, while in the majority, stack the rules to favor their policies, then a new Republican majority should reverse these provisions and adopt rules that help promote GOP priorities. One might well interpret several of the changes adopted in 1995 by the new Republican Congress in these terms.

Cox and McCubbins (1993, 1997) are careful to point out that institutional arrangements geared toward promoting specific policies are by no means the only type of deck-stacking that the majority party is likely to employ. As a result, partisan rules changes are not inevitable whenever party control of the House changes hands. Nonetheless, Cox and McCubbins's discussion (1994, 220) of the role of the "judicious allocation of committee power" in upholding majority party logrolls suggests that partisan deck-stacking often does have a policy-specific element (see also 1993, 248-57). A classic nineteenth-century example is the Holman rule, which was repeatedly adopted by Democratic Congresses in the post-Reconstruction era. It allowed expenditure-reducing riders to appropriations bills and was an important Democratic weapon in the party's battles with Republican presidents (Kiewiet and

McCubbins 1991; Stewart 1989).19 Therefore, the second hypothesis tested is:

Hypothesis 2: Change in Party Control. A switch in majority party control of the House is likely to lead to rules changes in the new majority party's favor.

The conditional party government approach of Binder (1996), Dion (1997), and Rohde (1991) generates three additional hypotheses. The first of these is derived from Binder:

Hypothesis 3: Party Capacity. The stronger the majority party relative to the minority party, the more likely are rules changes that advantage the majority party. The weaker the majority party relative to the minority, the more likely are changes that disadvantage the majority party.

¹⁹ Notice that changes in party control should have less effect when Binder's list of rules changes is the dependent variable. The reason is that Binder's focus on rules affecting minority rights excludes most cases of policy-specific deck-stacking. My criteria, however, include several cases that arguably fit that description.

Following Binder, party capacity is operationalized as a function of the relative homogeneity of the two parties and the share of seats held by each. The basic idea is that if the majority party is highly unified and has a large share of House seats, then its capacity to impose rules changes is higher than if it is internally divided and has only a slim majority. Binder (1996, 13) uses each party's Rice cohesion score as a measure of its homogeneity, and for each Congress she multiplies this score by the percentage of seats that the party holds to gauge its strength. Party capacity is the difference obtained when minority party strength is subtracted from the majority party's strength. For purposes of comparability, I constructed a measure of partisan capacity using the standard deviation of each party's members' first-dimension DW-NOMINATE scores in place of the Rice cohesion scores (see Appendix B for a precise definition of both measures).

The party capacity hypothesis focuses on the observed level of party capacity in Congress t, not on the change between Congresses t and t-1. One might argue that change scores are the more appropriate variable to use when predicting rules changes (see Cox and McCubbins 1997, 1385); furthermore, the hypotheses about homogeneity and the ideological balance of power are stated and tested in terms of change scores. I estimate separate models using either change scores or observed levels of party capacity as an independent variable.²⁰

Binder's concept of party capacity has much in common with the notion of the ideological balance of power on the floor. When the majority party has a large edge and is highly unified, it seems likely that the median voter on the floor will be quite close to the majority party median; when the majority party is divided relative to the minority and has only a slim margin, it is more likely that the floor median will be distant from the majority party median.²¹ But Binder's theory and the median voter approach nonetheless identify different causal mechanisms for rules changes. In Binder's view, the characteristics of the parties themselves—their unity and size—determine House rules changes pertaining to minority rights. By contrast, the ideological power balance model holds that the position of the median voter on the floor is critical. Put simply, the median voter is identified as the key decision maker by the ideological power balance approach, whereas the parties have a more prominent role in determining institutional rules in Binder's model.

In contrast to Binder, who argues that the majority party is more likely to pass rules changes that increase its agenda control when it has a substantial majority, Dion (1997) asserts that small majorities are most likely to be cohesive and to limit minority rights. Because Dion focuses on the nineteenth century, which was the era of greatest minority obstruction, it may not be fair to test his model with data that extend into the late twentieth century. Nonetheless, the fourth hypothesis is:

HYPOTHESIS 4: MAJORITY SIZE. As the majority party's share of seats becomes smaller, rules changes that advantage the majority party are likely.²²

The fifth hypothesis is derived from Rohde's (1991) work on conditional party government:

Hypothesis 5: Change in Party Polarization. As the majority party median moves farther from the minority party median, rules changes that advantage the majority party are likely.

The Rohde hypothesis is premised on the idea that when the majority and minority party are far apart in their policy preferences—that is, when there is a high level of party polarization—the majority party has more incentive to enact rules that enhance its agenda control (see also Aldrich and Rohde 1998). When party conflict over major issues is low, the majority party has less reason to shut out the minority, and it can allow more cooperative, bipartisan arrangements. In support of this argument, Rohde (1991) traces how the House Democratic majority tightened its grip on the legislative process in the 1970s and 1980s as Democrats and Republicans moved farther apart in their ideological positions. I measure change in polarization by computing the difference between the two party medians and then assessing change in that variable from Congress t-1 to Congress t.

In the ideological balance of power model, the key to partisan rules changes is the location of the floor median relative to the two parties. The model suggests that the median voter will confer a greater share of agenda control on the majority party when she moves closer to the median ideological position of majority party members. The floor median will be more willing to work with the minority to undercut the majority party's agenda control when she moves farther from the majority party median and closer to the minority party median. Thus, the sixth hypothesis is:

Hypothesis 6: Change in Ideological Power Balance. Rules changes that advantage the majority party and its leaders are more likely when the floor median moves in the direction favoring the majority party. Rules changes that reduce majority party advantages are more likely when the floor median moves in the direction favoring the minority party.

²⁰ Using observed levels of party capacity would be problematic if that variable were nonstationary (Kennedy 1994). But Dickey-Fuller and Phillips-Perron tests both reject the null hypothesis of a unit root at p < .05, and visual examination of the party capacity time series and autocorrelation function suggest the series is stationary.

²¹ That said, the correlation between Binder's measure of party capacity and the location of the median voter (relative to the majority and minority party-medians) is just 36. Her measure correlates more strongly with change in the location of the median voter, however, which suggests that multicolmearity may hinder hypothesis testing when change scores are used. The low standard errors in the models that include both the floor median and party capacity variables indicate that this is not a major problem (see Table 3).

²² A strong case can be made that the Dion hypothesis is more appropriately tested with respect to Binder's list of rules changes, since that list is confined to rules affecting minority rights. The hypothesis receives little support, however, regardless of which dependent variable is used.

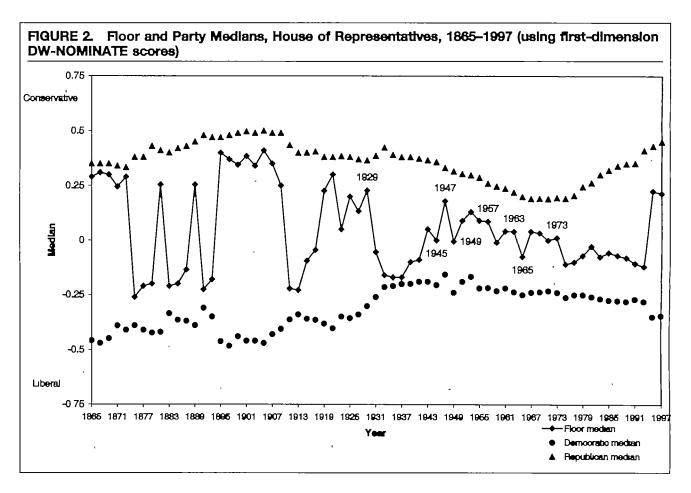


Figure 2 graphs movement in the floor and party medians over time. For each Congress, progressive or liberal positions are associated with negative values for NOMINATE scores, and conservative positions are associated with positive values.²³ Note also that the Democratic median is always negative, and the GOP median is always positive. Once again, shifts in the floor median for the most part make substantive sense. For example, the big Democratic seat gains following the 1930, 1948, 1958, 1964, and 1974 elections each led to substantial shifts in the floor median to the left, as Republican gains in 1946, 1950, and 1966 led to marked shifts to the right. (The positions just before each of these election years are highlighted in the body of Figure 2.)

Assessing the ideological balance of power on the floor requires measuring movement in the floor median toward the majority or minority party. This task is complicated by switches in party control. A movement of the floor median in the positive direction (e.g., from

-.10 to +.10) has opposite implications for the majority party depending on whether it is the Republicans or Democrats. The measure that best deals with these complications is called " $\Delta MEDIAN_{ADV}$." It is defined as follows: Take the distance between the minority party median and the floor median, compare it to the distance between the majority party median and the floor median, and assess change in that variable between Congress t and Congress t-1.

$$\begin{split} \text{MEDIAN}_{\text{ADV}} &= \text{abs}(\text{MEDIAN}_{\text{floor}} - \text{MEDIAN}_{\text{min}}) \\ &- \text{abs}(\text{MEDIAN}_{\text{floor}} - \text{MEDIAN}_{\text{maj}}) \end{split}$$

$$\Delta MEDIAN_{ADV} = MEDIAN_{ADV(t)} - MEDIAN_{ADV(t-1)}$$

This variable denotes change in the relative proximity of the majority party median and the minority party median to the median legislator on the floor.²⁴ I experimented with alternative measures for change in the median and obtained results that are extremely

²³ The term "liberal" before the 1930s is somewhat anachronistic. The meaning of the NOMINATE scale has likely shifted over time. The data analysis uses change scores from one Congress to the next, and thus presupposes that changes in the scale do not occur within a two- to four-year period (e.g., from the beginning of the 98th Congress in January 1983 to the end of the 99th Congress four years later). Such an assumption is more plausible than the claim that the scale does not shift over more extensive time spans Poole and Rosenthal (1997) present evidence that the first NOMINATE dimension has, with rare exceptions, captured conflict concerning government intervention in the economy.

²⁴ The ideological power balance model assumes that a single ideological dimension is primary when it comes to partisan rules changes. Analysis of the roll call votes on the individual rules changes suggests that this is a reasonable assumption: First-dimension NOM-INATE scores are generally the strongest predictor of members' votes. On a few occasions the second dimension also is significant, as in the overthrow of Speaker Cannon in 1910. Even in that case, however, first-dimension NOMINATEs are a much stronger predictor of members' votes than are second-dimension scores (see Schickler and Sides n.d. for an analysis of a nonpartisan 1899 Senate rules change in which the second dimension was primary).

TABLE 1. Descriptive Stati	stics for l	ndepende	ent Variab	les				
	Full Se	ımple	Congress Net Antli Party Cl (n =	ses with najority nanges	Congree Which C Do Not One F (n =	hanges Benefit Party	Congress Net Pror Party Cl (n =	najority nanges
Varlable	Mean	Std. Dev.	Mean	Sta. Dev.	Mean	Sta. Dev.	Mean	Sta. Dev.
Δ MEDIAN_{ADV} (change in floor median proximity to party medians)	006 (.020)	.159	210 (.063)	.177	010 (.021)	.128	.081 (.028)	.131
Δ HOMOGENEITY (change in maj. party std. dev. divided by floor std. dev.)	.000 (.010)	.078	.025 (.025)	.071	.006 (.014)	.087	020 (.013)	.061
SWITCH _{DEM} (change to Democratic majority)	.106 (.038)	.310	.125 (.125)	.354	.108 (.052)	.315	.095 (.066)	.301
SWITCH _{REP} (change to Republican majority)	.106 (.038)	.310	0 (n/a)	0	.081 (.045)	.277	.190 (.088)	.402
ΔHOMOGENEITY IN TWO DIMENSIONS (change in avg. distance between majority party reps.)	.002 (.007)	.055	001 (.010)	.028	.004 (.014)	.063	001 (.013)	.052
Δ MAJPCT (change in majority party seat share)	004 (.010)	.082	081 (.026)	.073	003 (.013)	.080	.024 (.016)	.072
PARTY CAPACITY (Binder measure)	13.301 (1.058)	8.597	6.398 (1.877)	5.309	12.140 (1.464)	8.905	17.977 (1.432)	6.564
PARTY CAPACITY-DW (DW-NOMINATE measure)	1.243 (.217)	1.775	.063 (.288)	.8 15	1.291 (.299)	1.845	1.604 (.389)	1.781
Δ POLARIZATION (change in distance between party medians)	000 (.005)	.040	025 (.010)	.029	002 (.006)	.037	.013 (.010)	.045
Note: Standard error of the mean is in per	entheses.							

similar to the results reported below. (See Appendix B for a description of one such alternative measure and an example that illustrates how scores on the $\Delta \text{MEDIAN}_{\text{ADV}}$ variable are calculated.)

As noted above, partisan models do not necessarily predict that change in the median will have no effect (see, e.g., Aldrich and Rohde 1998, who readily acknowledge the importance of the floor median). Yet, for partisan theories to add to our understanding of House rules changes, the variables identified by those theories should have an influence net of change in the floor median.²⁵

RESULTS

Table 1 presents descriptive statistics for the variables used in the analysis and also shows the mean for each variable in Congresses with no rules changes, promajority changes, and antimajority changes. At the bivariate level, there does seem to be a relationship between change in the floor median and partisan rules changes. For example, in the eight Congresses in which antimajority changes occurred, the median shifted by an average of .210 (SE = .063) in favor of the minority party; in the twenty-one Congresses with promajority changes, the median shifted by an average of .081 (SE = .028) in favor of the majority party. A similar, although statistically weaker, relationship exists as well between changing majority party homogeneity and

²⁵ It is also worth noting that even in the present era of highly polarized parties, the ideological power balance and homogeneity hypotheses will often yield different predictions. For example, if the majority party loses 20 seats in the House, the floor median will almost certainly move in the minority's direction even if each party's homogeneity remains unchanged and the two parties' distributions of ideal points do not overlap. In that circumstance, the ideological power balance hypothesis leads one to expect a prominority party

rules change, while the homogeneity hypothesis leads one to expect no change.

Variable	Theory (expected sign)	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Δ MEDIAN_{ADV} (change in median)	Medlan voter (+)	7.968** (2.075)	8.014** (2.136)	7.676** (1.985)	7.630** (2.014)		9.426* (4.499)
AHOMOGENEITY (change In majority party std. dev. dlvlded by floor std. dev.)	Party cartel (-)	-1.518 (3.422)	-3.553 (4.510)		-	-9.577** (3.629)	.198 (5.746)
SWITCH _{DEM} (change to Democratic majority)	Party cartel (+)		.742 (.989)		886 (1.065)		•
SWITCH _{REP} (change to Republican majority)	Party cartel (+)		200 (1.035)		1.121 (1.148)		
ΔΗΟΜΟΘΕΝΕΙΤΥ IN TWO DIMENSIONS (change In avg. distance between majority party reps.)	Party cartel (-)			6.133 (5.435)	12.215 (7.438)		
Δ MAJPCT : (change In majority party seat share)						12.670** (3.694)	-2.989 (8.184)
INTERCEPT1		2.524 (.458)	2.481 (.479)	2.254 (.467)	2.259 (.495)	2.430 (.448)	2.520 (.458)
INTERCEPT2		982 (.307)	-1.041 (.337)	-1.057 (.348)	-1.116 (.390)	924 (.294)	982 (.307)
χ ² for covarlates (degrees of freedom) ρ value N		20.883 (2) .0001 66	21.351 (4) .0003 66	18.861 (2) .0001 54	20.237 (4) .0004 54	16.036 (2) .0003 66	21.022 (3) .0001 66

rules changes. The question is how well these relationships hold up in a multivariate analysis.

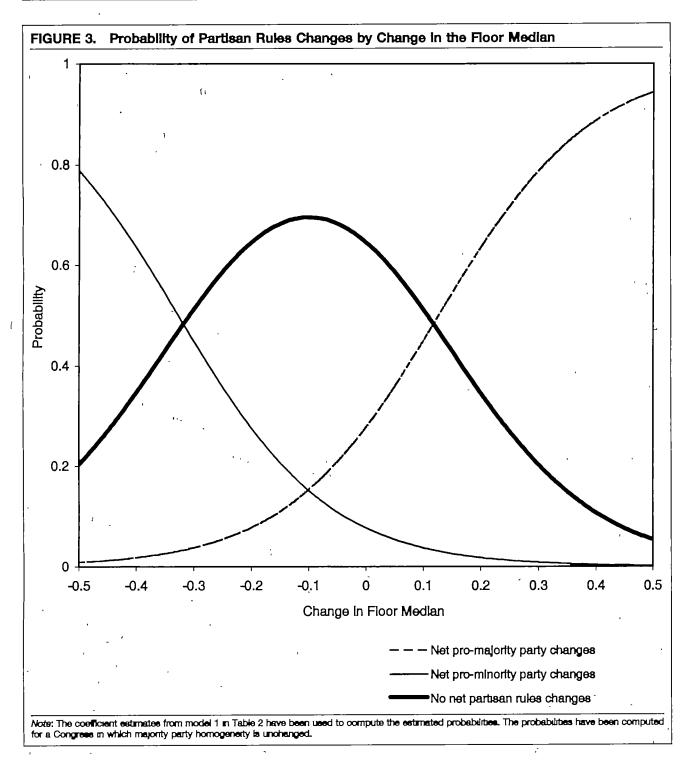
Since the dependent variable is a set of ordered categories, I use ordered logit to test the competing explanations of rules changes. Table 2 reports a series of models that assess the ideological power balance and party cartel models. Starting with a baseline model (labeled model 1), which includes change in party homogeneity and change in the floor median, the results are striking: The coefficient for change in homogeneity is small and insignificant (b = -1.518; SE = 3.422), while the effect of change in the floor median is of the correct sign and is both substantively and statistically significant (b = 7.968; SE = 2.075).

This first model, therefore, supports the ideological balance of power hypothesis and provides no evidence for the homogeneity hypothesis.

To understand these results better, it is helpful to use substantively interesting sample values for the variables in the model. Figure 3 takes a simple case in which the majority party's homogeneity is constant and shows the estimated effect of movement in the floor median on the probability of partisan rules changes. In such a situation, if there is no change in the floor median, the expected probability of a promajority rule change is .27, and the expected probability of an antimajority change is .07. If the floor median shifts by .25 in the majority party's direction—about the size of the shift toward the Democrats following the 1964 and 1974 elections—then the probability of a promajority rules change rises from .27 to .73. The probability of an antimajority change falls from .07 to just over .01. In fact, the 1964 and 1974 Democratic gains were both followed by promajority rules changes: the adoption of the 21-day rule to aid the passage of liberal legislation in 1965, and, among other changes, the elimination of

expected probability of a promajority rules change only slightly, from 27 to .32. By contrast, a similar increase in the median in favor of the majority party increases the probability of a promajority change from 27 to .82.

²⁶ The ordered logit models were estimated using SAS, version 6.12. The use of over-time data may produce problems of nonstationarity, autocorrelation, or heteroscedasticity. But the dependent variable and main independent variables are change scores, likely mitigating problems of nonstationarity and of autocorrelated residuals. Phillips-Perron and Dickey-Fuller tests on each variable reject the null hypothesis of a unit root at p < .05. Furthermore, when the analysis is replicated using OLS, the results are unchanged, and Durbin-Watson tests give no evidence for autocorrelated residuals. Finally, as Binder (1996, 16) argues with reference to her similar data set, "there are no theoretical reasons to expect ... autocorrelation or heteroscedasticity.... The historical record lends no evidence that past reforms have been mimicked by subsequent majorities or that there have been any learning effects from one Congress to another." ²⁷ If there is no change in the floor median, then an increase of two standard deviations in majority party homogeneity increases the



a committee staffing guarantee for the minority in 1975.

If the median shifts by .30 toward the minority party—about the size of the shift toward the Republicans following their strong, although not House-capturing, showing in the 1966 elections—then the likelihood of an antimajority rules change rises from .07 to .47, and the probability of a promajority change falls from .27 to .03. The 1966 GOP gains did lead to an antimajority rules change in 1967: A cross-party coalition uniting Republicans with conservative Democrats repealed the 21-day rule that had been passed in the

previous Congress. These examples illustrate the substantial effect of shifts in the floor median on partisan rules changes.

An important issue is the robustness of these results in response to plausible changes in model specification. First, when dummy variables for switches in party control are added to the model, there is no support for either the homogeneity or party control hypotheses, but change in the median remains significant. This is shown in model 2 of Table 2. Second, if the observed level of majority party homogeneity is used in place of change in homogeneity, the floor median continues to

be a strong, significant predictor, but the effect of party homogeneity is insignificant.28 Third, the results are again unaffected when I use a measure for change in homogeneity that is based on both NOMINATE dimensions, as opposed to just the first dimension (see models 3 and 4 of Table 2).29 Fourth, I altered the baseline model by adding a lagged value for the change in homogeneity variable. This move was based on the hypothesis that a change in homogeneity may not have an instantaneous effect because it may take time for majority party members to rally support for proposed reforms (see Rohde 1991). Even in this revised model, neither homogeneity variable had a significant influence, but change in the median continued to be significant.30 Finally, using Binder's list of rules changes as the dependent variable has little effect on the results. Those estimates are presented in Appendix C.31

Contrary to the predictions of the Cox and McCubbins majority party cartel model, neither changing party homogeneity nor changes in partisan control of the House appear to have a significant effect on partisan rules changes, but a shift in the floor median has a strong influence.³²

Some might question the validity of the measure for change in the floor median, as it is based on the admittedly imperfect DW-NOMINATE scores. A further robustness test of the model is to substitute change in the percentage of seats held by the majority party for change in the floor median. All else equal, as the majority party increases its share of seats, the ideological balance of power on the floor should shift in the party's favor. This specification also offers the opportunity to assess Dion's model. His theory suggests the majority party's seat share should have a negative effect on the adoption of partisan rules changes: The majority party is more likely to tighten its control when it has a slim majority.

When change in the share of seats held by the majority party is substituted for change in the floor median, both the homogeneity and seat share variables are significant (see model 5 of Table 2). The effect of the seat share variable is *positive*, as expected by the ideological balance of power model, but contrary to the expectations derived from Dion's theory.³³ Still, this

finding is consistent with some variants of conditional party government theory (Aldrich and Rohde 1998; Binder 1996). The key question is what happens in a model that contains the floor median as well as the majority party's seat share. Model 6 of Table 2 shows that, controlling for change in the floor median, neither the majority party's seat share nor majority party homogeneity has a significant effect on the likelihood of partisan rules changes. But the effect of change in the median remains strong and significant. Including the variable for change in the median also improves the fit of the model considerably, increasing the χ^2 statistic by 4.986 while sacrificing one degree of freedom (p < .05). The results reported in model 6 are unaffected by adding dummy variables for switches in party control.

It might be the case, however, that a more complicated conditional party government model would outperform the floor median as a predictor of rules changes. Table 3 presents a series of models that assess this possibility. Since Binder's (1996) test of her partisan model has justly been praised as convincing evidence for the conditional party government theory (see, e.g., Cox and McCubbins 1997), I estimated several models with the party capacity variable that Binder finds to be an important predictor of the adoption of rules concerning minority rights.

Model 1 of Table 3, which excludes the floor median variable, suggests that party capacity has a strong, significant influence on the adoption of partisan rules changes (b = .099; SE = .031). But as model 2 shows, when change in the floor median is added, the estimated effect of party capacity is cut substantially, although it remains just statistically significant at the .05 level (b = .058, SE = .035; p = .05). Change in the median is once again a strong and statistically significant predictor of rules changes, and its inclusion improves the predictive accuracy of the model considerably ($\chi^2 = 11.764$, 1 d.f., p < .01).

Although model 2 suggests that party capacity has a significant influence on the adoption of partisan rules changes when controlling for change in the median, the coefficient for party capacity is both lower in magnitude and much less robust than the coefficient for change in the median. In terms of magnitude, the standardized coefficient for change in the median is twice as large as that for party capacity (.58, as compared to .28). Furthermore, if Binder's list of rules changes is used as the dependent variable, as in model 3 of Table 3, party capacity is no longer statistically significant when change in the floor median is included (b = .043); SE = .039). Similarly, if change in party capacity is used as the independent variable in place of the observed level of party capacity (data not shown), the coefficient for party capacity is again insignificant. This holds

scores and only cover the 1879-1988 period.

The estimate for change in the median is 8.268 (SE = 2.064), and the estimate for homogeneity is 1.508 (SE = 1.272). The results are similar when the two-dimensional homogeneity measure is used. ²⁹ As noted in Appendix B, these two models use D-NOMINATE

⁵⁰ The estimate for change in homogeneity from Congress t-2 to Congress t-1 was -4.502 (SE = 3.576); the estimate for change in homogeneity from Congress t-1 to Congress t was -2.435 (SE = 3.586). The estimate for change in the median was 7.756 (SE = 2.107). The results were similar when I used the *level* of homogeneity in the current and preceding Congresses instead of change scores. ⁵¹ Binder's list of rules changes ends in 1990. The analysis using her list covers 1867-1990.

 $^{^{32}}$ These results are not attributable to collinearity. The correlation between change in the median and the one-dimensional measure for change in homogeneity is just -27. The correlation between change in the median and the two-dimensional measure for change in homogeneity is -31.

³³ Again, Dion restricts his discussion to rules affecting mimority rights, and he focuses on the nineteenth century, an era when

minority obstruction was particularly troublesome. This analysis is not a refutation of Dion's treatment of four cases of rules changes to limit minority obstruction. Instead, it suggests that Dion's theory cannot be extended to explain the broader set of changes analyzed here.

²⁴ The majority party seat share variable now is negatively signed, as one would expect from Dion, but it falls far short of statistical significance.

Changes, 1867-1998				Mandal O		Model 5	
Varlable	Theory (expected sign)	Model 1	Model 2	Model 3 (Binder list)	Model 4	(Binder list)	Model 6
Δ MEDIAN _{ADV} (change in median)	Median voter (+)		6.588** (2.164)	6.450** (2.213)	7.787** (2.079)	7.184 ** (2.109)	8.191** (2.168)
PARTY CAPACITY (Binder measure)	Cond. party govt. (+)	.0 9 9** (.031)	.058* (.035)	.043 (.039)			
PARTY CAPACITY-DW (DW-NOMINATE measure)	Cond. party govt. (+)				.131 (.15 6)	.108 (.171)	
ΔPOLARIZATION (change in distance between party medians)	Cond. party govt. (+)						13.890* (7.271)
Δ HOMOGENEITY (change in majority party std. dev. divided by floor std. dev.)	Cond. party govt. (-)					,	.019 (3.523)
INTERCEPT1		.934 (.490)	1.852 (.611)	2.314 (.679)	2.380 (.479)	2.692 (.547)	2.655 (.480)
INTERCEPT2		-2.264 (.561)	-1.809 (.623)	-2.149 (.694)	-1.152 (.373)	-1.683 (.437)	-1.030 (.319)
χ^2 for covariates (degrees of freedom) p value N		11.490 (1) .0007 66	23.254 (2) .0001 66	17.414 (2) .0002 60	21.419 (2) .0001 66	16.618 (2) .0002 60	24.788 (3) .0001 66

Note Standard errors are in parentheese, *p < 05, **p < .01; one-tailed tests. Models 3 and 5 use Binder's list of rules changes as the dependent

regardless of which list of rules changes is used as the dependent variable. Finally, if NOMINATE scores are used in place of Rice cohesion levels when calculating party capacity, the capacity variable is insignificant, controlling for change in the median. This again is the case regardless of which list of rules changes is used as the dependent variable (see models 4 and 5 of Table 3). Indeed, the only specification in which party capacity achieves statistical significance (when change in the floor median is included in the model) is model 2 of Table 3. Change in the median is statistically significant in each of these models.³⁵

vanable.

Finally, when Rohde's variant of conditional party government theory is assessed, the results support both the median voter model and the party polarization hypothesis. Model 6 of Table 3 includes change in homogeneity, change in party polarization, and change in the median. The homogeneity variable is small and insignificant, but both changes in party polarization

and changes in the median have a significant effect. In terms of magnitude, the standardized coefficient for change in the median is more than twice as large as that for change in polarization (.72, as compared to .31). When this analysis is replicated using Binder's list of rules changes, the effect of change in the median remains robust, but the polarization variable now is smaller and falls well short of statistical significance (b = 4.919; SE = 7.815). Nonetheless, the results in Table 3 suggest that party polarization may well have an important influence on the adoption of partisan rules changes. At the same time, it is worth emphasizing that the effect of change in the median is strong no matter which other variables are included in the model and no matter which dependent variable is used.³⁶

As a final robustness check, I split the time period in

³⁵ I also examined whether the Speaker's ideological distance from his party's median affects the adoption of partisan rules changes. Jones (1968) argues that if a party leader adopts positions that depart from the policy views of the coalition that elected him, then he will often face revolts that challenge his institutional power. Nonetheless, when this variable is included alongside change in homogeneity and change in the median, it has a small and insignificant effect, while the effect of change in the median remains robust. The Speaker distance variable is computed by comparing the Speaker's first-dimension NOMINATE score in the last Congress before he became Speaker with his party's median in the current Congress.

 $[\]infty$ One variable missing from the analysis is partisan need. Rules changes may be likely when majority party members' policy goals are stymied by rampant minority obstruction. One plausible measure for partisan need is the number of roll call votes in the preceding Congress (Binder 1996). When that variable is added to the baseline model (see Table 2, model 1), the estimates for the median and party homogeneity are unaffected, but the number of roll calls is significant. Use of the roll call variable is problematic, however, because it is nonstationary (using Dickey-Fuller and Phillips-Perron tests, one cannot reject the null hypothesis of a unit root, p=.58 and p=.61, respectively). Indeed, the mean number of roll calls increases dramatically after a 1970 rule change that made it easier to obtain recorded votes on amendments. When the roll call series is differenced (making it stationary), it is reduced in size and is maignificant.

	1867	'–1933	· 1933–98		
Variable	Model 1	Model 2	Model 3	Model 4	
ΔMEDIAN _{ADV} (change in median)	10.894**	10.778**	6.350*	6.752*	
	(3.469)	(3.506)	(3.517)	(3.604)	
Δ HOMOGENEITY (change in majority party std. dev. divided by floor std. dev.)	-4.535	-3.502	.064	370	
	(5.504)	(6.104)	(5.155)	(5.295)	
ΔPOLARIZATION (change in distance between party medians)		4.070 (10.677)		20.559* (11 . 419)	
INTERCEPT1	2.700	2.737	· 2.546	. 2.680	
	(.701)	(.713)	(.652)	(.692)	
INTERCEPT2	-1.705	-1.697	515	678	
	(.573)	(.576)	(.381)	(.413)	
χ^2 for covariates	18.186	18,333	4.916	8.601	
(degrees of freedom)	(2)	(3)	(2)	(3)	
ρ value	.0001	.0004	.0856	.0351	
N	33	33	33	33	

half, analyzing separately those Congresses before Franklin Roosevelt's election as president and those Congresses from 1933 through 1998.³⁷ This allows for the possibility that the parameters of interest have changed substantially in recent years. As models 1 and 3 of Table 4 show, when only change in homogeneity and change in the median are included, there is no support for the party government interpretation in either period, whereas the ideological power balance model gains support both before and after 1933. Models 2 and 4 add change in polarization to this baseline model, and the results are instructive. In the 1867–1933 period, changes in neither polarization nor homogeneity have a significant effect, but the median has a strong, significant influence. In 1933-98, however, change in both the median and polarization have a statistically significant effect on the adoption of partisan rules changes. The standardized estimate for the polarization variable is still smaller than that for the median (.396, compared to .483), but the difference is much narrower than for the full 132-year period.38

In summary, at least one element of conditional party government theory—the emphasis on changes in party polarization—has become more important in recent years. Consistent with Rohde's (1991) analysis, as the parties have increasingly diverged in their policy

³⁷ An anonymous reviewer suggested using 1933 as the dividing point.

preferences, there apparently has been greater determination by majority party members to tighten agenda control. The homogeneity hypothesis receives little support, regardless of the period examined. The ideological power balance model has substantial explanatory force, no matter which period or dependent variable is examined.

DISCUSSION

In the ongoing debate concerning the role of political parties and floor majorities in shaping legislative rules (Cox and McCubbins 1993, 1997; Krehbiel 1991, 1993; Rohde 1991, 1994; Schickler and Rich 1997a, 1997b; Sinclair 1994), few studies have attempted to measure the key variables of interest over a long period. Binder's (1996) work is an important exception (see also Fink and Humes 1997), and it is perhaps the most convincing demonstration of the usefulness of a conditional party government approach for understanding institutional change in the House over a long time span. But the results reported here suggest that the apparent effect of majority party capacity uncovered by Binder may largely be an artifact of its relationship to change in the floor median. Although the majority party's unity and seat share appear to have a measurable influence on the adoption of partisan rules changes when the floor median is omitted from the analysis, including change in the median generally negates the effects of these variables and contributes significantly to the predictive accuracy of the model. Party capacity is significant in model 2 of Table 3, but its effect appears somewhat fragile.

Party government models of rules changes are premised on the assertion that the majority party is able, under certain circumstances (such as when it is homogeneous), to bind swing voters to its side on controver-

One might argue that the period covered should be split into even finer categories to capture the ebb and flow of partisan influence, since partisan variables may fare better during eras when parties are stronger, such as 1890–1910, than in other years. But splitting the period into shorter time spans is difficult to justify theoretically: Each theory assessed in this article aims to generalize about the determinants of legislative organization across time. The theories would be of extremely limited use if they were applicable only to sharply delimited time spans. The findings reported here suggest that the ideological power balance model successfully explains variation in legislative rules across an extended period.

sial rules votes (Cox and McCubbins 1994, 221). The success of the ideological balance of power model in explaining House rules changes and the general failure of party-based variables to do so suggests that the median voter on the floor has been free to unite with members of either party to enact new rules. Indeed, it has been relatively common for cross-party ideological coalitions to form and pursue their substantive agendas through amendments to House rules. The 1910 coalition of Democrats and dissident Republicans that overthrew Speaker Cannon, thereby providing an important boost to the growing Progressive movement in the House and the nation at large (Holt 1967), was hardly an aberration. In much the same way, the repeated battles over the discharge rule in the 1920s pitted conservatives against progressives (Beth 1994; Cooper [1960] 1988), and the clashes over the role of the Rules Committee in the 1940s-60s pitted conservatives drawn from both parties against liberal Democrats and their northeastern Republican allies (Bolling 1968).

Indeed, the battles over the discharge rule and Rules Committee illustrate the role of changes in the floor. median. In both cases, liberals (or, in the 1920s, progressives) repeatedly engaged conservatives in conflicts that turned on the ideological balance of power on the floor. Following large Democratic gains in the 1922 election, Democrats and progressive Republicans held a slim floor majority in the new 68th Congress (1923-25). The median, which had been nearly identical to the GOP median in the preceding Congress, shifted by more than one-third of a point toward the Democrats (refer to Figure 2). Soon after the opening of the 68th, Democrats and progressive Republicans united to pass rules changes that loosened GOP control of the flow of business to the floor (Cooper [1960] 1988). In 1925, after large gains by conservative Republicans in the 1924 election, the floor majority shifted back in their favor. Now, the floor median was 38 closer to the Republican than to the Democratic median. The new conservative majority tightened GOP leaders' agenda control by repealing the liberalized discharge rule adopted in the 68th Congress. Much the same pattern emerged in fights over the 21-day rule. Large Democratic seat gains in 1948 and 1964 gave liberals a floor majority, which shifted the floor median closer to the Democratic than to the GOP median. This led to adoption of the promajority 21-day rule in 1949 and 1965. GOP gains in 1950 and 1966 gave conservatives a floor majority, which shifted the floor median back toward the Republican median. As noted above, the nominal Democratic floor majorities that remained were unable to block repeal of the 21-day rule in 1951 and 1967.39

The basic conclusion is that a parsimonious explanation for partisan rules changes can be found in the ideological balance of power on the floor. As paradoxical as it may seem, a relatively nonpartisan model, based on change in the floor median, generally does a better job than explicitly partisan models in explaining rules changes that have significant partisan overtones.

This paradoxical conclusion should not be taken too far. The finding that changes in party polarization have played a significant role in recent years suggests that conditional party government models can add to our understanding of rules changes, particularly in this era of highly polarized parties. Furthermore, although change in the floor median appears to offer the best understanding of House rules changes, it may not be as potent an explanation for many other features of legislative organization. 40 For example, it is plausible that other variables—including party homogeneity—do a better job of explaining changes in internal party organization and in committee assignment practices. Unlike House rules, these do not necessarily require a vote of the full House and therefore may be less susceptible to influence by the median voter. Rohde (1991) convincingly argues that as the Democrats became increasingly unified in the 1970s and 1980s, they better used their caucus to form policy positions and were more aggressive in employing committee assignments to reward party supporters and sanction defectors. A critical issue for future research is to determine the extent to which the floor median constrains the majority party's ability to use caucus rules changes to obtain favored outcomes on the House floor.

Still, the evidence presented here strongly suggests that at least in the realm of legislative rules the ideological balance of power on the floor is a potent force limiting the majority party's ability to adopt changes that serve its shifting interests. More than three decades ago, Jones (1968) drew attention to the limitations placed upon party leaders by their need to maintain a procedural majority on the floor. This analysis of partisan rules changes over 132 years reinforces and extends his insight. To a substantial degree, House rules reflect and respond to an underlying ideological power balance on the floor that can be captured by a relatively simple spatial model (Black 1958; Downs 1957). As the floor median moves toward the majority party, its leaders can shape rules that enhance the party's agenda control; when the median moves away from the majority party, cross-party coalitions are well positioned to shift rules and procedures in ways that respond to the new ideological power balance.

³⁹ See Froman 1967, 97-9, for a discussion of how the 1965 version

of the 21-day rule, in particular, strengthened the Democratic leadership while it was (briefly) in effect.

⁴⁰ But see King 1997 and Schickler and Rich 1997a for evidence that the majority party typically does not control another key aspect of legislative institutions, committee jurisdictions

APPENDIX A: CONGRESSES WITH RULES CHANGES THAT HAD SIGNIFICANT PARTISAN OVERTONES

TABLE A-1.	Coding of Rules Changes	j
Congress	Code Rule Change	
40 (1868)	+1 Limited dilatory motions pending a vote on motion to suspend the rules	
43 (1874, 1875)	+1 Required majority to second a motion to suspend rules; sharply limited dilatory motions	
44 (1875, 1876)	0 Repealed rules changes from 43d Congress (-1); adopted Holman rule on appropriations bil	II ridera (+1)
46 (1880)	0 Required debate on rules suspensions and previous question (-1); reinstated seconding of r suspensions motions (+1)	гиюв
47 (1882, 1883)	+1 Limited dilatory motions in contested election cases, allowed rules suspensions by majority valued bills	vote for
51 (1890)	+1 Reed rules: abolished disappearing quorum; restricted dilatory motions	1 * 1
52 (1891, 1892)	0 Repealed Reed rules (-1); increased Rules Committee prerogatives and restored Holman rule	le (+1)
53 (1894)	+1 Beedopted Reed's disappearing quorum rule ,	` ,
54 (1896)	+1 Readopted remainder of Reed rules .	
60 (1909)	-1 Created Calendar Wedneeday	
61 (1909, 1910)	 Strengthened Calendar Wedneeday; created discharge process, motion to recommit guarant minority; enlarged Rules Committee 	eed for
82 (1911)	+1 Tightened germaneness requirement on revenue bills; restored Holman rule	
64 (1916)	-1 Amended Calendar Wednesday process to make more workable	
38 (1924)	-1 Liberalized discharge rule; loosened germaneness rule on revenue bills	•
9 (1925)	+1 Repealed discharge rule from 68th Congress	
7 2 (1931)	-1 Liberalized discharge rule; loosened Speaker control over discharging conferees	
'3 (1933)	+1 Made reports from Rules Committee nondivisible	•
74 (1935)	+1 Discharge petition signatures raised to 218	
79 (1945)	-1 House Un-American Activities Committee granted standing committee status	
31 (19 49)	+1 21-day rule adopted, making it easier to bypass Rules Committee	
32 (1951) [;]	1 21-day rule repealed	
37 (1961)	+1 Rules Committee expanded from 12 to 15 members	
39 (1965)	+1 21-day rule adopted; limited obstruction factics	' r
0 (1967)	-1 21-day rule repealed	
91 (1970)	O Protections for minority (e.g., staffing funds) in Reorganization Act (-1); reading of Journal divided with, unless ordered by a majority (+1)	spensed
12 (1971)	+1 Minority party guarantee of committee investigatory staff funding eliminated	
3 (1973)	+1 Limits on minority obstruction; various devices adopted to expedite business	
94 (1975, 1976)	+1 Eliminated proxy voting ban in committee; eliminated 1974 guarantee of one-third of funds for investigatory staffing; Rules Committee can walve requirement for 2-hour availability of cor- reports; abolished House Un-American Activities Committee	or minority Inference
95 (1977) ,	+1 Increased number of days for rules suspensions; Ilmited obstruction tactics; abolished Joint (Committee
6 (1979)	+1 Increased threshold for demanding a recorded vote; limited obstruction tactics	
8 (1983)	+1 Limited riders to appropriations bills	
02 (1991)	+1 Senes of changes, including requirement that any legislation with direct spending or revenues binding Congressional Budget Office estimate for "pay as you go" sequestration	s include a
03 (1993)	O Package of changes allowing delegates to vote in Committee of the Whole and allowing com to declare a quorum once a majority of members had been present for some part of sessic eliminated three select committees, made public the names of members who sign discharge p	on (+1):
104 (1995)	+1 Eliminated three committees; required supermajority for tax increases; several additional char opening of session (see Evans and Oleszek 1997)	nges at

Note: Coding Issues. Binder's (1996) alternative coding excludes cases that I identify in the 54th, 64th, 73d; 79th, 81st, 82d, and 87th Congressee I exclude the prominonty change identified by Binder in the 65th Congress, which made party committee states nondrivisible, because it was done to protect both parties from attacks on the floor. Floor debete and secondary sources each suggest that members saw the change as neutral with respect to the majority and minority parties (see, e.g., Hasbrouck 1927). The only other coding difference is that I code the 93d Congress as having promagority changes, whereas Binder codes it as having both promajority and prominority changes (she does not assess the relative importance of the changes, but the main minority party victory, a minimum staffing guarantee, was not implemented). Binder does not code Congresses after 1990

Binder (1996) estimates separate models to predict the suppression of minority rights. She assigns each Congress two dummy variables. The first is coded 1 if a rule oreating a new minority right was adopted in that Congress, 0 otherwise. The second is coded 1 if a rule suppressing an exetting minority right was adopted in that Congress, 0 otherwise. I combine these two variables into a three-point scale. Each Congress is accred +1 if a minority right was suppressed but no new ones were created; -1 if a minority right was created and no existing rights were suppressed; and 0 if no relevant rules changes were adopted or if a package of changes was adopted that both created and suppressed minority rights. When the analyses in Appendix C are replicated using Binder's dummy variable for minority rights suppression, the results are substantively similar to the estimates when the three-point scale that includes both suppression and creation of minority rights is used.

It is also worth highlighting a handful of difficult coding decisions. One concerned the expansion of the House Rules Committee. This promajority change was adopted in 1961 but applied only to the 87th Congress (1961–63). Two years later, the enlargement was made permanent I counted the 1961 change but not the 1963 decision, which perpetuated the status quo created in 1961 (i.e., the 1963 rule did not alter the composition of the Rules Committee, the 1961 change altered its composition in the Democratel favor). I estimated all the models presented here using both 1961 and 1963 as promajority changes and using only 1963. The coefficient estimates did not offend these as "minor" and did not suggest that the new rules had significant parties overtones. Therefore, consistent with my coding of other Congresses, I scored this Congress as a "0," but I also resstimated the models recoding this case as a promajority change. None of the coefficient estimates changed appreciably.

APPENDIX B: MEASUREMENT OF INDEPENDENT VARIABLES

Independent Variables

The following measures were used to operationalize the independent variables.

- ΔHOMOGENEITY = HOMOGENEITY (t) HOMO-GENEITY (t - 1), where HOMOGENEITY is the standard deviation of first-dimension DW-NOMINATE scores for majority party members divided by the standard deviation of all House members' first-dimension DW-NOMINATE scores.
- 2. ΔHOMOGENEITY IN TWO DIMENSIONS is change in the average distance (using D-NOMINATE scores) in two dimensions across all pairs of majority party members (see Poole and Rosenthal 1997, 82, 269). This measure has only been calculated using D-NOMINATE scores, and it covers only the years 1879–1988. As a result, the models using this measure were restricted to that period.
- SWITCH_{DEM} is a dummy variable scored 1 if the previous Congress was Republican and the present Congress is Democratic, 0 otherwise.
- SWITCH_{REP} is a dummy variable scored 1 if the previous Congress was Democratic and the present Congress is Republican, 0 otherwise.
- 5. PARTY CAPACITY = CAPACITY_{MAJ} CAPACITY_{MIN}, where $CAPACITY_{MAJ} = (MAJPCT) \times (MAJCOHESION)$, $CAPACITY_{MIN} = (MINPCT) \times (MINCOHESION)$,

MAJPCT = % of seats held by majority party,

MINPCT = % of seats held by minority party,

MAJCOHESION = Rice cohesion score for majority
party members, and

MINCOHESION = Rice cohesion score for minority party members.

 PARTY CAPACTTY-DW: Same as PARTY CAPACTTY, except replace MAJCOHESION and MINCOHESION with:

MAJCOHESION_{NOMINATE} = $1/\sigma_{maj}$, where σ_{maj} = the standard deviation of majority party members' first-dimension DW-NOMINATE scores.

MINCOHESION_{NOMINATE} = $1/\sigma_{mm}$, where σ_{mm} = the standard deviation of minority party members' first-dimension DW-NOMINATE scores.

7. Δ POLARIZATION = POLARIZATION (t) - POLARIZATION (t-1), where

POLARIZATION = $abs(MEDIAN_{MAJ} - MEDIAN_{MIN})$, where

MEDIAN Majority party median, and MEDIAN Minority party median.

8. ΔΜΕDΙΑΝ_{ADV} = MEDIAN_{ADV}(t) - MEDIAN_{ADV}(t - 1), where

MEDIAN_{ADV} = abs(MEDIAN_{floor} - MEDIAN_{min}) - abs(MEDIAN_{floor} - MEDIAN_{may}), and

MEDIAN_{floor}: Floor median, dimension 1, DW-NOMINATE.

An example illustrates how the measure is calculated. In the 89th Congress (1965–66), the estimated floor median was -.075; the Democratic majority's median stood at -.250, and the Republican minority's median was .200. From the equation above,

MEDIAN_{ADV (89th)} = abs(
$$-.075 - .200$$
) - abs($-.075 - .200$) = .10.

Following GOP gains in the 1966 elections, the floor median in the 90th Congress shifted to the right, standing at .040; the majority Democrats' median was -.239, and the Republicans' median shifted to .190. Thus,

MEDIAN_{ADV (90th)} = abs(.040 - .190)
- abs(.040 -(- .239)) = -.129, and

$$\Delta$$
MEDIAN_{ADV (90th-89th)} = -.129 - .100 = -.229.

The 1950 elections shifted the floor median so that, whereas it had been .10 closer to the majority party median than the minority median in 1965–66, it now stood .129 closer to the minority party than the majority party median.

In addition to the measure for change in the median described above, I replicated the analysis substituting a simpler measure:

$$\Delta \text{MEDIAN}_{\text{floor}} = \text{MEDIAN}_{\text{floor}}(t) - \text{MEDIAN}_{\text{floor}}(t-1),$$
 when Republican majority at time t ,
$$= (-1) \times [\text{MEDIAN}_{\text{floor}}(t) - \text{MEDIAN}_{\text{floor}}(t-1)],$$
 when Democratic majority at time t .

This has the drawback that it tends to take on an extreme value whenever a change in party control occurs, even if the new median is no closer to the position favored by the new majority party than the old median had been to the position favored by the outgoing majority. Nonetheless, it produces similar results to the estimates when the more complicated measure is used. The one caveat is that it requires dummy variables taking into account switches in party control to the Democrats or the Republicans, whereas such controls make no difference when the more complicated measure is used.

Use of Nominate Scores

A final issue concerning measurement is the use of DW-NOMINATE scores. These are virtually identical to the more well-known D-NOMINATE scores (see McCarty, Poole, and Rosenthal 1997). The two sets of scores correlate at higher than .92. The primary advantage of DW-NOMINATE scores is that they have been calculated through the 105th Congress, while D-NOMINATE scores have only been calculated through the 100th Congress (1987–88). When D-NOMINATE scores are used (this requires restricting the analysis to 1867–1988), the coefficients for party polarization and party capacity are reduced in size and become statistically insignificant, although the floor median remains strong and significant in each specification.

It is essential that both D-NOMINATE and DW-NOMINATE scores are designed to be comparable over time. More specifically, the location of individual members in the issue space is allowed to change over time only through a linear trend parameter for that member. This restriction also means that the location of a specific member is not solely a function of his or her votes in the present Congress. As a result, the homogeneity measure for a given Congress is based on information from all the Congresses in which members of that Congress served. This makes it more plausible to argue that the dispersion of members' ideological positions measures majority party homogeneity and is not simply a function of the majority party's actual record of success on the roll calls taken in that Congress. The party's record of success, of

course, might be endogenous, as it could be shaped in part by the rules adopted at the beginning of each Congress. This makes it all the more important to use measures of homogeneity, such as DW-NOMINATEs, that are not simply a function of the majority party's success rate in the particular Congress under examination.

Rice cohesion scores, by contrast, are calculated for each party based on all roll calls conducted in a given Congress. The scores are the mean absolute difference of the percentage of party members voting "yes" and the percentage of party members voting "no" over all the roll calls. I am wary of using Rice cohesion scores to measure party homogeneity, since they do not purport to measure individual members' preferences and may closely reflect the party's actual success in maximizing its voting unity. DW-NOMINATE scores are also calculated based on the roll call record, and thus are somewhat vulnerable to a similar critique, but they are less vulnerable because of the constraint on change in individual members' scores across Congresses.

One might object that it is inappropriate for DW-NOMINATE scores to include a member's entire voting history from preceding Congresses in calculating that member's ideology score in the present Congress. This could be a problem if a member's voting behavior changes markedly in the same Congress that major rules reforms are adopted. While Poole and Rosenthal (1997) show that it is quite rare for members' voting behavior to change appreciably over time, I nonetheless sought to avoid this potential difficulty by considering a measure for majority party homogeneity derived from Cox and McCubbins (1993, 149): They examine party loyalty scores on "party agenda votes" for each post-World War II Congress. Substituting this measure had little effect on the results. Indeed, during that period, the measure of homogeneity based on party agenda votes is less closely related to partisan rules changes than is the homogeneity measure based on NOMINATE scores.

Varlable	Theory (expected slgn)	Model 1a	Model 2a	Model 3a	Model 4a	Model 5a	Model 6a
∆MEDIAN _{ADY} (change in median)	Median voter (+)	7.380** (2.111)	7.947** (2.253)	6.735** (1.967)	6.946** (2.028)		13.224** (4.935)
Δ HOMOGENEITY (change in majority party std. dev. divided by floor std. dev.)	Party cartel (-)	750 (3.739)	-2.321 (4.898)			-7.827* (3.842)	6.249 (6.364)
SWITCH _{DEM} (change to Democratic majority)	Party cartel (+)		.004 (1.077)		-1.240 (1.150)	•	,
SWITCH _{REP} (change to Republican majority)	Party cartel (+)		969 (1.146)		.581 (1.145)		
ΔΗΟΜΟΘΕΝΕΊΤΥ IN TWO DIMENSIONS (change in avg. distance between majority party reps.)	Party cartel (-)			3.100 (5.763)	8.807 (8.010)		
Δ MAJPCT (change in majority party seat share)	,					10.094** - (3.732)	-12.033 (8.836)
INTERCEPT1		2.804 (.522)	2.945 (.571)	2.575 (.522)	2.685 (.564)	2.606 (.492)	2.827 (.527)
INTERCEPT2		-1.532 (.362)	-1.442 (.382)	-1.601 (.393)	-1.581 (.429)	-1.386 (.333)	-1.559 (.365)
χ ² for covariates (degrees of freedom) o value N		16.253 (2) 7.0003 62	17.011 (4) .0019 62	14.131 (2) .0009 54	15.307 (4) .0041 54	9.561 (2) .0084 62	18.225 (3) .0004 62

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The Political Economy of Expenditures by the Peruvian Social Fund (FONCODES), 1991–95

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The Peruvian Social Fund (FONCODES) was created by President Alberto Fujimori in 1991 with the stated objectives of generating employment, alleviating poverty, and improving access to social services. This article uses province-level data on monthly expenditures, socioeconomic indicators, and electoral outcomes to analyze political influences on the timing and geographic distribution of FONCODES expenditures between 1991 and 1995. I reach three main conclusions. First, these expenditures increased significantly before national elections. Second, FONCODES projects were directed at provinces in which the marginal political effect of expenditures was likely to be largest. Third, these projects favored the poorest provinces, which suggests that the program also had a redistributive function. The results are robust to a large number of specifications and controls. The Peruvian data support predictions made in the literature on political business cycles as well as the literature on political influences on the allocation of discretionary funds.

o incumbents use social programs to get themselves reelected? There is a longstanding debate about the extent to which politicians can successfully manipulate the economy to coincide with elections. The traditional political business cycle literature predicts increases in expenditures before an election (Nordhaus 1975; Tufte 1978). A fundamental assumption underlying these early models—that voters are myopic and cannot see what motivates politicians—was brought into question with the advent of rational expectations theory. Since then, however, more sophisticated models have been developed, some of which lead to political budget cycles even in the face of fully rational voters.¹

The empirical evidence from less developed countries (LDCs) tends to support the predictions made in the political business cycle literature. In a study of 17 Latin American countries over 35 years, Ames (1987) shows significant increases in total public sector expenditures in electoral years. More recently, Schuknecht (1996) finds further evidence for political business cycles in a study of 35 countries between 1970 and 1992. One partial exception to this body of evidence is Remmer (1993), who uses recent data from six Latin American countries to argue that economic conditions generally deteriorate in the period leading up to an election and improve thereafter. This suggests that politicians may try to manipulate macroeconomic vari-

ables but are often unsuccessful, an argument also made for Western Europe (Lewis-Beck 1988). A related issue, which has not been resolved in the empirical literature, is whether voters in LDCs actually reward politicians for what Rogoff (1990, 21) calls their "preelection budget antics" (Dornbusch and Edwards 1991; Stokes 1996a).

There is also considerable controversy about the strategy a rational incumbent should follow with regard to the geographical distribution of expenditures. Much of the debate revolves around the relationship between voting patterns, on the one hand, and ideological preferences, income levels, and net transfers, on the other.

Theories of the determinants of voting behavior often consider the affinity of voters for different candidates (an "ideological" component) and the changes in welfare that voters can expect to see after an election (an "economic" component) (Dixit and Londregan 1996; Landon and Ryan 1997; Lindbeck and Weibull 1987; Markus 1988; Peltzman 1990). Incumbents can (obviously) increase their standing in voters' eyes with policies that improve overall welfare—economic growth, broadly speaking. Yet, they also can choose to favor some voters over others by being selective with taxes and transfers. How should a rational incumbent distribute taxes and transfers across the electorate? The answer clearly depends on whether a given level of net transfers sways some voters in favor of the incumbent more easily than others.

Define R_i to be the change in the probability that voter i will vote for the incumbent as a result of a unit change in net transfers. R_i is therefore a measure of the "political productivity" of net transfers to different voters:

$$R_i = g(y_i, a_i), \tag{1}$$

where y_i is income, and a_i is a time-invariant measure of the "affinity" between voter i and the incumbent. This affinity could include voter i's perception of the incumbent's character, or the ideological proximity between voter i and the incumbent. I assume that the

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¹ A useful summary of competing perspectives can be found in Alesina and Roubins, with Cohen, 1997.

parameter a_i ranges from $-\infty$ to ∞ , which represent strong dislike and like for the incumbent, respectively.

There are numerous competing theories about the relationship between a_i and R_i . Cox and McCubbins (1986) argue that R_i is largest when transfers are made to voters with a value of a_i that is large and positive. There are two key insights here. First, incumbents may be risk averse. Transfers by incumbents to core supporters, whose needs and preferences they understand well, are likely to be a safer investment than transfers to other (unknown) voters. Second, administrative costs and inefficiencies limit the fraction of transfers that actually materializes as a benefit to voters. This fraction may be higher when incumbents make transfers to core supporters.

Lindbeck and Weibull (1987) argue that R, is largest when transfers are made to swing or marginal voters with a value of a_i close to zero. The intuition behind this result is that transfers to voters who strongly favor (oppose) the incumbent are unlikely to make a difference, as these voters will probably vote for (against) the incumbent regardless of the transfer. Lindbeck and Weibull extend their model to consider cases in which politicians seek to maximize the probability of being reelected rather than the expected number of votes. Under these circumstances, if candidates are not equally popular, Lindbeck and Weibull show that supporters of the more popular candidate are more likely to be pivotal voters who can decide the outcome of the election. For any two values of a_i , that are identical in magnitude but opposite in sign (αa_{ij}) αa_i), incumbents therefore should favor supporters of the candidate who is more popular.

Dixit and Londregan (1996) suggest that the choice between a strategy that favors swing voters and one that favors core supporters depends on whether incumbents can collect taxes and distribute benefits more effectively among their supporters. In general, rational incumbents should favor swing voters when there are no such differences, but core supporters when there are.²

The discussion above implies that incumbents can observe the individual a_i directly. This is obviously unrealistic, but incumbents may be able to approximate voter preferences by looking at geographic patterns in previous elections. For example, they can assume that all voters in a province are identical or, more plausibly,

that the distribution of preferences differs systematically across provinces. It can be shown that the probability a randomly selected voter in province d voted for a party j is equivalent to the share of the vote for party j in that province (Deacon and Shapiro 1975). Voters in provinces in which 50% of the population voted for the incumbent in the last election are most likely to be marginal: The probability that they supported the incumbent is exactly the same as the probability that they supported other candidates. The same logic applies to core supporters, who are most likely to be found in provinces that voted heavily for the incumbent.

One also may consider dynamic models in which politicians reward fickle voters. Take the example of an incumbent who has already survived (at least) one reelection. One strategy for this two-time incumbent would be to buy back traditional supporters who have recently favored opposition candidates—either because it is cheaper or less risky to bring them back into the fold or because there are long-term benefits to having a stable base of support.³ Alternatively, a twotime incumbent can attempt to consolidate his position among voters who backed him for the first time. Comparing the results from two or more past elections enables an incumbent to identify provinces in which voters are most likely to have switched their support. Where politicians are clearly associated with their parties, incumbents also may be able to extract information by comparing the vote for their party in the two most recent elections.

What about the relationship between the political productivity of transfers and income? If there is diminishing marginal utility of income, voters with low initial levels of income should benefit more from a given transfer than their wealthier counterparts. Therefore, the political productivity of transfers to the poor should be higher (Dixit and Londregan 1996; Lindbeck and Weibull 1987). If the poor are less likely to vote than the nonpoor, however, the negative correlation between R_i and y_i may be dampened or become positive (Cox and McCubbins 1986; also Fleck 1994). Note, finally, that a negative correlation between y_i and transfers also can be explained by an altruistic parameter in the incumbent's utility function, a possibility that is discounted in all these models.

In this article, I use data on voting patterns and social expenditures from Peru in the early 1990s to analyze the influence of elections on the timing and distribution of expenditures made by the Peruvian Social Fund (Fondo Nacional de Compensación y Desarrollo Social—FONCODES) during President Alberto Fujimori's first term (1990-95). FONCODES

² The Dixit and Londregan (1996) model focuses on groups of voters (e.g., garment workers or senior crizens). In this context, Dixit and Londregan propose two additional parameters that should affect the decision to favor some groups over others. First, for any given value of a_{ij} some groups are more willing than others to compromise on their ideological preferences in exchange for material rewards. Groups less attached to their ideology should expect to receive higher transfers. Second, for any two groups with a mean value of a, equal to zero, one will likely have a larger variance (m statistical terms) m a, than the other. The lower variance group will have a larger concentration of voters with a value of a, close to zero. A given level of transfers will induce more members of this group to switch their vote, and they will therefore be favored by rational incumbents. In practice, these two additional parameters cannot be identified with data on electoral outcomes and expenditures, and I do not consider them further.

³ I thank an anonymous referee for this suggestion. Note that this is a different use of the term "buy back" from that found in numerous other works (e.g., Bruhn 1996; Gershberg 1994), where buy back refers simply to the negative coefficient on a regression of expenditures on the level of the progovernment vote in the most recent election, rather than the negative coefficient on the change in the progovernment vote. Insofar as the vote on this most recent election is not compared to the vote in a prior election, however, there is no way of telling whether these voters are being bought back or wooed for the first time.

was created in 1991, during the severe recession that followed the adoption of stringent stabilization and structural adjustment measures by the Fujimori government. Its stated objectives were to generate employment, alleviate poverty, and improve access to social services (World Bank 1998). By 1993, the Peruvian economy had begun a remarkable recovery, and FON-CODES was being hailed by government officials and foreign donors alike as an important component of a medium-term strategy to reduce poverty.

Scholars in Peru and abroad have generally been more guarded in their praise. Critics argue that social funds like FONCODES undermine line ministries and local governments, and questions about the "politicization" of FONCODES have bedeviled the program from its creation (Ballón and Beaumont 1996; Graham 1994; Kay 1996; Weyland 1998). Yet, there are no satisfactory studies of the influence of elections on FONCODES expenditures.⁴

This article attempts to fill that gap using highly disaggregated data on expenditures, socioeconomic indicators, and electoral outcomes. I use econometric techniques to analyze the timing and geographic distribution of FONCODES expenditures, and I reach three main conclusions. First, these expenditures increased significantly before national elections. Second, FONCODES projects were directed to provinces in which their marginal political effect was likely to be largest. Between 1991 and 1993, FONCODES expenditures favored core supporters and marginal voters. After a disappointing result in a national referendum held in 1993, however, the Fujimori administration redirected resources in an attempt to buy back the vote in turncoat provinces. Third, FONCODES projects favored the poorest provinces, which suggests that the program also had a redistributive function. My results, which are robust to a large number of specifications and controls, show that the kind of targeted povertyalleviation programs which have become increasingly widespread in Latin America and elsewhere may be particularly vulnerable to political interference.

The rest of the article proceeds as follows. After providing a brief description of Peruvian politics between 1990 and 1995 and of FONCODES, I describe data requirements and sources for this study. I then estimate the effect of the electoral calendar on the timing of FONCODES expenditures as well as the effect of the 1990 presidential election and the 1993 referendum on the distribution of expenditures. I conclude with some general thoughts on the use of social expenditures for electoral purposes.

THE FUJIMORI YEARS AND FONCODES

The election of the dark horse Alberto Fujimori to the presidency in 1990 marked the beginning of a fundamental transformation of Peruvian politics. Fujimori, an independent with no significant party organization,

defeated the center-right coalition headed by the well-known novelist Mario Vargas Llosa. Fujimori's party did not win a majority in either house of the Peruvian congress. Blaming "corrupt" institutions for his inability to introduce a coherent economic program or combat the terrorist insurgencies of Shining Path and the Túpac Amaru, Fujimori used the military to shut down congress and the judiciary in April 1992. Under international pressure to restore democracy, he drafted a new constitution, which was approved by a small margin in a referendum held in October 1993, and won a landslide reelection to the presidency and a majority for his party in the new unicameral congress in April 1995.

The academic literature on Peru argues that traditional sources of authority and intermediation between citizens and the state have been weakened under Fujimori (Cameron 1994; Conaghan 1996; Mauceri 1995; Seddon 1997). The "historical" political parties have all but disappeared.5 The legislative and judicial branches have not acted as an effective check on the executive. There also have been important changes in the relationship between various levels of government and in the conduct of social policy. Decree 776, passed in December 1993, abolished most sources of tax revenue traditionally used by municipal governments. Responsibility for the administration of numerous social and basic infrastructure programs has been transferred from local governments and line ministries in education, health, and transportation to the Ministry of the Presidency. By 1995, this ministry controlled more than 20% of the central government budget and oversaw thirteen programs involved in, among other things, nutrition, education, health, water, sanitation, and housing (World Bank 1996, 2).

FONCODES is one of the new programs created by Fujimori. Its charter establishes it as an "autonomous body" outside the traditional line ministries. By law, the FONCODES executive director reports directly to a five-member board of directors appointed by the president. This institutional arrangement raised concerns that FONCODES could easily become a means for dispensing pork, and these misgivings were reinforced when Luz Salgado, a former secretary general of

⁴ Moncada (1996) and Graham and Kane (1998) discuss the timing and distribution of FONCODES expenditures, respectively. As I discuss below, however, both works have serious limitations.

The four major parties that dominated civilian politics in Peru for decades have become irrelevant These four—the conservative Popular Christian Party, the center-right Popular Action, the center-left Alianza Popular Revolucionaria Americana (APRA), and the left-wing United Left—jointly garnered 82.2% and 97% of the vote in the presidential elections of 1980 and 1985, respectively. In the 1995 presidential election they won only 6.3% of the vote (Jurado Nacional de Elecciones 1995; Tuesta Soldevilla 1994). Cambio 90-Nueva Mayoría, Fujimori's party, has no outstanding leaders other than the president himself. Independent candidates with no party affiliation have dominated local politics. In the 1993 municipal elections, for example, independents received almost two-thirds (64.7%) of the votes cast (Tuesta Soldevilla 1995, 106).

The législature, where Fujimori's party has had an absolute majority since 1992, rubber-stamps the president's initiatives; on the only important occasion on which the Corte Suprema contravened Fujimori's wishes, ruling that a draft law which would allow Fujimori to run for a second reelection in 2000 was unconstitutional, the three members who voted with the majority opinion were dismissed from the beach.

Fujimori's Cambio 90 Party, was appointed the first executive director (Graham 1994, 108–9; Kay 1996, 79). Subsequent directors were recruited from the private sector (Arturo Woodman, Alejandro Afuso) and from other branches of government (Manuel Estela, Manuel Vara Ochoa), which helped dispel some of the initial concerns. Over time, FONCODES has improved coordination in the planning, execution, operation, and maintenance of projects with the line ministries and, to a lesser extent, with local governments and nongovernmental organizations (NGOs).

Between December 1991 and May 1995, FON-CODES funded almost 16,000 community-based projects, for a total budget of about 465 million soles.⁷ At the 1992 exchange rate, this was equivalent to roughly \$285 million (U.S. dollars). The average project size was quite small—about \$18,000 (U.S. dollars). These community-based endeavors include nutrition and family planning projects, rotating credit schemes, and projects for the construction or rehabilitation of schools, health posts, water and sanitation systems, rural roads, electrification schemes, and smallscale irrigation works. In addition, FONCODES has executed a series of centrally designed "special" projects, including a school breakfast program, the distribution of uniforms for schoolchildren, shovels for farmers, and motorized canoes for isolated jungle communities. Between December 1991 and May 1995, FONCODES spent about 135 million soles (roughly \$83 million in U.S. dollars) on these special projects. In 1993, FONCODES expenditures accounted for roughly 0.55% of GNP—more or less in the same order of magnitude as the Solidarity program in Mexico, which accounted for about 0.71% of GNP in 1992.

FONCODES has much in common with other social funds in the region (Glaessner et al. 1994). The bulk of its projects are demand driven and targeted. FONCODES' community-based projects (but not the special projects) are demand-driven in that communities choose a project from a menu and prepare a proposal for funding. FONCODES then functions as a financial intermediary: Rather than execute projects itself, it approves proposals and releases funds to the *núcleo ejecutor*, a group of community members elected specifically for this purpose. FONCODES also targets its investments, first by using a "poverty map" to assign resources to small geographic areas and then by conducting an informal on-site assessment of the poverty of the community requesting a project.8

THE DATA SET

In 1997, Peru was divided into 13 administrative regions, 24 departments, 194 provinces, and 1,812 districts (Webb and Fernández Baca 1997, 112). Regions and departments are administrative arms of the central

government, which appoints a governor for every region. Provincial and district-level mayors are chosen at municipal elections every three years. Data on FON-CODES expenditures, socioeconomic characteristics, and voting patterns are available at different levels of geographic aggregation.

FONCODES keeps monthly records on the number of projects and aggregate amounts spent in each district. Three points are worth noting about these data. First, because expenditures are recorded in the month in which a project is approved, there is a lag of about two weeks before disbursement of the first funds and of several months before the second (and final) disbursement. Second, only expenditures on projects are included, not administrative costs or general overhead. Third, district-level information on the distribution of funds is only available for the community-based projects. There is no information on allocation formulas or actual fund distribution for the special projects.

The Peruvian National Statistical Institute (Instituto Nacional de Estadística e Informática—INEI) has estimated population size, the proportion rural, and a host of socioeconomic indicators at the district level. Most of these indicators are based on a population and housing census conducted in 1993. The one important exception is the rate of chronic malnutrition, which is based on a census of height and weight among school-children also conducted in 1993.

The Jurado Nacional de Elecciones, the body that supervises elections in Peru, keeps records on elections held since 1990. Some of these data have been compiled and published (Jurado Nacional de Elecciones 1995; Tuesta Soldevilla 1994). Results are generally available for all provinces in the country and for all districts in metropolitan Lima.

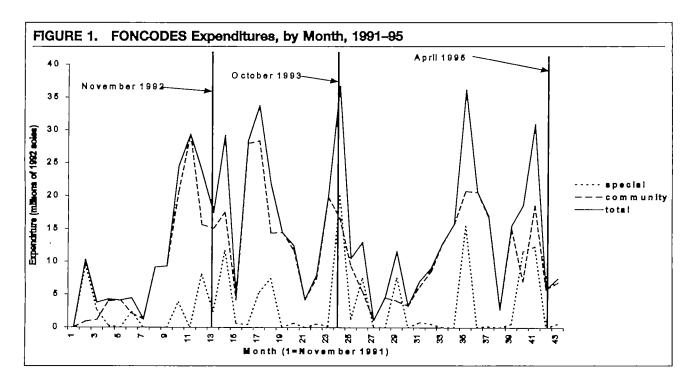
The level of aggregation used for the analysis in this article was dictated by theoretical considerations and the availability of data. The next section uses national aggregates of FONCODES expenditures to analyze the relationship between the timing of expenditures and the electoral calendar. Sources of funding for and the administration of the community-based and special projects are different, so the analysis is broken down by project type. FONCODES has assigned resources to individual provinces since 1993 and to individual districts since 1996. The following section uses a provincelevel data set to analyze the influence of electoral outcomes on the geographic distribution of FON-CODES expenditures for community-based projects. To avoid having one data point account for 5.78 million inhabitants—more than one-quarter of the population of the entire country—the province of metropolitan Lima has been further divided into its districts. The term "province" is used throughout to refer to these metropolitan districts and to national provinces.

THE TIMING OF FONCODES EXPENDITURES

Is there evidence of a political cycle in FONCODES expenditures? Figure 1 graphs monthly data for the amounts approved for community-based and special

⁷ All expenditures are in constant 1992 soles, unless otherwise indicated.

Note, however, that even without political interference actual expenditures in a given province rarely would be equal to allocations because of the demand-driven nature of the program.



projects between November 1991 and May 1995.9 The chart shows that expenditures are highly irregular, especially (but not only) for the special projects. There are at least five distinct spikes in total expenditures. Three of these coincide with periods immediately before three national elections: the November 22, 1992, election to the constituent assembly (month = 13); the October 31, 1993, referendum (month = 24); and the April 9, 1995, presidential election (month = 42). Figure 1 provides some prima facie evidence that the timing of FONCODES expenditures was affected by elections.

To formally test the possible effect of the electoral calendar on expenditures, I estimate a series of regressions of the following form:

$$\operatorname{Exp}_{t} = \alpha + \beta \operatorname{Elec}_{t} + \chi z_{t} + u_{t}, \tag{2}$$

where Exp_t stands for FONCODES expenditures in time t; Elec, is a dummy variable for the presence (or absence) of an election in the three previous months; z_t is a vector of exogenous variables that affect expenditures, including a linear time trend and quarterly dummies; and u_t is the error term. If expenditures are timed to coincide with elections, we would expect the parameter β to be positive. Note that this formulation assumes that the timing of elections is exogenous. Insofar as it is easier to manipulate the timing of FONCODES expenditures than the timing of elections, this is a reasonable assumption.

Table 1 reports results of OLS regressions of expen-

ditures both in total and broken down for community-based and special projects. Columns 1, 3, and 5 report results from the regressions of expenditures on a single electoral dummy variable; columns 2, 4, and 6 include three separate electoral dummies to allow the effect of elections on expenditures to vary by election. All estimates are corrected for serial correlation in the error term using the Cochrane-Orcutt (1949) procedure.

The results in Table 1 confirm the general picture in Figure 1. The electoral parameter in columns 1, 3, and 5 is positive and significant at the 5% level or better in all three specifications. This suggests that expenditures on both community-based and special projects were significantly higher than expected in the three months leading up to elections. When the three elections enter into the regression separately, their influence on the timing of expenditures is most apparent for the special projects, especially for the October 1993 referendum and the April 1995 presidential election.

The changing political context in Peru may explain differences in the effect of elections on FONCODES expenditures. The Fujimori government had much at stake in the October 1993 referendum and the April 1995 presidential election. The referendum on the new constitution was widely seen as a referendum on three years of Fujimori government (Roberts and Arce 1998, 229). Moreover, the new constitution included a clause that would allow Fujimori to run for reelection, which he could not do under the 1980 constitution. The April

⁹ Moncada (1996) presents a simple timeline of quarterly FON-CODES expenditures and elections between 1991 and 1995. Interpretation of Moncada's graph is not easy, however, because elections do not always fall at the end of a quarter. Also, Moncada does not formally test the relationship between elections and expenditures, perhaps because of the small number of quarters (sixteen) and the large number of national elections (three) during the period.

¹⁰ Because of the two-week lag between approval of a project and disbursement of funds, the relevant month for the parameter Elec, in equation 2 is taken to be that in which the project was approved, if the election took place in the second half of the month, or the month preceding it, if the election took place in the first half of the month. Note that this is equivalent to shifting all approved expenditures forward by two weeks.

	Community-Based Projects		Specia	Special Projects		Total Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	
Election	7.05* (3.48)	_	5.07* (2.12)	_	12.72** (4.58)	_	
Election 1	_	6.08 (5.32)	→ `	0.82 (2.28)		3.73 (6.52)	
Election 2	_	4.51 (6.54)	_	14.74*** (4.17)	_	21.84* (8.47	
Election 3		10.49 (6.52)	_	11.72 ** (4.30)		17.93* (8.48)	
Monthly linear trend	0.04 (0.15)	0.03 (0.16)	0.04 (0.05)	-0.00 (0.04)	0.09 (0.15)	0.01 (0.17)	
Seasonal dummles	Yes	Yes	Yes	Yes	Yes	. Yes	
Durbin-Watson statistic (original)	1.20	1.22	2.45	2.43	1.49	1.42	
Durbin-Watson statistic (transformed)	2.13	2.10	2.00	2.05	2.03	2.06	
R ²	0.167	0.173	0.278	0.370	0.274	0.328	
Number of observations	42	42	42	42	42	42	

Note Standard errors corrected for sensi correlation using the Coohrane-Oroutt procedure are reported in parentheses. There are four sessional duminuses one for each quarter of the calendar year. A constant was calculated but is not reported p < 0.05, p < 0.05, p < 0.05, p < 0.05.

1995 election would assure Fujimori five more years as president. By contrast, the November 1992 election to the constituent assembly took place at a time when the government was virtually unchallenged: The opposition was in disarray after Fujimori's "self-coup" of April 1992, and the president's popularity was very high. Indeed, three of the four "historical" parties— Popular Action, APRA, and the United Left—did not even participate in the 1992 election, alleging that this would "legitimize" Fujimori's coup. The Popular Christian Party participated but won only 9.8% of the vote. Fujimori's Cambio 90-Nueva Mayoría party won 49.2% of the vote and 44 of the 80 seats in the constituent assembly. The remaining 41% of the vote went to smaller parties, many of which were formed shortly before the election.

The empirical analysis above suggests that Fujimori boosted FONCODES expenditures when it mattered most, but other factors are obviously at work. Consider, for example, the expenditure spike in September 1994 (month = 35), the second largest in the data set. In August 1994, Manuel Vara Ochoa replaced Manuel Estela as the executive director of FONCODES. Estela was a technocrat who was highly regarded at the World Bank and the Inter-American Development Bank. Before assuming the post at FONCODES, he had been the head of SUNAT, the government agency charged with the collection of taxes in Peru. Estela brought to FONCODES his advisors from SUNAT and a working method that emphasized a rigorous screening of project proposals. This was not to Fujimori's liking, and the president quickly became dissatisfied with Estela's unwillingness to meet "disbursement targets." By contrast, Vara Ochoa was a friend of Fujimori's from their days as professors at the Agrarian University in Lima, and he was understood to be more sensitive to the political importance of a speedy approval process. The massive spike in expenditures in September 1994 may be a result of the wholesale approval of projects that had not been funded under Estela.

THE GEOGRAPHIC DISTRIBUTION OF FONCODES EXPENDITURES

To determine the influence of elections on the geographic distribution of FONCODES expenditures for community-based projects, I test a model in which *past* electoral outcomes are used to predict *future* expenditures.¹² I start by describing some features peculiar to

¹¹ Anthor's conversations with staff at FONCODES and at the Ministry of the Presidency. One knowledgeable staff member described the relationship between Estela and Pujimori as "strained" and related a telephone conversation in which Fujimori gave Estela a serious warning ("un toque de atención de palacio").

¹² Arguably, voters base their decisions on the promises made by an incumbent and his challenger(s) before an election, and this is the starting point for much theoretical work on the subject (Cox and McCubbins 1986; Dixit and Londregan 1996; Lindbeck and Weibull 1987). Promises can be just that, however, and voters are likely to take the "revealed preferences" of incumbents, evidenced by their behavior in office, as a more reliable indication of the policies they intend to follow in the future. The issue of the (lack of) credibility of promises is particularly relevant in Peru. Pujimori was elected in 1990 on a platform that opposed economic shock therapy, yet he implemented one of the most radical orthodox programs to stabilize and restructure the economy within weeks of taking office (Roberts

TABLE 2. Socioeconomic Characteristics, Voting Patterns, and FONCODES Per-Capita Expenditures on Community-Based Projects, by Province

	Mean	St. Dev.	Median	25th %	75th %	Mln.	Max.
Population	97,165	118,630	58,168	28,814	116,486	185	692,265
FONCODES poverty index*	15.31	6.03	16.14	11.49	19.57	1.00	28.24
Proportion rural	44.92	30.49	49.88	14.40	72.29	0	91.00
Pro-Fujlmori vote: 1990 ^b	65.37	13.70	65.07	56.32	75.01	17.76	93.35
Pro-Fujlmori vote: 1993 ^b	50.60	12.05	51.52	45.78	58.77	12.17	80.05
Per-capita expenditures ^c	31.18	28.97	24.25	13.59	41.71	0	191.83

Source: Information on the FONCODES poverty index and per-capita expenditures was made available by FONCODES. Information on population and the proportion rural is based on the 1993 Census and is available from INEL information on the pro-Fujimon vote in 1990 and 1993 is found in Tuesta Soldevilla 1994.

the Peruvian setting, discuss the econometric specification, and present results.

Two-Tier Elections, Taxes, and Transfers in Peru

At least two features of elections and expenditures need to be discussed in the context of a model of political influences on the geographic distribution of social expenditures in Peru: the two-tiered structure of presidential elections and the geographic incidence of the taxes, if any, that must be raised to finance expenditures.

Presidential elections in Peru are often decided in two stages. If no candidate receives a simple majority in the first round of the election, a run-off is held between the two candidates with the largest number of votes. In the 1990 election, Vargas Llosa led by a small margin after the first round of voting on April 8 (32.6% to Fujimori's 29.1%), but he lost by a large margin in the second round on June 10 (37.6% to Fujimori's 62.4%). Fujimori was a virtual unknown in many areas of the country until shortly before the first round (Carrión 1996; Schmidt 1996; Stokes 1996b), but he had become a household name two months later. Because voters in Peru were arguably constrained by lack of information in the first round, I use the results of the run-off for all estimations below.

Transfer programs like FONCODES are generally paid for out of tax revenues. Since taxes have a geographic incidence pattern that can affect electoral outcomes, one must consider the distribution of "net transfers," or the per-capita difference between transfers and taxes needed to finance a program.

FONCODES was initially financed out of general

and Arce 1998; Stokes 1996b). For this reason, I take Fujimori's record in office to be a more credible indication of his intended policies than any campaign promises.

government revenues. In early 1994, however, the Peruvian government appealed to the World Bank and the Inter-American Development Bank for funding, and two loans for \$100 million and \$94 million (U.S. dollars), respectively, were made available for FON-CODES.¹³ These loans included provisions for a fiveyear grace period and full amortization over seventeen years—presumably, well after Fujimori would have left office. Theory suggests that fully rational voters should take into account the net present value of the expected taxes that must be raised in the future to repay loans. In practice, however, short time horizons, lack of information, and the real or imagined possibility of debt renegotiation or default could limit the extent to which voters in Peru considered these future costs. This article only considers FONCODES expenditures.

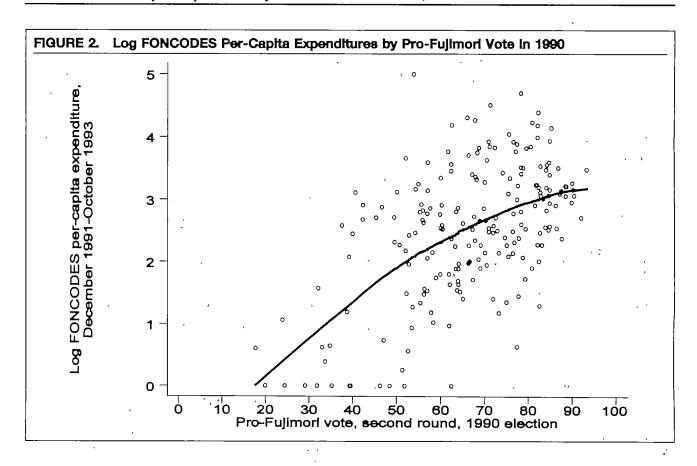
Table 2 provides descriptive statistics of socioeconomic characteristics of provinces, voting patterns, and per-capita expenditures made by FONCODES. It shows that provinces in Peru vary a great deal on all these counts. The variation in per-capita FONCODES expenditures is particularly noteworthy: Some prov-

[&]quot;This is the poverty index used by FONCODES in 1996. It is a weighted mean of eight variables: the rate of chronic matinization among schoolchildren, the rate of filteracy, the proportion of children not in school, and the proportion of households who live in overcrowded housing, have inadequate roofing, no access to running water, no sewerage, and no electricity. Higher values of the index are an indication of higher poverty. All values are normalized so that the province with the lowest amount of poverty has a value of the FONCODES poverty index equal to 1.

^bSummary statistics for the pro-Fujimon vote in 1990 and 1993 are population weighted. They do not correspond exactly to the actual fractions voting for Fujimori because of differences between total population and the population of eligible voters. Moreover, electoral data for the second round of the 1990 presidential election are only available for 205 of the 224 provinces that exacted at the time.

This refers to per-capita expenditures made by FONCODES on community-based projects between December 1991 and March 1996, in 1992 soles.

¹³ Determining the exact moment at which foreign funding became available is not straightforward. The World Bank loan, for example, was approved in December 1993 and became effective in March 1994; disbursement agamst it began in May 1994. But the loan included a proviso that up to \$10 million (U.S. dollars) could be used to fund eligible community-based projects approved since August 1, 1993. There is also controversy about the extent to which multilateral organizations such as the World Bank, the Inter-American Development Bank, and the International Monetary Fund (IMF) attempted to convince Fujimori of the importance of programs targeted at the poor or went along with his own initiatives only reluctantly. Although the IMF and the banks had some initial concerns about FONCODES (Graham 1994; Kay 1996, Weyland 1998), the fact that it was very similar to other programs funded by the World Bank and the Inter-American Development Bank throughout Latin America meant that loans for FONCODES could be justified because of their likely effect on the poor.



inces received no transfers whatsoever, while others received more than six times the mean.

Figures 2 and 3 show scatter diagrams and nonparametric (kernel) regressions of the logarithm of percapita FONCODES expenditures as a function of the proportions voting for Fujimori in the 1990 presidential election and the 1993 referendum, respectively. Figure 2 reveals a positive correlation between the 1990 Fujimori vote and expenditures between December 1991 and October 1993; Figure 3 shows a negative correlation between the 1993 Fujimori vote and expenditures in the November 1993—March 1995 period. In both instances, there appears to be some curvature in the relationship between the vote and expenditures.

Figures 2 and 3 provide some prima facie evidence that the distribution of FONCODES expenditures between 1991 and 1995 may have been responsive to electoral influences and that the 1993 referendum was a turning point for FONCODES. This has been argued elsewhere (Graham and Kane 1998; Kay 1996; Roberts and Arce 1998), but much of the earlier work has some

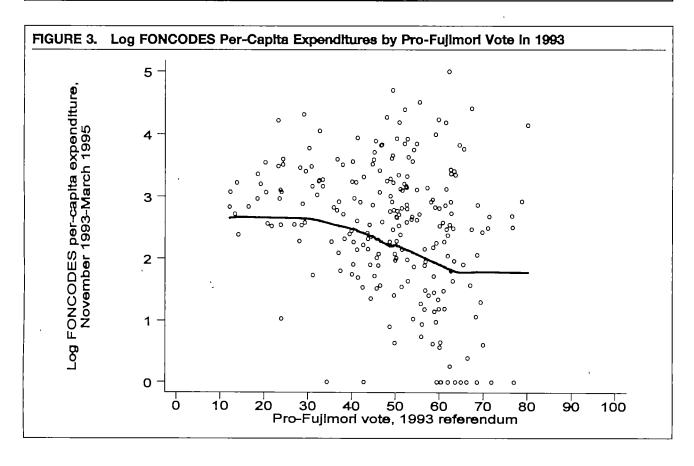
important limitations. First, it frequently does not use a multivariate regression framework to separate political from other effects. To see why this may seriously bias the results, consider expenditure patterns after 1993. FONCODES spent more on poor provinces before and after the 1993 referendum, and these poor provinces largely voted against the government in 1993. Were the expenditures made by FONCODES after October 1993 motivated by electoral considerations, redistributive considerations, or both? Multivariate regression analysis allows for a parsing out of the "political" and "poverty" influences on the distribution of FONCODES funds.

Second, previous studies generally test for a linear effect of the last election on the distribution of expenditures (Graham and Kane 1998). This approach does not allow for nonlinearities, which are at the heart of the marginal voter models, or the dynamic changes inherent in models that take into account more than one election.¹⁵

Third, earlier work uses the larger department-level (rather than province- or district-level) information on the distribution of FONCODES expenditures and the vote in various elections. This seriously diminishes the sample size and makes it difficult to run meaningful

 $^{^{14}}$ Nonparametric regression analysis can be thought of as a moving average. The expectation of Y (in this case, the log of per-capita FONCODES expenditures) is estimated conditional on X (the progovernment vote in the 1990 election or the 1993 referendum) without imposing any assumptions about functional form. It is therefore a much more flexible and accurate way to represent bivariate associations than, say, ordinary least squares (OLS). For an excellent introduction to nonparametric regressions see Deaton 1997, 190–203, as well as Silverman 1986 and Hardle 1991. To avoid dropping provinces that received zero transfers, I added 1 to per-capita expenditures in every province before taking logs (see Maddala 1983, 181, for a similar ad hoc correction).

¹⁵ This is a shortcoming in much of the empirical literature on political influences on expenditure patterns. Wright's (1974) analysis of geographic patterns in the distribution of discretionary funds during the New Deal is one early and important exception. More recently, Case (1997) finds evidence that expenditure patterns in Albania conform to the predictions made in the Lindbeck-Weibull (1987) model



regressions. Even more important, in Peru there are often very large differences in the vote within departments. In the department of Amazonas, for example, only 23.2% of the population in the province of Condorcanqui but 60.0% of the population in neighboring Bongara voted for the government in 1993. More formally, the total variance in the distribution of the progovernment vote can be decomposed into interand intradepartment components (e.g., Cowell 1995). Such a decomposition shows that less than half the total variation across provinces in the vote in the 1990 election and the 1993 referendum (43.0% and 46.8%, respectively) is accounted for by differences across departments. Since all this information was published by province, it is hard to believe that the government would deliberately choose not to make use of it.

Finally, the results presented by Graham and Kane (1998) and others do not acknowledge that estimates of the influence of elections on FONCODES expenditures after 1993 may be contaminated by simultaneity bias. I now turn to this issue.

Econometric Specification

Formally estimating the (possible) effect of elections on expenditures presents a number of econometric complications. Simultaneity bias is perhaps the most obvious. Consider the following equation.

$$\operatorname{Exp}_{pr} = \alpha + \beta \operatorname{Elec}_{pr-1} + 8z1_{pr} + \varepsilon_{pr}, \qquad (3)$$

where Exp_{pt} is per-capita expenditures in province p at time t, $\operatorname{Elec}_{pt-1}$ is the outcome of an election in

province p in the previous period t-1, $z1_{pt}$ is a vector of other variables that affect the distribution of expenditures, and ε_{pt} is the error term. If the error term is autocorrelated over time, it could be rewritten as:

$$\varepsilon_{pt} = \rho \varepsilon_{pt-1} + \nu_{pt}, \tag{4}$$

where ρ is the autocorrelation coefficient. Equation 3 then becomes:

$$\operatorname{Exp}_{pi} = \alpha + \beta \operatorname{Elec}_{pi-1} + \delta z 1_{pi} + \rho \varepsilon_{pi-1} + \nu_{pi}.$$
(5)

Politicians attempt to manipulate expenditures because they believe that these have an effect on elections:

$$Elec_{pt} = \chi + \gamma \ Exp_{pt} + \eta z 2_{pt} + w_{pt}, \qquad (6)$$

where $z2_{pt}$ is a vector of other variables that affect election outcomes, and w_{pt} is the error term. If both equations 5 and 6 hold, then it can easily be shown that estimating the parameter β by OLS will produce results that are biased and inconsistent (e.g., Deaton 1997, 97-8). The intuition is that electoral outcomes are "contaminated" by expenditures in the previous period(s).

Bias is induced when both conditions—serial correlation in the error term and an effect of expenditures on elections—are met. In practice, there is often good reason to suspect that the error term ε_{pi} is positively autocorrelated over time. Demand-driven programs like FONCODES probably involve a cumulative acquisition of skills and knowledge by potential beneficia-

ries: It takes time to understand what kinds of projects are eligible for funding, practice to prepare a good proposal, and time to develop contacts with the social fund staff who are ultimately responsible for review of the application. If there is such a learning curve, then communities that prepare a successful proposal once should find it easier to do so again, whereas those who are turned down may decide that applying for funding is not worth the effort. In terms of equation 5, positive and negative shocks to the error term in one period are likely to be carried forward into the next period. Meanwhile, the presumption that social expenditures affect electoral outcomes seems reasonable. Since there may well be serial correlation and an effect of expenditures on elections, studies that disregard the possibility of simultaneity (e.g., Bruhn 1998; Graham and Kane 1998) must be treated with caution.16

The standard solution to this simultaneity problem is instrumental variables, but the search for appropriate instruments is not simple. Variables that affect elections tend to affect expenditures as well. The Peruvian case offers a partial solution, however, because we may legitimately be able to treat the 1990 election as exogenous to the model. Because FONCODES did not fund any projects until more than a year after the 1990 election, that election cannot have been affected by FONCODES expenditures, equation 6 does not apply, and simultaneity should not be a source of bias in the parameters estimated in equation 3 for the December 1991-October 1993 period. Therefore, the break in FONCODES expenditures before December 1991 may work as a "natural experiment" that allows us to identify the effect of an election on expenditures (a general discussion of these issues can be found in Angrist and Krueger 1999; Deaton 1997, 112-5). More generally, this result highlights the value of research that focuses on new social programs to estimate the effect of elections on public expenditure patterns.¹⁷ Estimates of the effect of elections on expenditures after October 1993 are more open to question because the 1993 referendum may itself have been determined in part by FONCODES expenditures in the preceding period.

Lack of comprehensive data and censoring are both potential sources of bias. Information on the results of the 1990 presidential run-off is not available for 19 provinces. Also, seven new provinces were created

between 1990 and 1995. As a result, we have data on FONCODES expenditures for 231 provinces but data on elections for only 205 provinces.

There is nothing distinctive about the provinces for which electoral data are missing—they are not, for example, particularly poor or rich, or concentrated in one part of the country. As a safeguard, however, I ran additional regressions in which the missing values were replaced with the relevant departmental averages. ¹⁸ The results (not reported) are very similar to those which do not include these provinces, which suggests that this is not a source of bias.

FONCODES financed no community-based projects in twelve provinces between December 1991 and October 1993 and in another twelve provinces between November 1993 and March 1995 (six of the provinces overlap). Censoring of this sort can make OLS estimates biased and inconsistent. Tobit regressions are routinely used as a solution to this problem, but Tobit estimates will also be biased and inconsistent if, as is often the case, the distribution of the error term is heteroskedastic. An alternative estimator, Powell's censored least absolute deviations (CLAD) estimator, is unbiased and consistent but requires large amounts of data (see Deaton 1997, 85-90, for an excellent introduction to these issues). In practice, the bias induced by censoring in the regressions in this article is likely to be small because censoring affects less than 6% of the sample. I therefore present OLS estimates and note when the Tobit or Powell estimators produce noticeably different results.

Estimates of Political Effects: 1991-93

Table 3 presents estimates of the effect of various parameters on the logarithm of per-capita FON-CODES expenditures between December 1991 and October 1993. Column 1 reports the result of an OLS regression on three parameters of interest only: the measures of marginality, core support, and initial consumption (Cox and McCubbins 1986; Dixit and Londregan 1996; Lindbeck and Weibull 1987). "Nonmarginality" is proxied by the absolute deviation of the vote from 50% (as in Case 1997) and core support by the vote for Fujimori¹⁹; income is proxied by the composite poverty index developed by FONCODES in 1996.²⁰

¹⁶ A similar problem can obviously bias the results in regressions of observed electoral outcomes on expenditures (e.g., Weyland 1998), if the distribution of these expenditures was itself politically motivated. ¹⁷ Unless, of course, the new program was promised before an election, the promise was regarded as credible, and it influenced the outcome of the election (Avinash Dixit, personal communication with the author, April 1998). Fujimori committed himself to not implementing the kind of shock program advocated by Mario Vargas Llosa, a commitment he quickly broke after the election. His platform also made references to continued subsidies for basic consumer goods and to the promotion of small domestic industries. (I thank an anonymous referee for this information) The results for the 1991-93 period will only be contaminated by simultaneity bias, however, if Fujimori's campaign promises were credible and specific enough to enable voters in different provinces to predict differences in the transfers they could expect to receive from a program such as FONCODES (which was never mentioned in the campaign).

¹⁸ For example, there are no electoral results for the seven provinces in the department of Apurímac, but we know that 81.54% of the population of the department voted for Fujimori. These regressions, therefore, would assume that the pro-Fujimori vote in each of the seven provinces was 81.54%.

¹⁹ Alternatively, core supporters could be approximated by the vote for Fujimon in the first round and swing voters by the additional vote for Fujimori in the second round (Chang Tai-Haieh, personal communication with the author, September 1998). A disadvantage of this procedure in the Peruvian context is that it would further limit the sample size, as the results of the first round are available only for 180 of the 224 provinces that existed at the time.

²⁰ FONCODES has made adjustments to the index in virtually every year—dropping some individual indicators, adding new ones, and changing the weights given to each. The indices used before 1996 were disaggregated at the province level only. Since I disaggregated the province of metropolitan Lima into its individual districts, only the 1996 index could be used for the analysis. Because most of the

TABLE 3. The Effect of the Second-Round 1990 Presidential Election on the Distribution of Per-Capita FONCODES Expenditures for Community-Based Projects, December 1991– October 1993

	(1)	(2)	(3)
1990 election:	0.023**	0.028**	0.023*
Percentage vote for Fullmorl	(0.009)	(0.009)	(0.011)
1990 election:	-0.002	-0.010	-0.030*
absolute value (% for Fullmorl–50%)	(0.009)	(0.011)	(0.014)
Log (FONCODES	0.696**	* 0.456**	_
poverty Index)	(0.115)	(0.175)	
Log (population)	_	-0.046	0.179**
		(0.078)	(0.068)
Percentage rural	_	0.006	-0.002
· ·		(0.003)	(0.006)
Additional controls	No	No	Yes
F-test	9.61***	11.85***	2.34
R ²	0.434	0.445	0.781
Number of observations	205	205	205

Note. Standard errors corrected for heteroekedasticity are reported in parentheses. An F-test for joint eignificance of both political variables is also reported. A constant was calculated but not reported. Additional controls: A Fourier sense expansion of the FONCODES poverty index; province-level averages for the age of the household head; the average number of years of schooling of household members, the occupation of all adults in the household; separate variables for the proportion of households who own a car, a refrigerator, color TV, and black-and-white TV; the proportion of the population that is indigenous (defined as those whose mother tongue is a language other than Spanish); the infant mortality rate; a dummy variable for border provinces; and 25 departmental dummies $^*p < .06$, $^{**p} < .01$, $^{***p} < .001$.

Note that one cannot test a dynamic model for 1991–93 because Fujimori was first elected in 1990. Moreover, given the dramatic changes in the Peruvian political landscape in the early 1990s and the fact that Fujimori was not the candidate of an established party, he could not reasonably hope to extract information from presidential elections held before 1990.

If variables are omitted from the model, and if these variables are correlated with the measures of marginality, core support, or the FONCODES poverty index, then the coefficients on these parameters will be biased and inconsistent in the usual way. I address this problem by including a large number of controls. The OLS regression for column 2 in Table 3 includes parameters for the (log of the) population of a province and the proportion rural. The large *t*-statistic on the FONCODES poverty index in columns 1 and 2 suggests that

individual indicators tend to be highly correlated with one another, however, the choice of index is unlikely to have an important effect on the results. The results in tables 3 and 5 are robust to alternative measures of income, such as the province-level estimates of imputed income and imputed proportion below the poverty line developed by INEI. (An explanation of these and other district-level welfare measures can be found in Schady 1999.)

it is important for the relationship between expenditures and the index to be specified correctly. To account for possible nonlinearities in this relationship. I include a Fourier series expansion of the FON-CODES poverty index in column 3.21 In addition, this regression includes numerous socioeconomic controls listed at the foot of the table and a dummy variable for border provinces. This border dummy is included as a control because FONCODES may have spent differently in areas near the volatile Ecuadorian border.22 Finally, the specification in column 3 includes 25 department-level dummies. Regional offices of FON-CODES generally correspond to departments, and these dummies are meant to capture differences in institutional capacity across offices as well as any other possible department-level differences.

The results in Table 3 show that marginal voters, core supporters, and the poor all received a disproportionately large share of FONCODES expenditures. The coefficient on the measure of nonmarginality is always negative and is significant at the 5% level when all the controls are added; the coefficient on the measure of core support is positive and significant at the 5% level in every specification.²³ Note that this is a very strong result indeed: The political variables are significant even after dozens of controls, including sophisticated nonlinear measures of the poverty of a province, are incorporated into the model. Finally, the coefficient on the FONCODES poverty index is positive and significant at the 1% level, which shows that FONCODES spent significantly more on the poor. The sign on the coefficients when the parameters are estimated with Tobit or Powell's CLAD is the same, and the magnitude of the effects of the political variables and the FONCODES poverty index tends to be larger

²¹ The Fourier series expansion includes terms for the log of the FONCODES poverty index, its square, the sine of the log of the index and the sine of twice the log of the index, and the cosine of the log of the index and the cosine of twice the log of the index. As usual, the logarithm of the FONCODES poverty index had to be rescaled to prevent values smaller than $-\pi$ or larger than π . The coefficient on the FONCODES poverty index is not reported in column 3 of Table 3, or in columns 3 and 6 of Table 5, because it is meaningless on its own when the square, sine, and cosine parameters are included in the regression.

²² A 150-year-old border dispute between Ecuador and Peru simmered throughout the early 1990s and broke out into open warfare in early 1995. Much of the area along the border is extremely remote jungle, mainly inhabited by indigenous people, with little connection to the formal Peruvian (or Ecuadorean) economy. There are anecdotal accounts which suggest that state institutions, including FON-CODES, were pressured to "develop" these border provinces. In none of the estimations in this article, however, did the border dummy variable, or a separate dummy only for provinces along the Peru-Ecuador border, have a significant positive coefficient. A permanent peace agreement between the two countries was signed in Brasilia on October 26, 1998.

²³ If politicians maximize the probability of being reelected, rather than the expected number of votes, then the positive coefficient on the pro-Fujimori vote could also be a measure of the "partisan reward" for the supporters of the stronger party predicted in the modified Lindbeck-Weibull model (Case 1997; Lindbeck and Weibull 1987). More generally, there is no obvious way to disentangle the effect of core support from that of partisan reward when the incumbent is also the stronger of the two parties (Mark Thomas, personal communication with the author, March 1998).

TABLE 4. The Magnitude of Political and Poverty Effects on the Distribution of Per-Capita FONCODES Expenditures for Community-Based Projects, December 1991– October 1993

	(1)	(2)	(3)
1990 election: Percentage vote for Fujimori	43.29	54.83	44.88
1990 election: absolute value (% for Fujimorl- 50%)	-2.65	-12.54	-39.67
Log (FONCODES poverty Index)	56.08	33.86	

Note: Each cell reports the effect of a change of one standard deviation in the variable in question on the distribution of per-capita FONCODES expenditures for community-based projects. Columns 1, 2, and 3 correspond to columns 1, 2, and 3 in Table 3.

than the corresponding OLS estimates. The only important difference is that the CLAD estimates produce larger standard errors, so that the political coefficients are no longer significant when all the controls are included.

How large are these estimated effects? Table 4 reports the predicted effect of a change of one standard deviation in core support, marginality, and the (log of the) FONCODES poverty index on per-capita FONCODES expenditures. Column 2, which corresponds to column 2 in Table 3, shows that, ceteris paribus, an increase of one standard deviation in the vote for Fujimori resulted in a 55% increase in per-capita expenditures, whereas a similar increase in marginality resulted in a 13% increase in per-capita expenditures. Increases in poverty also have a large influence on expenditures: An increase of one standard deviation in the (log of the) FONCODES poverty index resulted in a 34% increase in per-capita expenditures.

Table 4 suggests that the effect of the two political variables is large indeed, especially as the number of controls increases. But interpretation of the magnitude of the political effects is not straightforward because marginality and core support work against each other at levels of support for Fujimori above 50%. For instance, a province in which 75% of the population voted for Fujimori could expect to receive large or small per-capita transfers, depending on whether the effect of core support or marginality is more important. To clarify this matter, Figure 4 graphs predicted expenditures at various levels of support for Fujimori for each of the specifications in Table 3. Other variables in the regressions are kept constant at their means.²⁴ Note that Figure 4 therefore summarizes both the core supporter and marginal voter effects. If only core supporters affect the distribution of expenditures, then the predicted per-capita expenditure schedule should be smooth, with no kink at the 50% point. Conversely, if only marginal voters affect the distribution of expenditures, then the line should be perfectly symmetrical above and below the 50% kink. Figure 4 shows that the estimated influence of core support is much larger than that of marginality when only the two political variables and the FONCODES poverty index are included in the regression (specification 1), but the effect of marginality increases noticeably with the number of controls (specification 3).

Estimates of Political Effects: 1993-95

The Peruvian political landscape changed dramatically between 1990 and 1993. In the 1993 referendum, Fujimori generally did well in wealthier urban areas, especially Lima, but he lost the support of large segments of lower class and rural voters who had brought him into office in 1990 (Roberts and Arce 1998; Stokes 1996b). What effect did these massive shifts in Fujimori's constituency have on the distribution of FONCODES expenditures?

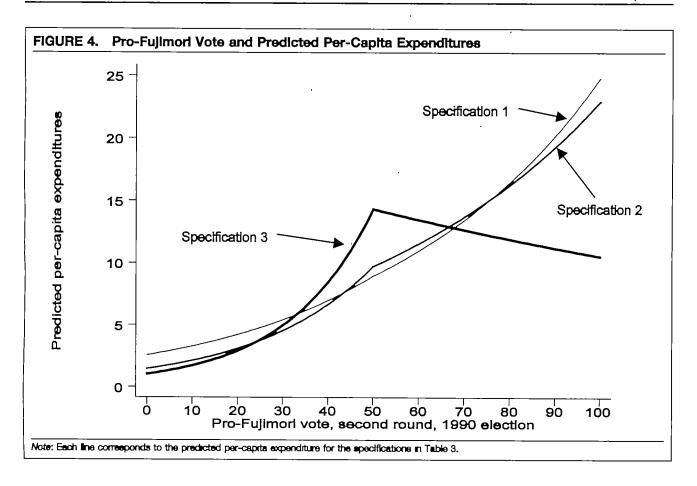
Table 5 reports results from a set of regressions of per-capita FONCODES expenditures between November 1993 and March 1995 on the 1993 referendum. Columns 1 through 3 are based on regressions of expenditures on measures of core support and nonmarginality from the referendum. These specifications therefore test a model similar to that found so important for the 1991-93 distribution of FONCODES expenditures, updated with the results of the 1993 referendum. Columns 4 through 6 are based on regressions of expenditures on the change in the pro-Fujimori vote.25 If Fujimori were trying to buy back the vote in errant provinces, then the coefficient on the change in the vote would be negative; conversely, if Fujimori were attempting to consolidate his power base in provinces with new-found supporters, then the coefficient would be positive.26

The results in Table 5 should be interpreted with caution. As I argue above, if FONCODES expenditures between December 1991 and October 1993 affected the outcome of the 1993 referendum, then any regression of expenditures after October 1993 that includes the results of the referendum as an explanatory variable is likely to be contaminated by simultaneity bias. Yet, with this caveat in mind, the results in Table 5 suggest that *changes* in the vote between 1993 and 1990, rather than simply the level of government

²⁴ Mechanically, this can be done by multiplying the coefficient on nonpolitical parameters in the regression by the mean value for the parameter in question and then adding them to the constant in the regression. The coefficients on the measures of core support and nonmarginality are then used to predict expenditures at every level of the vote between 0 and 100%.

²⁵ "Change" is defined as the 1993 pro-Fujimori vote minus the 1990 pro-Fujimori vote, divided by the pro-Fujimori vote in 1990. Dividing through by the results of the 1990 presidential election recognizes that, say, a 5% reduction in the pro-Fujimori vote is unlikely to mean the same thing in a province in which 70% of the population voted for Fujimori in 1990 as in one in which the level of support in 1990 was only 20%.

²⁶ One shortcoming of an approach that focuses on changes in the vote is the fact that the 1990 presidential election and the 1993 referendum were different in character. In the referendum, voters could "safely" express opposition to part of Fujimori's program without having to choose an alternative (Graham and Kane 1998, 82, Roberts and Arce 1998, 229).



support in the 1993 referendum, influenced the distribution of FONCODES expenditures after 1993. The coefficients on the measures of marginal vote and core support are jointly insignificant at the 5% level in all the specifications. By contrast, the coefficient on the change in the pro-Fujimori vote is negative and significant at the 1% level or more in two of the three specifications in Table 5. Roberts and Arce (1998) argue that Fujimori attempted to "recapture the loyalty of the popular sectors" by picking up the pace of poverty-alleviation programs after 1993. The results in Table 5 suggest that social expenditures channeled through FONCODES were used for such a buy-back strategy in a very focused way: FONCODES projects were specifically directed at provinces that supported Fujimori in 1990 but abandoned him in 1993.

Table 5 shows that poverty continued to be an important determinant of per-capita expenditures: The coefficient on the (log of the) FONCODES poverty index is positive, large, and highly significant in all specifications. Rurality is also associated with significantly higher per-capita expenditures (columns 2 and 5).²⁷ This finding does not conform with the extensive literature in the political economy of development that argues that vocal and organized urban constituencies in LDCs siphon off the bulk of public funds (Bates 1981;

Graham 1997; Nelson 1992). FONCODES expenditures between November 1993 and March 1995 favored rural areas, perhaps because other social programs in Peru at the time were directed at urban populations.²⁸

What, finally, can we say about the magnitude of these effects? Table 6 summarizes the effect of a change of one standard deviation in the log of the FONCODES poverty index and the measure of the change in pro-Fujimori support on the distribution of per-capita FONCODES expenditures. The results suggest that the political influence on the distribution of FONCODES expenditures continued to be large: A decrease of one standard deviation in the pro-Fujimori vote resulted in an increase of between 16% and 34% in per-capita expenditures. The effect of poverty on the FONCODES expenditures appears to have increased after the referendum. This result is consistent with other work (Paxson and Schady 1999), which shows that the targeting of FONCODES investments to the poor improved over time.29

²⁷ Interpretation of the coefficients on the proportion rural and population size in columns 3 and 6 of Table 5 is not straightforward because of the high degree of colinearity between these variables and all the additional controls. The same caveat obviously applies to column 3 in Table 3 as well.

²⁸ These included two housing programs (ENACE, Banco de Materiales); a program for the construction of electricity, water, and santtation systems (UTE-FONAVI); a small welfare program for destitute families (INABIF); and a school construction program that spent mamly in urban areas (INFES) (see World Bank 1996).

²⁶ Parson and Schady (1999) present nonparametric regressions for FONCODES expenditures on education projects as a function of mean district per-capita meome, by year. The regressions show that the geographic targeting of FONCODES projects clearly improved between 1992 and 1995.

TABLE 5.	The Effect of the	1993 Referendu	m on the Di	stribution of	Per-Capita FONCODES
Expenditu	res on Community	y-Based Projects	, November	1993-March	n 1995

	Level of	Pro-Fujimori V Referendum		Change	e in Pro-Fujim	on Vote
	· (1)	(2)	(3)	(4)	(5)	(6)
1993 referendum: Percentage vote for Fujimori	-0.005 (0.004)	-0.005 (0.003)	-0.010 (0.005)	_	- .	_
1993 referendum: absolute value (% for Fujlmorl–50%)	-0.006 (0.005)	-0.011 * (0.005)	0.000 (0.006)	—	_	
Change In pro-Fullmorl vote (vote 90 - vote 93)/vote 90	_	_	_	-0.283 (0.161)	-0.391 ** (0.151)	-0.560** (0.191)
Log (FONCODES poverty Index)	1.01*** (0.058)	0.746*** (0.082)	_	0.879*** (0.092)	0.55 4*** (0.121)	_
Log (population)	_	-0.087 (0.066)	0.037 (0.077)	_	-0.101* (0.055)	0.010 (0.057)
Proportion rural	_	0.007** (0.002)	-0.005 (0.005)	_	0.007** (0.002)	-0.007 (0.005)
Additional controls	No	No	Yes	No	No	Yes
F-test	0.86	2.57	1.95		_	_
R ²	0.514	0.548	0.769	0.529	0.567	0.803
Number of observations	230	230	230	205	205	205

Note: Standard errors corrected for heteroekedastrorty are reported in perentheses. An F-test for joint significance of both political variables from the 1993 referendum is also reported. A constant was calculated but not reported. For a list of additional controls, see note to Table 3. *p < 05, **p < 01, ***p < 001, ***p < 001.

CONCLUSION

Much theoretical and empirical work on public expenditure patterns in democratic regimes argues that politicians manipulate the timing, composition, and geographic distribution of expenditures to maximize the likelihood of remaining in office. Increases in public expenditures are timed to coincide with upcoming elections; budgetary trade-offs are made to satisfy important constituencies; and expenditures are directed toward areas of the country that are considered critical for an incumbent's reelection bid.

The bulk of studies on political influences on expenditures in LDCs and elsewhere has focused on aggregate macroeconomic variables (e.g., Ames 1987; Remmer 1993). This literature has shown that politicians frequently manipulate aggregate demand and large-scale subsidy or universalist social programs to suit

TABLE 6. The Magnitude of Political and Poverty Effects on the Distribution of Per-Capita FONCODES Expenditures for Community-Based Projects, November 1993– March 1995

	(4)	(5)	(6)
Decrease in the pro-Fujimori vote	15.80	22.43	33.62

Log (FONCODES poverty Index) 75.44 42.53

Note: Each cell reports the effect of a change of one standard deviation in the variable in question on the distribution of per-capita FONCODES expenditures for community-based projects. Columns 4, 5, and 6 correspond to columns 4, 5, and 6 in Table 5.

their electoral ambitions. Such manipulation may now be less feasible, however. An increasingly global environment makes countries more vulnerable to sudden losses of investor confidence and capital flight (e.g., Rodrik 1999), and the costs of blatant political interference in the macroeconomy may be prohibitive. At the same time, many universalist social programs have ceased to exist or have been cut along with public sector budgets. Understanding how politicians, in Peru and elsewhere, attempt to manipulate small-scale, targeted poverty-alleviation programs is particularly timely.

This article uses formal econometric modeling and a rich, highly disaggregated data set to analyze the effect of elections on the timing and distribution of expenditures made by the Peruvian Social Fund between 1991 and 1995. An important advantage of this approach is that it allows the political and poverty influences on FONCODES expenditures to be parsed out. I argue that there is a large, significant effect of elections on the timing and distribution of FONCODES expenditures. These were boosted before national elections, and community-based projects were channeled to provinces where the political returns were expected to be large. Moreover, the Fujimori administration altered its expenditure patterns to take into account the worsethan-expected government performance in the 1993 referendum.

The results of this study show that FONCODES has clearly been responsive to political imperatives. Indeed, it was arguably very well suited for President Fujimori's electoral ambitions. Unlike most programs

conducted by the line ministries in Peru, FONCODES expenditures are truly discretionary. Since 1993, FONCODES has had a backlog of thousands of proposals that could be funded at opportune times in provinces deemed electorally important. Polls suggest that FONCODES projects are closely identified with the presidency and with Fujimori himself (Moncada 1996, 62).

But FONCODES also appears to have had an important redistributive function. My results show that funds flowed disproportionately to poor provinces. Household survey data indicate that the infrastructure constructed by FONCODES reaches a larger fraction of households, especially poor ones, than other programs in the Ministry of the Presidency (Paxson and Schady 1999; World Bank 1999). Such success in reaching the poor can in part be explained by the fact that FONCODES is free of many of the restrictions, much of the paperwork, and many of the inefficiencies that have given the public sector in Latin America a bad name.

Was FONCODES an effective poverty-alleviation program or a brazen attempt to buy votes? There is no simple answer because decisions about funding apparently were made on the basis of both political and poverty criteria. Ironically, it is precisely those features that enabled FONCODES to reach the poor—such as a high degree of flexibility in the allocation, timing, and composition of expenditures—which made it particularly vulnerable to political interference. This tension between effective redistribution and pork barreling need be no coincidence: Nelson (1992) argues that programs for the poor are much more likely to be implemented when the executive keeps control over patronage.

The government of President Alberto Fujimori has privatized state-owned enterprises, liberalized markets, and slashed subsidies and tariffs in Peru. Fujimori has thus eliminated many of the tools traditionally used by incumbents to enlist the support of important constituencies before an election (Grindle 1996; Herbst 1990). Of course, low inflation and high rates of economic growth also bring important returns at the polls, returns that may far exceed those from the more interventionist policies of yore (Nelson 1992; Przeworski 1993; Stokes 1996a). This article shows, however, that old populist habits die hard.

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Free to Trade: Democracies, Autocracies, and International Trade

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Relatively little research has focused on whether countries' political institutions affect their international trade relations. We address this issue by analyzing the relationship between regime type and trade policy. In a formal model of commercial policy, we establish that the ratification responsibility of the legislature in democratic states leads pairs of democracies to set trade barriers at a lower level than mixed country-pairs (composed of an autocracy and a democracy). We test this hypothesis by analyzing the effects of regime type on trade during the period from 1960 to 1990. The results of this analysis accora with our argument: Democratic pairs have had much more open trade relations than mixed pairs.

The effects of domestic political institutions on trade policy have long been a source of controversy among social scientists. Despite the widespread interest that this debate has prompted, however, remarkably few studies have addressed the links between political regime type and commercial policy.1 Most work on the political economy of trade policy that examines institutional factors focuses only on variations in policy among democracies and avoids comparing democracies with other types of regimes. Moreover, many analyses of trade barriers—especially those by economists—ignore the effects of domestic political institutions. In light of the recent interest expressed in how regime type influences various aspects of foreign policy (e.g., Doyle 1986; Farber and Gowa 1995; Russett 1993; Siverson and Emmons 1991), a systematic analysis of the links between regime type and trade policy seems long overdue.

Here, we examine whether groups of democracies are better able to liberalize trade than are groups of autocracies or groups comprised of both democracies and autocracies. By providing one of the first assess-

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ments of whether patterns of commercial protection depend on regime type, we help shed light on whether there is a "democratic difference" in foreign economic policy. Our study also should contribute to a broader understanding of the political economy of trade policy. One recent analysis concludes: "Theoretical and empirical work relating [domestic] institutional contexts to trade policy outcomes is in its infancy but should be a promising area of research" (Rodrik 1995, 1485). We seek to help fill this important gap in the literature.

We develop a formal model that emphasizes the role of domestic legislatures in democracies. Central to this model is that the chief executive (i.e., prime minister or president) in a democracy needs the approval of a legislative majority to enact her preferred trade policies.2 Absent such approval, the legislature's preferred policy becomes law. We argue that legislative "ratification" of commercial policy occurs in both parliamentary and presidential systems. In parliamentary systems, ratification is often ex ante; the potential prime minister must negotiate an acceptable trade policy with her legislative majority before taking office. This internal bargain is enforced afterward because the majority can call for a vote of (no) confidence if the prime minister strays from the accepted position in international economic negotiations. In presidential systems, major changes in trade policy usually require legislative ratification ex post.

By contrast, autocracies vest the chief executive with much more authority than democracies. Autocrats do not need a legislative majority to pass their trade policy initiatives; either a legislature does not exist or it rubber-stamps executive proposals. We claim that this institutional difference contributes to a greater tendency for pairs of democratic countries to agree upon lower trade barriers than pairs comprised of a democracy and an autocracy (i.e., mixed pairs). We further demonstrate that whether pairs of autocracies liberalize trade more extensively than either pairs of democracies or mixed pairs depends on the preferences of the

¹ For a few exceptions, see Bliss and Russett 1998, Dixon and Moon 1993, and Morrow, Siverson, and Tabares 1998.

² We are not arguing that autocracies do not need domestic political support. Our point is that autocrats do not require the majority approval of a legislature elected by the public to represent its interests. Autocrats may well depend on the support of such actors as the military or industrialists, but these groups do not represent the voting public.

political decision makers involved, as well as on institutional differences between their regime types. Hence, we can make no prediction about the trade barriers of autocratic pairs relative to mixed pairs or democratic pairs without knowledge of the respective executives' trade policy preferences.

Our focus differs sharply from studies that address how institutional variations among countries with the same regime type affect trade policy. Crucial to the following analysis is that the institutional feature which distinguishes democracies from autocracies is the existence of a popularly elected legislature with the capacity to constrain a country's chief executive. We assume that legislatures are more protectionist than executives in democracies and show that, even so, pairs of democracies are more likely than mixed pairs to liberalize commerce.³ Our focus, then, is on differences across regime types not within them.

The results of an empirical analysis based on the period 1960–90 are consistent with these predictions. Holding constant various economic and political factors, democratic dyads tend to trade more freely than dyads composed of a democracy and an autocracy. Furthermore, no significant difference exists in the openness of commerce within autocratic pairs and within democratic pairs.

THE MODEL

The Actors and Their Preferences

A key difference between an autocracy and a democracy is that the latter is vested with a legislature that has de jure and de facto ratification power over the chief executive's proposals. Because no such institution exists or has such powers in an autocracy, it is modeled as a unitary actor; it has only an executive, who is effectively a dictator and is labeled A. In contrast, a democracy is characterized by two actors: an executive, who may be the president or prime minister, P, and a legislature, represented by the median legislator, C. In what follows, we consider the case in which two countries enter into negotiations to reduce trade barriers between them. The labels A, P, and C refer to the "home" country in these negotiations, whereas A^* , P^* , and C^* refer to the "foreign" country.

In the home country, each of the aforementioned actors tries to maximize its utility by obtaining the greatest possible level of electoral support. That support depends on the trade policy chosen. Every actor wants to enact the level of trade barriers that will maximize its political support (i.e., its ideal point). But each one must strike a balance among the conflicting preferences of its constituents to determine the ideal level. Actors receive political support from a variety of domestic groups. Some groups prefer that the home country lowers trade barriers (e.g., consumers, export-

ers, users of imported inputs, owners of relatively abundant factors), and others prefer that it raises barriers (e.g., import-competing firms, owners of scarce factors). All groups, however, prefer that the foreign country's trade barriers be as low as possible, an outcome that benefits consumers, exporters, and import-competing firms alike. Ceteris paribus, all domestic groups will increase their electoral support for policymakers in the home country if these officials negotiate very low levels of protection abroad. We refer to the home country's trade barriers as t and those of the foreign country as t^* .

The political support function of each actor, U_i , can be expressed as a simple loss function. Every actor has an ideal level of trade barriers at home and abroad, denoted (t_i, t_i^*) , that i would choose if it were not constrained by any of the other actors. All domestic actors prefer the elimination of foreign trade barriers; hence, $t_i^* = 0$ for i = P, C, A, and $t_i = 0$ for $i = P^*$, C^* , A^* . Each actor maximizes its utility by minimizing the difference between the level of protection that it prefers both at home and abroad and the level that actually obtains. For the home country, i's utility function is

$$U_i(t, t^*) = -(t - t_i)^2 - t^{*2}$$
 for $i = P, A, C$. (1)

For the foreign country, its actors attempt to maximize

$$U_i(t, t^*) = -t^2 - (t^* - t_i^*)^2$$
 for $i = P^*, A^*, C^*$.
(1*)

These utility functions imply that the actors' indifference curves are circles. Their utility decreases symmetrically as changes occur in any direction away from their ideal level of trade barriers at home and abroad. Such utility functions are very common to spatial models (e.g., Enelow and Hinich 1984).

Our approach to the making of trade policy is consistent with behavior in both democracies and autocracies. For instance, it captures the notion that democratically elected politicians are motivated by a desire to be reelected, the chances of which grow larger in the face of both rising welfare experienced by voters

³ If legulatures are not more protectionist than executives, then the results depend on the actors' preferences. Differences in trade policy will depend on both institutions and preferences in that case. We focus instead on the case in which the effect of institutional variation is robust to any preference ordering among the players.

⁴ This formulation of the objective functions bears some similarity to Krugman's GATT-think (Ethier 1998; Krugman 1997), in which trade negotiators are motivated by mercantilist notions: Exports are good, imports are bad, and an equal increase in exports and imports is good.

⁵ As implied above, the objective functions 1 and 1* can be seen as the reduced form of a model that combines elements of the underlying economy with a political game of policymaking. For instance, if political support (as in Hillman 1982) is a function of consumer surplus (CS), tariff revenue (T), profits of the import-competing firms (Profits₁), and profits of the exporting firms (Profits₁), then for the home country, $(t_i, t_i^*) = \operatorname{argmax} F(\operatorname{CS}(t, t^*), \operatorname{Profits}_1(t), \operatorname{Profits}_1(t^*), \operatorname{and} T(t))$ for t = P, C, A, where F is increasing in each argument (Hillman, Long, and Moser 1995). The exact form of the function F and the weights placed on each term depend on the underlying market demand and supply conditions, the relative political pressure that each group can bring to bear, and the political institutions that mediate this pressure. This approach is consistent with Grossman and Helpman (1994), who posit that the government cares about a weighted average of social welfare and campaign contributions. Then equations 1 and 1* would be the second-order approximations of a political support function around the ideal points.

and increasing campaign contributions (or lobbying activity undertaken) by organized lobbies, such as industry groups. Hence, trade policies that raise consumer surplus and firm profits increase the political support that a politician receives. Consumers are made better off by a rise in their surplus; and because they vote according to their pocketbooks, their support for the government grows. In addition, as firms' profits rise, they distribute higher profits to consumers (who are also stockholders) and make larger contributions to (or exercise greater influence over) politicians. Note that the government responds to the pressures of both its import-competing and exporting firms. We assume that the government does not have a direct instrument to benefit the exporting sector (such as a subsidy), but it can reduce trade barriers abroad through negotiations and reciprocal concessions, thereby benefiting local exporting industries.

In autocracies, the executive must also maintain political support. The extent of the support provided by consumers and firms often depends on economic conditions. For example, a recent analysis notes that, in the wake of Deng Xiaoping's death, China's autocratic leaders "are acutely aware that a vibrant, flourishing, and progressing economy is the core of their legitimacy" (New York Times, February 21, 1997, A15). Again, trade policy has a direct influence on consumer surplus and firms' profits. Greater consumer surplus means higher real incomes, fewer incentives to riot and rebel, and thus more support for autocratic leaders. Similarly, greater firm profits mean higher incomes for at least some groups (the firm owners), implying increased political support for the autocrat. Whether autocrats care more about consumers and the overall state of the economy or the special interests of firms depends on a number of factors that do not concern us here.

The trade policy preferences of autocrats relative to those of democratic leaders are of central concern. It is unclear whether, on average, autocrats are more protectionist than democratic heads of state. Freed from the need to finance expensive electoral campaigns, autocrats may be more attentive to the economy's performance and hence less protectionist than their democratic counterparts. Yet, if autocrats depend only on the support of a small group of powerful interests, they may be more willing to extract economic rents (through trade barriers and other means) from those sectors of the economy in which these interests do not operate. We handle this difficult issue by varying the preferences of the autocrat relative to those of the democratic executive and assessing how changes in their relative preferences combined with the institutional differences between these two regime types affect our results.

In a democracy, the two main political actors often do not agree on the ideal level of home trade barriers; that is, t_P and t_C are unlikely to be identical. The executive and the median legislator may weigh consumer surplus and firm profits differently in their electoral calculus. Legislators, who represent smaller constituencies than the chief executive, are more easily

captured by special interests than the executive (Mansfield and Busch 1995; Rogowski 1987). We assume that the median legislator cares more about the profits of import-competing firms than about consumer surplus and holds more protectionist preferences than the democratic executive (Lohmann and O'Halloran 1994; Rosendorff 1996). Thus, we assume that $t_P < t_C$ and $t_{P^*} < t_{C^*}$.

All domestic agents prefer that, ceteris paribus, foreign barriers be as low as possible. Ideal values for the domestic trade barriers do vary, however. The median voter theorem is therefore applicable: Single-peaked objective functions over a single dimension imply the median voter is decisive (Black 1958). This similarity of preferences allows us to avoid the cycles associated with median voter models in larger dimensions. We can identify the median legislator as that member who holds the median value for the level of domestic trade barriers, and the legislature as represented by this median legislator.

Each player receives the maximum utility when its ideal trade barrier combination is adopted. Maximum feasible utility for each player is $U^P(t_P, 0)$, $U^C(t_C, 0)$, $U^A(t_A, 0)$, $U^{P^*}(0, t_{P^*}^*)$, $U^{C^*}(0, t_{C^*}^*)$, $U^{A^*}(0, t_{A^*}^*)$.

The Noncooperative Trade Barrier Setting Game: The Trade War

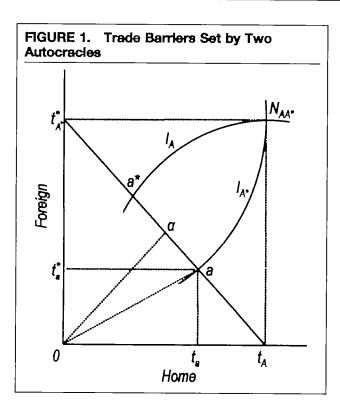
In the absence of any agreement on mutual trade policy concessions, each country is free to set trade barriers only in its own country. Each country sets its trade policy simultaneously, taking the actions of the other as given. The Nash equilibrium to this noncooperative trade barrier-setting game can be considered the "trade war" outcome, which is what we assume occurs when countries do not agree to mutually acceptable trade barrier concessions. Political leaders at home have authority over t. Foreign barriers (t^*) are controlled by the foreign government. When both countries are autocratic (denoted AA*), each executive determines the level of trade barriers in her country. Consider the problem facing the home autocrat, A, who maximizes equation 1 taking t^* as given. A's reaction function is independent of t^* and hence is vertical at t_A . Similarly, \hat{A}^* 's reaction function is independent of t and hence horizontal at $t_{A^*}^*$. The Nash equilibrium is the pair of preferred trade barrier levels (t_A, t_{A^*}) chosen by the home and the foreign autocrat. In Figure 1, this point is labeled N_{AA} . Note that both parties could be made better off if they could negotiate to a point on or closer to the Pareto frontier, $t_A t_A^*$.

When one or both countries are democracies, we assume that the legislature $(C \text{ or } C^*)$ has the final say on domestic trade policy. Again, each domestic play-

⁶ Throughout, we assume that there is a meaningful difference between their preferences; we assume that $t_C > 2t_P$ and $t_C^* > 2t_{P^*}^*$.

⁷ This follows from the additive separability of t and t^* in the objective functions in equations 1 and 1*.

This assumption is not necessary for our results. If the legislature simply has some ability to force the executive to take its preferences into account in the event of the trade war outcome, then our results



er's ideal domestic policy is independent of the actions of the foreign player (and vice versa). When the home country is a democracy and the foreign one is an autocracy (denoted DA^*), the best-response functions call for C to implement t_C for any value of t^* and for A^* to play t_A^* , irrespective of t. The noncooperative game between home and abroad has as its equilibrium $N_{DA^*} = (t_C, t_A^*)$, as labeled in Figure 2A, B, and C. Realized returns to C, P, and A^* are $C(t_C, t_A^*)$, $P(t_C, t_A^*)$, and $A^*(t_C, t_A^*)$. These returns are lower than the respective optima $C(t_C, 0)$, $P(t_P, 0)$, and $A^*(0, t_A^*)$. Pareto improvements are possible; if C, P, and A^* can agree to mutually reduce trade barriers (slightly) from (t_C, t_A^*) , all could gain.

Finally, when both countries are democracies (DD^*) , the Nash equilibrium is set by the two legislatures. In this case, failure to find mutual concessions leads to point $N_{DD^*} = (t_C, t_{C^*}^*)$ in Figure 3.

leads to point $N_{DD^*}=(t_C,t_{C^*}^*)$ in Figure 3. In all three cases, the Nash equilibrium to this simultaneous game of trade barrier setting is the defect-defect outcome in the prisoners' dilemma. If the countries do not cooperate, they will choose a level of trade barriers at home that is ideal, but that results in a higher level abroad than they ideally desire. Since both countries make such a choice, they end up at the noncooperative Nash equilibrium, which is not welfare maximizing. If they coordinate their trade policies, then they can both achieve welfare gains. As such, the problem facing political leaders is to reach a pair of trade barrier levels closer to their optima, and this necessarily involves mutual concessions.

are maintained. The legislature's preferences must simply affect a democracy's unilateral, noncooperative choice of barriers.

The Negotiation Game: TILI

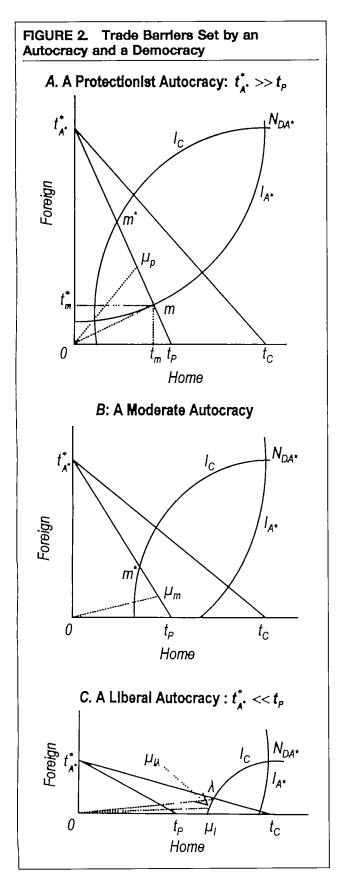
Consider the simplest bargaining game, in which we assume that the actors have perfect information. The players' ideal trade barrier levels at home and abroad are common knowledge, as is the structure of the game. We consider two bargaining structures. In the first game, the home country—by virtue of having the first move—is chosen to make a take-it-or-leave-it (TILI) offer to the foreign country. The latter can either accept and implement this offer or reject it, thereby inducing a reversion to the trade war outcome, that is, the Nash equilibrium. Then we switch the identity of the first mover and consider the same TILI game; that is, the foreign country makes the first move, and bargaining power is reversed. Under either bargaining structure, neither country can dictate the levels of trade barriers that both choose. Both countries know they can gain from making mutual concessions, and they know that everyone must be left at least as well off as they would be in the trade war outcome. We establish that the results are robust to this change in the bargaining structure, and we infer that any structure in which bargaining power is distributed in a less extreme fashion between the players will yield similar results.

For instance, if the parties agree to split evenly the gains from any mutual reductions in trade barrier levels—an approach consistent with the symmetric Nash Bargaining Solution (Nash 1950)—then they will generate outcomes that lie somewhere between that of TILI when the home executive proposes and TILI when the foreign executive proposes. Thus, our results are general to a variety of bargaining structures.

In the TILI game, the moves of the players occur in two steps. First, the home executive, whether democratic or not, makes an offer, specifying a mutually reduced level of home and foreign trade barriers. If both countries are autocracies, then the foreign leader is asked to agree. If home is a democracy and foreign is an autocracy, then the home executive puts her offer to both the foreign autocrat and the domestic legislature. Either player has veto power, which induces a reversion to the trade war outcome. If both countries are democracies, then the home executive picks an offer, which must be acceptable to the foreign executive and both legislatures.9 Failure to obtain legislative ratification in the latter two cases leads to the trade war, or Nash equilibrium, either N_{DA^*} or N_{DD^*} . It is assumed that no player will ever take an action that is dominated: They will not reject an offer that is better than that which will be received in the trade war. The elimination of dominated strategies is equivalent (in this game) to requiring the equilibrium to be subgame perfect.

Note that the structure of preferences in the countries is symmetric. That is, the optimal level of trade barriers preferred by A is the same as that preferred by A^* , and similarly for P and P^* as well as C and C^* . We

⁹ In fact, we require that the international negotiators choose a ratifiable offer that lies on their Pareto frontier when such an agreement exists.



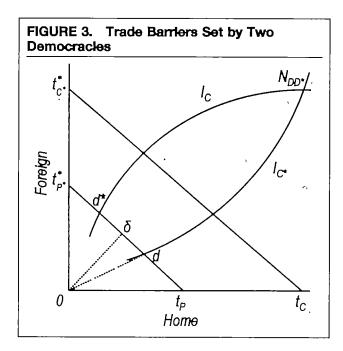
make this assumption because our focus is on differences between regime types, not within each regime type. This assumption, however, has two implications.

First, we do not model what happens when a protectionist autocrat faces an autocrat who prefers to liberalize trade, an interesting issue but one that departs from our focus on variations across regime types.

Second, the symmetry assumption implies that the distance between the democratic executive and its legislature is the same at home and abroad for all democratic dyads. Again, we make this assumption because we are interested in variations between regime types, not within them. Both our model and our data analysis treat each type of regime as homogeneous. Democracies are distinct from autocracies because they have a legislature that exercises ratification power, regardless of its preferences. The differences among regime types rest on institutional features and not on the policy preferences of decision makers. Varying the difference in the ideal policies of P and C does not alter the degree of democracy present; it only changes the extent to which government is divided within democracies. Furthermore, whether a government is divided or unified has no bearing on the degree to which a country is democratic. Hence, examining the influence of such divisions (i.e., as the ideal points of P and C diverge or converge) does not inform us about democracies versus autocracies, only about more divided versus less divided democracies.10 Our model is capable of deriving outcomes when such symmetry of preferences is relaxed, but we have neither the space nor the data in this article to explore these variations within democracies.

Figures 1-3 show the central results. We measure the aggregate level of trade barriers by the sum of the two countries' levels of trade barriers. The indifference curves for the actors C, C^* , A, and A^* through N have been drawn and are labeled I_C , I_{C^*} , I_A , and I_{A^*} , respectively. These curves mark the boundary of the set of mutual reductions in trade barriers that are preferred by each player (respectively, C, C^* , or/and A, A^*) to the noncooperative Nash equilibrium outcome. The solution to the bargaining must lie in this feasible set, often termed the "win set." In addition, the contract curve between the executives A^* (or P^*) and A(or P) has been drawn. It is a straight line between the executives' ideal points (since the indifference curves are circles). The contract curve between C and P (or C^* and P^*) can be thought of as that part of the horizontal (vertical) axis that lies between t_C and t_P $(t_{C^*}^*$ and $t_{P^*}^*$). More generally, the contract curve refers to the set of points where the two actors' indifference curves are tangent, that is, the set of trade barriers that both actors mutually prefer to any other set of barriers. Note, however, that movements along

¹⁰ This is why comparative statics are less helpful. They ask what happens when the executive moves slightly closer to the legislature, or vice versa, and thus examine how divisions in government matter for trade policy. We believe that research on how democracies vary among themselves is vitally important, but we leave for another study an exploration of such intrademocracy divisions. See Lohmann and O'Halloran 1994, Milner and Rosendorff 1996, and O'Halloran 1994 for the effects of changes in the degree of divided government on trade policy.



the contract curve cannot make one actor better off without making the other worse off.

In Figure 1, two autocracies must agree on mutual concessions about trade barriers. Home will offer a mutual reduction of barriers to point a on the contract curve; this is the point on the Pareto frontier that leaves foreign indifferent between accepting and rejecting, thus allowing home to earn all the surplus. The level of bilateral trade barriers under TILI is measured by the sum of the home and foreign trade barriers. These aggregate barriers are $0t_a + 0t_a^*$.

Figure 2 presents the results of bargaining within mixed pairs, while varying the relative preferences of the autocrat and the democratic executive. In Figure 2A, a democracy seeks to find mutually acceptable trade concessions with a protectionist autocrat. The democratic executive, P, has freer trade inclinations than the autocratic leader, A^* . P must offer a set of trade barriers inside the area between the two indifference curves, I_C and I_{A^*} , in order for C and A^* to accept it. In addition, P will offer a point on the contract curve between t_P and $t_{A^*}^*$. Although P optimally wants to choose her ideal point, t_P , she is constrained by A^* . Thus, P must choose the point at which A^* 's indifference curve through the no-agreement point intersects the Pareto frontier, that is, point m.

In Figure 2B, a moderate autocrat seeks mutual trade concessions with a democracy. Now P's ideal point is in the win set. Naturally, P chooses this as the offer $(t_P, 0)$, which both the foreign autocrat and the democratic home legislature accept.

In Figure 2C, a liberal autocrat attempts to find mutual trade concessions with a democracy. The democratic executive is much more protectionist than the autocrat. The outcome in this case also must lie within the win set created by the legislature's indifference curve through the Nash trade war equilibrium, I_C , and the autocrat's indifference curve through this point,

 I_{A^*} . The legislature exercises the major constraint here. The democratic executive and the autocrat will choose a point as close as possible to their ideal points that is still acceptable to the legislature; that is, one lying along the legislature's indifference curve. Hence, their choice is bounded by the legislature's preference in this case. Since P makes the offer, P chooses $\mu_l = t_C - t_{A^*}^*$, which is accepted by A^* as well as by $C.^{11}$

In Figure 3, two democracies with symmetric preferences try to find mutually acceptable trade concessions. The executives must choose a set of trade barriers within the win set formed by the indifference curves of the median legislators through the Nash equilibrium, that is, between I_C and I_{C^*} . Again, the contract curve between the two executives inside the win set is the line segment d^*d . P chooses the point on that line segment closest to her ideal point, t_P . The offer is at point d, which is accepted by the foreign executive and both legislatures.

Levels of Trade Barriers and Regime Type

Can we compare the levels of trade barriers across the three types of regime pairs?

Proposition: Aggregate trade barriers are lower within democratic pairs than within pairs composed of an autocracy and a democracy.

A complete proof for both bargaining structures is given in the Appendix (propositions 1 and 2). Although we measure the aggregate barriers by the sum of the two countries' trade barriers in the proofs, intuitive use can be made of the length of the ray from the origin to the agreement point. Figure 4 shows all the possible outcomes in a single diagram. When home makes the offer, the bilateral trade barrier level in the democratic pair is measured by the length of the ray 0d. In the case of the protectionist autocracy in the mixed pair, the ray has length 0m; in the moderate case, the length is $0t_P$; in the liberal case, the ray has length $0\mu_l$. In all these cases, the ray corresponding to the democratic pair (0d) is smaller than that corresponding to the mixed pair $(0m, 0t_P, \text{ or } 0\mu_l)$.

A democracy lowers its trade barriers more when it seeks mutually acceptable concessions with another democracy than when it deals with an autocracy, no matter what the relative preferences of the two leaders. Why? When both are democracies and home makes the initial proposal, it is the pressure of the foreign legislature that forces the home country to compromise or else end up in a trade war. The Nash equilibrium is worse for both countries in the case of two democracies (N_{DD^*}) than in the autocracy-democracy case (N_{DA^*}) , which induces the democratic executives to agree to liberalize trade more than otherwise. The threat of a legislative veto in both countries moves the executives

¹¹ Recall that P is required to offer to A^* a point on their Pareto frontier; in Figure 2C, no point on the line $t_A^* d_P$ is ratifiable. In this case, P offers μ_b , which is ratifiable.

¹² As Axelrod (1970, 56) observes, a worsening of the no-agreement point can induce greater cooperation between the actors under certain circumstances.

0

to a freer trade equilibrium than in the case of a mixed pair. Paradoxically, then, a protectionist legislature forces democracies to lower their trade barriers more than otherwise.

Home

In contrast, whether such barriers will be higher within democratic pairs than within autocratic pairs depends on the preferences of the leaders in the different regimes. When autocrats are more (less) protectionist than democratic executives, mutual reductions in trade barriers will be smaller (greater) between autocracies than between democracies (proposition 3 in the Appendix). Institutional differences alone do not distinguish between the outcomes; the structure of preferences is also important. The same is true for mixed pairs relative to autocratic pairs. When the preferences of the autocrats are protectionist relative to those of the legislature in a democracy, the pair of autocracies will be unable to lower barriers as much as the mixed pair (proposition 4 in the Appendix). Because the two autocrats do not have to contend with a legislature that is more protectionist than they are, they do not have to compromise as much. This reinforces our counterintuitive claim that a protectionist legislature may actually promote freer trade among

In the Appendix we also show what happens when the foreign country is able to make the first offer. On the whole, the results reported above continue to hold. As the first mover, however, the foreign government has greater bargaining power. In each case, the equilibrium outcome now lies closer to the foreign government's ideal point. In Figure 1, for instance, the outcome is a^* instead of a. When A^* proposes, she knows that A will accept any point on the contract curve at or below the intersection of her indifference curve through the Nash trade war equilibrium, I_A . Thus, A^* proposes the set of trade barriers closest to her ideal point that A will also accept, that is, point a^* . In Figure 2A, the outcome is m^* ; in Figure 2B, it is also m^* . In Figure 2C, the outcome is now λ , since this is

the point at which the highest indifference curve of the foreign autocrat, I_{A^*} , is tangent to the home legislature's indifference curve. ¹³ In the case of two democracies, as in Figure 3, P^* proposes d^* , which both the home executive and the two legislatures accept. In effect, changing the proposer moves the outcome along the contract curve closer to the proposer's ideal point, which improves the outcome for the proposer at the expense of the other country. These outcomes, displayed in Figure 4, show that the ray $0d^*$ (aggregate barriers under joint democracy) is shorter than the rays $0m^*$ (aggregate barriers under the mixed case with a protectionist or a moderate autocrat) and 0λ (aggregate barriers in the mixed case with a liberal autocrat).

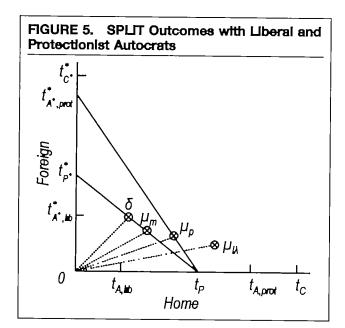
As pointed out above, the equilibrium outcome of any game in which the two countries divide the concessions equally (which we call the SPLIT game) is always in the middle of the range of the contract curve bounded by the maximum value that each can get when she proposes. In the case of two democracies (Figure 3), the SPLIT outcome is in the middle of the portion of the contract curve bounded by d^* and d, labeled 8. The outcomes in the other cases under SPLIT are labeled α , μ_p , μ_m , and μ_h in figures 1, 2A, 2B, and 2C, respectively. Inspection of the lengths of the rays in Figure 5 establishes that $08 < \min\{0\mu_p, 0\mu_m, 0\mu_h\}$.

In sum, regime type is likely to exert an important influence on trade policy. Our model predicts that pairs of democracies should trade more freely than mixed pairs, a result that arises from the tendency for a protectionist legislature to enhance the prospects of trade barrier reductions. Yet, our model is silent on whether commerce will be more open within autocratic pairs than within either democratic or mixed pairs, an issue that hinges on the trade policy preferences of decision makers.

Our model is also silent on the effects of variations within regime types. It does not yield predictions about how institutional differences either among autocracies or among democracies (e.g., whether the government is presidential or parliamentary) affect trade policy, issues that have been addressed elsewhere (e.g., Mansfield and Busch 1995; Milner 1997; Milner and Rosendorff 1996, 1997; Pahre 1997). We focus solely on the institutional differences between autocracies and democracies. Central to our analysis is the assumption that, unlike autocracies, democracies are marked by a legislature that can effectively constrain the chief executive and that is more protectionist than the executive.

Following lemma 2 in the Appendix, it is clear that, given our symmetry assumption in the case of two democracies, aggregate barriers are t_P irrespective of the location of t_C . Hence, it is a direct consequence of lemma 2 that variations in the degree of division between the executive and the legislature in the democracies will have no effect on the outcome when two democracies bargain. Therefore, our claim that democratic dyads prefer lower trade barriers than mixed

¹³ As in note 11, no point on the Pareto frontier between the ideal points of A^* and P is ratifiable in Figure 2C. In this instance, A^* offers λ , which is acceptable to both C and P.



pairs is preserved, even if the legislatures in the democratic dyad case become more protectionist. Put more formally as a corollary to lemma 2 and proved in the Appendix:

COROLLARY. Irrespective of which country makes the first offer, as the legislatures become more protectionist, the aggregate level of barriers on which a pair of democracies agree is unchanged.

EMPIRICAL ANALYSIS

We now turn to an empirical analysis of the hypotheses derived from the model. Our central hypothesis is that trade barriers should be lower between two democracies than between an autocracy and a democracy. It is not possible to test this proposition directly, since reliable data on bilateral trade barriers are not available for many of the countries analyzed here, but data on bilateral trade flows are readily available for most countries and can serve as a proxy for trade policy. Various studies have found that, other things being equal, there is an inverse relationship between the height of trade barriers and the volume of interstate trade (e.g., Harrigan 1993; Leamer 1988; Rodrik 1994, 72; Trefler 1993). We therefore test the propositions stemming from our model by analyzing the effects of regime type on bilateral trade flows and controlling for various other known influences on interstate commerce.

The Statistical Model

To this end, we begin with a gravity model of bilateral trade, which predicts the amount of commerce that should occur between two countries in the absence of trade barriers. It includes the national income and population of both trading partners, as well as the geographic distance between them. Existing research indicates that the gravity framework—which can be derived from a broad class of international trade models—is quite successful in explaining the flow of

interstate commerce (Anderson 1979; Bergstrand 1985, 1989; Deardorff 1998; Eichengreen and Irwin 1995; Feenstra, Markusen, and Rose 1998; Frankel 1993; Frankel, Stein, and Wei 1995; Gowa 1994; Helpman and Krugman 1985; Linnemann 1966; Mansfield and Bronson 1997; Pollins 1989). We extend the gravity model in the following way:

$$\log(X_{q}) = \log \beta_{0} + \beta_{1}\log(GDP_{i} \times GDP_{j})$$

$$+ \beta_{2}\log(POP_{i} \times POP_{j}) + \beta_{3}\log(DIST_{q})$$

$$+ \beta_{4}MIXED_{ij} + \beta_{5}AUT_{ij} + \beta_{6}OTHER_{ij} + \beta_{7}ALLY_{ij}$$

$$+ \beta_{8}PTA_{q} + \beta_{9}MP_{q} + \beta_{10}(ALLY_{ij} \times PTA_{q})$$

$$+ \beta_{11}(ALLY_{ij} \times MP_{q}) + \beta_{12}(PTA_{ij} \times MP_{q})$$

$$+ \beta_{13}GATT_{q} + \beta_{14}COL_{ij} + \beta_{15}COM_{ij} + \beta_{16}WAR_{q}$$

$$+ \beta_{17}\operatorname{lagged} \log(X_{ij}) + \log z_{ij}. \tag{2}$$

In equation 2, X_{ij} is the value of exports from state i to state j in year t. GDP_t and GDP_t are these states' respective gross domestic products in year t-1, POP_t and POP, are their respective populations in t-1, and $DIST_{ij}$ is the geographical distance between them. 14 We take the natural logarithm of each variable because the gravity model's underlying functional form is multiplicative (Anderson 1979; Bergstrand 1985, 1989; Deardorff 1998). Entering the trading partners' national incomes and populations (or, equivalently, their per-capita incomes) in product form is common and accords with theories of international trade based on models of imperfect competition (Eichengreen and Irwin 1995; Frankel 1993; Frankel, Stein, and Wei 1995; Helpman and Krugman 1985).15 These theories indicate that β_1 should be positive and that β_2 and β_3 should be negative.

Ideally, we would use a measure of regime type that directly captures the extent to which countries have a legislature that can constrain the chief executive and affect trade policy choices. Because such a measure does not exist, we rely on a well-known index of regime type developed by Gurr and his colleagues (Jaggers and Gurr 1995; Gurr, Jaggers, and Moore 1989). This measure, which has been used repeatedly in studies of international relations (e.g., Farber and Gowa 1995; Mansfield and Snyder 1995; Oneal and Rusett 1997; Russett 1993), emphasizes five institutional factors that

¹⁴ Per-capita moome is sometimes included in gravity models instead of national population, but doing so yields equivalent results Data on exports are taken from the International Monetary Fund's Direction of Trade (various years); those on GDP and population are taken from the Mark 5 6 version of the Penn World Table (Summers and Heston 1991) and from Maddison 1995. All data on GDP and exports are expressed in U.S. dollars and in real terms; they are deflated using the U.S. wholesale price index and producer price index. Distance is measured based on the closest ports between i and j or, if the countries are landlocked, the shortest land or rail distance between them. These data are taken from the Defense Mapping Agency 1985, Fitzpatrick and Modlin 1986, Times Atlas of the World, and Times Concise Atlas of the World.

¹⁵ The effects of regime type on trade reported below do not depend on whether we introduce the GDPs and populations of the trading partners in product form.

distinguish democracies from autocracies: (1) the competitiveness of the process through which a country's chief executive is selected, (2) the openness of that process, (3) the extent to which there are institutional constraints on a chief executive's decision-making authority, (4) the competitiveness of political participation within a country, and (5) the degree to which binding rules govern political participation within it.

One element of this measure is directly related to our central argument: States are coded as increasingly democratic as the chief executive faces greater institutional constraints, including those emanating from the legislature. But its other components are important as well. If political leaders are not elected competitively, then the legislature will be in a much weaker position vis-à-vis the executive. As Shumpeter (1942) observed, unless leaders can lose office, democratic constraints on their behavior-including those imposed by the legislature—are likely to be ineffective. The competitiveness of elections and political participation, then, are telling indicators of the extent to which institutions such as legislatures can constrain the head of state. Since Gurr's data do not directly tap whether the chief executive needs legislative ratification to enact policies, the following analysis provides only an indirect test of our model. We are unaware, however, of any data that more directly measure legislative control over the head of state. Moreover, this index seems appropriate for distinguishing between regimes with no legislature or an ineffective one and regimes with a popularly elected legislature that can constrain the executive.

To measure regime type, Jaggers and Gurr (1995) create an 11-point measure of a state's democratic characteristics (DEMOC) and an 11-point measure of its autocratic characteristics (AUTOC). They then derive a variable, REG = DEMOC - AUTOC, which takes on values ranging from -10 to 10. Initially, we define countries for which $REG \ge 6$ as coherent democracies, those for which $REG \le -6$ as coherent autocracies, and all remaining regimes as incoherent. After generating an initial set of estimates, we also assess the robustness of our results to this coding procedure. Relying on these criteria, we code each state in the sample as democratic, autocratic, or incoherent in year t-1.

In equation 2, $MIXED_{ij}$ is a dummy variable that equals one if i and j include a democracy and an autocracy, zero otherwise; AUT_{ij} equals one if both i and j are autocracies, zero otherwise; and $OTHER_{ij}$ equals one if either i or j is an incoherent polity, zero otherwise. The reference category is a pair of democratic countries. Since our model indicates that greater trade liberalization should occur between democratic dyads than between mixed pairs, we expect β_4 to be negative. Our model, however, yields no determinate prediction about the sign of either β_5 or β_6 .

Besides addressing the influence of national income, population, distance, and regime type, it is also important to take account of variables that might be respon-

sible for any observed relationship between regime type and bilateral trade. As we explain in more detail below, our analysis centers on the period after World War II. This era was marked by the existence of two major blocs, one made up of autocracies (the Warsaw Pact) and the other composed almost entirely of democracies (the North Atlantic Treaty Organization). To ensure that our results do not simply reflect Cold War rivalries, we include an array of international political variables in equation 2. For instance, there is evidence that democracies were especially likely to ally with one another during the Cold War (Siverson and Emmons 1991). Furthermore, various studies have found that alliances promote trade, which suggests that alliances may account for any observed effects of regime type on trade flows (Gowa 1994; Mansfield and Bronson 1997). In the same vein, recent research indicates that, since World War II, pairs of democracies have been much more likely to establish preferential trading arrangements (PTAs) than other pairs (Mansfield, Milner, and Rosendorff 1998); and it is widely recognized that such arrangements tend to promote the flow of commerce between member states (Frankel 1993; Frankel, Stein, and Wei 1995; Gowa 1994; Linnemann 1966; Mansfield and Bronson 1997; Pollins 1989). To control for the effects of alliances and PTAs, we include $ALLY_{ij}$ and PTA_{ij} . The former equals one if i and j are allied in year t-1, zero otherwise. The latter equals one if i and j belong to the same PTA in t-1, zero otherwise.

Previous studies also have found that states trade more extensively if they are members of the General Agreement on Tariffs and Trade (GATT), if they include a major power, if they have command economies, if they are not involved in a war, and if they had a colonial relationship (Gowa 1994; Kleiman 1976; Mansfield and Bronson 1997; Pollins 1989). As such, we include the following dummy variables, each of which is measured in t-1: MP_{ij} equals one if either i or j is a major power, zero if neither state is a major power; $GATT_{ij}$ equals one if both i and j are parties to the GATT, zero otherwise; COL_{ij} equals one if i and j had a colonial relationship that concluded in or before t-1 (and after World War II's onset), zero otherwise; COM_{ii} equals one if both i and j have command economies, zero if they do not; and WAR_{ij} equals one if i and j are at war, zero if they are not. 17 By including all these variables, we control for the most likely international political influences on trade relations during the Cold War.

Earlier research indicates that states belonging to

¹⁶ To compute these measures, we rely on the coding rules set forth m Gurr, Jaggers, and Moore 1989.

¹⁷ Data on alliances are taken from Small and Singer 1969 and from updates of their list provided by the Correlates of War Project and dated March 25, 1993. Data on PTAs are taken from de Melo and Panagariya 1993, Hartland-Thunberg 1980, Pomfret 1988, and the World Trade Organization 1995. See Singer and Small (1994) for data on major powers and wars, selected usues of the GATT's International Trade and Basic Instruments and Selected Documents for data on GATT membership, Kornai (1992, 6–7) for data on command economies from 1960 to 1985, Staar et al. (1991) for these data in 1990, and Kurian (1992) for data on colonial relations. See Mansfield and Bronson (1997, esp. 105–6) for more on all the data used in this study, except those on regime type.

the same alliance and the same PTA conduct more trade than countries that are either allies or PTA members, but not both. Furthermore, there is evidence that allies and PTA members trade more extensively if at least one state is a major power (Mansfield and Bronson 1997). We thus introduce $ALLY_{ij} \times PTA_{ij}$, $ALLY_{ij} \times MP_{ij}$, and $PTA_{ij} \times MP_{ij}$ in equation 2. To account for any temporal dependence in bilateral trade flows, we include an instrument for the lagged value of log X_{ij} . We also account for any country-specific or year-specific effects on trade by including dummy variables for all but one country and for all but one year analyzed here. Finally, $\log z_{ij}$ is a stochastic error term.

Our sample consists of all pairs of states listed as members of the interstate system by the Correlates of War Project (Singer and Small 1994) in 1960, 1965, 1970, 1975, 1980, 1985, or 1990 for which complete data are available. The independent variables in our model are measured in these years (t-1), and the value of exports from i to j is measured one year hence (in years t, which are 1961, ..., 1991) to minimize any potential problems of simultaneity. We do not analyze years before 1960 because data on GDP and exports are especially limited for many autocracies until that point. Including the available data for these years could bias our results by systematically underrepresenting dyads that are not jointly democratic. After pooling these data, ordinary least squares was used to generate estimates of the parameters in equation 2. Tests of statistical significance are based on White (1980) heteroskedasticity-consistent standard errors.19

Estimates of the Parameters

Our initial results, which are reported in the first column of Table 1, accord with the central proposition advanced earlier. The estimate of $MIXED_{ij}$ is negative and statistically significant, which indicates that trade flows between democratic states tend to be greater than trade flows between democracies and autocracies. Moreover, the predicted difference in trade between democratic pairs and mixed pairs is relatively large: On average, a democracy and an autocracy engage in roughly 15% to 20% less commerce than a dyad composed of two democracies.²⁰

As noted earlier, our model does not yield determinate predictions about whether trade will be more open within autocratic pairs than within either demo-

open within autocratic pairs than within either demo
18 We use an instrument for the lagged value of $\log X_{ij}$, rather than its observed value, because the natural logarithm of the lagged value of exports is likely to be correlated with the error term in equation 2, which would yield inconsistent estimates. This instrument is generated by regressing the lagged value of the natural logarithm of exports on the lagged values of the natural logarithms of GDP_{ij} , GDP_{ij} , POP_{ij} , and POP_{ij} , as well as a dummy variable for each year but one in the sample. Note that we use data on exports from 1956 and data on GDP and population from 1955 to derive estimates of the

lagged value of $\log X_v$ for 1961. ¹⁹ All of the statistical analyses in this article are conducted using STATA version 5.0.

²⁰ More precisely, the estimated change in the predicted volume of trade for a mixed pair compared to a democratic pair is $e^{\beta 4} - 1$. From column 1 of Table 1, $\beta_4 = -0.188$; and $e^{-0.188} - 1 = -0.17$.

cratic or mixed pairs. The results in the first column of Table 1 provide little evidence that the flow of commerce between autocracies differs markedly from the flow between democracies, since the estimate of AUT_{ij} is not statistically significant. Yet, autocratic pairs trade much more extensively than mixed pairs. The estimate of AUT_{ij} is considerably larger than the estimate of $MIXED_{ij}$, and the difference between them is significant (t = 5.79). Finally, the estimate of $OTHER_{ij}$ is negative. Pairs that include an incoherent polity conduct significantly less trade than democratic or autocratic dyads, although they engage in significantly more commerce than mixed pairs.²¹

To assess the robustness of these results, we conduct a number of additional tests. First, our model emphasizes how domestic legislatures constrain the trade policy choices of executives in democracies. But in fledgling democracies, legislative institutions are often weak and poorly developed (e.g., Haggard and Kaufman 1995; Haggard and Webb 1994). Hence, trade policy between mature democracies may differ from that between countries that include a nascent democracy with a weakly institutionalized legislature. To address this issue, we include $DEMZ_{ij}$ in equation 2. It equals one if either i or j experienced a democratic transition between years t - 6 and t - 1 (i.e., if either state is not a coherent democracy in t-6 and if both states are democratic in t-1), zero otherwise. For present purposes, AUT_{ij} , $MIXED_{ij}$, and $OTHER_{ij}$ are set equal to zero if $DEMZ_{ij}$ equals one.

As shown in the second column of Table 1, pairs with a fledgling democracy conduct significantly less trade than pairs composed of two mature democracies, since the estimate of $DEMZ_{ij}$ is negative and statistically significant. Furthermore, when $DEMZ_{ij}$ is included in the analysis, there is even stronger evidence that democratic pairs trade more extensively than mixed pairs. The estimate of $MIXED_{ij}$ continues to be statistically significant, and its absolute value is noticeably larger than before.

Second, we address the robustness of the preceding results with respect to the coding of democratic and autocratic regimes. Thus far, we have coded states as democratic if $REG \ge 6$, and we have coded them as autocratic if $REG \le -6$. These operational definitions have been used in other studies, but they are clearly somewhat arbitrary, and it is important to determine if relaxing them affects our findings. We therefore estimate equation 2 after redefining these thresholds as: (a) 5 and -5, (b) 4 and -4, (c) 3 and -3, (d) 2 and -2, and (e) 1 and -1.

Table 2 shows the estimates of $MIXED_{ij}$, AUT_{ij} , and $OTHER_{ij}$ for each set of thresholds. (The estimates of the remaining parameters are not presented to conserve space.) These results provide a further indication that our earlier findings are quite robust. The estimate of $MIXED_{ij}$ is negative and statistically significant

²¹ Note that, like the difference between democratic pairs and dyads with an incoherent polity shown in Table 1, the differences between $OTHER_{\eta}$, on the one hand, and both $MIXED_{\eta}$ and AUT_{η} , on the other, are statistically significant (t=3.07 and 3.52, respectively).

TABLE 1. Regression of Trade on GDP, Population, Distance, Regime Type, Alliances, Preferential Trading Arrangements, Major Power, GATT, Prior Colonial Ties, Command Economies, and War, 1960–90, Using Different Measures of Regime Type

Economies, and war,			Regime Type	
	Jaggers and		Alvarez et	al. (1996)
Varlable	(1)	(1A)	(2)	(2A)
$\log \beta_0$	17.274***	17.688***	22.550***	23.263***
	(3.058)	(3.057)	(3.166)	(3.175)
$\log(GDP_i \times GDP_j)$.	.512***	.512***	.580***	.582***
	(.039)	(.039)	(.044)	(.044)
$\log(POP_i \times POP_j)$	937***	943***	-1.211***	1.232***
	(.080)	(.080)	(.083)	(.084)
log(DIST _{ij})	759***	−.758 ***	−.778 ***	−.7 77***
	(.014)	(.014)	(.014)	(.014)
MIXED _{IJ}	188***	233***	111***	134***
	(.035)	(.039)	(.025)	(.027)
AUT,	.098	.036	053	075
	(.065)	(.0 6 9)	(.051)	(.052)
OTHER _{IJ}	−.088* (.039)	141*** (.043)	_	_
DEMZ _{ij}	_	142** (.053)	_	−.120** (.043)
$ALLY_{ij}$.119*	.115*	.184***	.180***
	(.052)	(.052)	(.051)	(.051)
PTA,,	.527***	.521***	.473***	.470***
	(.039)	(.039)	(.040)	(.040)
MP _{ij}	.548***	.548***	.618***	.620***
	(.136)	(.135)	(.136)	(.137)
$ALLY_{ij} \times PTA_{ij}$.535***	.537***	.618***	.620***
	(.0 6 6)	(.067)	(.066)	(.066)
$ALLY_{ij} \times MP_{ij}$.179**	.182**	.052	.050
	(.068)	(.068)	(.067)	(.067)
$PTA_{ij} \times MP_{ij}$	−.47 6***	483***	518***	522 ***
	(.068)	(.068)	(.068)	(.068)
GATT _{IJ}	.074	.072	.126**	.125 **
	(.038)	(.038)	(.040)	(.040)
COL,	1.682***	1.684***	1.780***	1.787***
	(.085)	(.085)	(.087)	(.087)
COM _u	1.033***	1.031***	.855***	.847***
	(.0 9 5)	(.095)	(.117)	(.117)
WAR _u	-6.463***	-6.447***	-6.556***	-6.562***
	(.107)	(.107)	(.110)	(.110)
lagged log (X _{ij})	.855***	.855***	. 94 6***	.946***
	(.014)	(.014)	(.014)	(.014)
Ř²	.53	.53	.55	.55
N	33,116	33,116	30,480	30,480

Note. Entries are unstandardized regression coefficients. Figures in parentheses are White heteroskedasticity-consistent standard errors. One-tailed tests are conducted for the regression coefficient of $MIXED_{ij}$, since its sign is specified by the model. Two-tailed tests are conducted for the remaining coefficients. Regressions include dummy variables for country-specific and year-specific fixed effects. * $p \le .05$, ** $p \le .01$, *** $p \le .001$.

regardless of which set of thresholds is used, and its size does not vary much. Furthermore, the estimate of AUT_{ij} continues to be positive, and there is only one

case in which it is statistically significant. The estimate of $OTHER_{ij}$ continues to be negative, and it is significant in two instances.

TABLE 2. Estimated Effects of Regime Type on Trade, Based on Different Operational Definitions of Democracy and Autocracy, 1960-90

Operational Definition of			Regression Coefficient	
Democracy	Autocracy	MIXED,,	AUT,,	OTHER,,
REG ≥ 5	REG ≤ -5	171*** (.034)	.109 (.064)	110** (.043)
REG ≥ 4	REG ≤4	184*** (.034)	.100 (.063)	−.119** (.046)
REG ≥ 3	<i>RE</i> G ≤ −3	141*** (.034)	.128* (.063)	060 (.053)
REG ≥ 2	<i>RE</i> G ≤ −2	138*** (.034)	.093 (.063)	047 (.058)
REG ≥ 1	<i>RE</i> G ≤ ~1	143*** (.033)	.053 (.062)	193 (.104)

Note. Entries are unstandardized regression coefficients based on equation 2 and using the Jaggers and Gurr (1995) measure of regime type. Figures in parentheses are White heteroskedasticity-consistent standard errors. One-tailed tests are conducted for the regression coefficient of $MDED_p$, since its sign is specified by the model. Two-tailed tests are conducted for the remaining coefficients. Regressions include dummy variables for country-specific and year-specific fixed effects. * $p \le .05$, ** $p \le .05$, ** $p \le .01$, *** $p \le .001$.

Besides altering the thresholds that distinguish between regime types when the Jaggers and Gurr (1995) measure (REG) is used, we examined whether our results differ if another measure of regime type is used. Various alternative measures exist, but very few cover the range of countries and years analyzed here.22 Alvarez et al. (1996), however, have coded the regime type of most countries in our sample on an annual basis during the period from 1950 to 1990. Unlike Jaggers and Gurr, Alvarez et al. identify each state as either democratic or autocratic in every year. Relying on their measure has certain advantages, since we do not need to establish quantitative thresholds to code regime type. From our standpoint, however, this measure also has certain drawbacks, especially Alvarez et al.'s (1996, 5) intentionally narrow definition of democracy, which centers solely on the competitiveness of the political process. In their view, democracy exists when there are competitive elections for the head of state and the legislature; otherwise, the regime is autocratic. As noted above, the competitiveness of the political process helps shape whether the legislature can effectively constrain the chief executive. In contrast to Jaggers and Gurr, however, Alvarez et al. do not directly measure our key variable of interest: the legislature's role in constraining the executive.

Nonetheless, it is useful to analyze the Alvarez et al. data in order to assess the robustness of our initial results. To this end, we reestimated equation 2 after using their data to code $MIXED_{ij}$, AUT_{ij} , and $DEMZ_{ij}$. Note that $OTHER_{ij}$ is not included in this analysis, since Alvarez et al. code each state as either a democracy or an autocracy. The results are presented in the third column (omitting $DEMZ_{ij}$) and the fourth column (including $DEMZ_{ij}$) of Table 1. Clearly, there is considerable agreement between these findings and those generated using the Jaggers and Gurr data.

Particularly important for present purposes is that the estimate of MIXED, continues to be negative and statistically significant, although its absolute value is somewhat smaller when we rely on the Alvarez et al. data rather than the Jaggers and Gurr data. Furthermore, we again find that democratic transitions dampen trade, since the estimate of $DEMZ_{i}$, is negative and significant. It is also noteworthy that no statistically significant difference exists between the flow of trade within autocratic pairs and democratic pairs. As in the first two columns in Table 1, the estimate of AUT_{II} is not significant, although it is negative when the Alvarez et al. data are used, and it is positive when the Jaggers and Gurr data are employed. In sum, our results are quite robust with respect to the coding of regime type.23

Third, it is useful to ensure that our statistical design does not mask any substantial intertemporal variation in the effects of regime type on trade. In a preliminary effort to address this issue, we conducted separate analyses of each decade in our sample; we pooled the observations for 1960 and 1965, for 1970 and 1975, and for 1980 and 1985, and we analyzed 1990 alone. For each set of observations, we estimated equation 2 using the same operational measure of regime type that was used to generate our original results (i.e., states are coded as democratic if $REG \ge 6$, autocratic if $REG \le -6$). Table 3 presents the estimated coefficients of $MIXED_{ij}$, AUT_{ij} , and $OTHER_{ij}$. (As in Table 2, the remaining coefficient estimates are not reported to

²² For example, the Political Regime Change data set developed by Gasiorowski (1996) is restricted to 97 less developed countries, a much different sample of countries than ours.

²³ Note that we have included country-specific fixed effects in all the preceding analyses, but we also analyzed whether replacing them with pair-specific fixed effects has any bearing on our results. To this end, we reestimated the models shown in the second and fourth columns of Table 1 after accounting for pair-specific fixed effects and found that doing so has little influence on the estimates.

²⁴ To estimate equation 2 for the 1960s, the 1970s, and the 1980s, a dummy variable for the first year in each decade (1960, 1970, and 1980, respectively) is included. Note that the following results are quite similar if the Alvarez et al. (1996) data on regime type are used instead of the Jaggers and Gurr data.

TABLE 3. Estimated Effects of Regime Type on Trade by Decade, 1960–90

	•				
-	Regression Coefficient				
Years	MIXED _{II}	AUT_u	OTHER,,		
1960 and 1965	191*	.182	.029		
	(.083)	(.156)	(.117)		
1970 and 1975	.015	.147	.147		
	(.083)	(.149)	(.119)		
1980 and 1985	- <u>.2</u> 94***	036	.00 6		
	(.081)	(.153)	(.119)		
1990	−.494 ***	547*	274		
	(.126)	(.256)	(.155)		

Note Entries are unstandardized regression coefficients based on equation 2 and using the Jaggers and Guirr (1995) measure of regime type. Figures in parentheees are White heteroekedasticity-consistent standard errors. One-tailed tests are conducted for the regression coefficient of $MIXED_g$, since its sign is specified by the model. Two-tailed tests are conducted for the remaining coefficients. Regressions include dummy variables for country-specific and (in the first three regressions shown above, year-specific) fixed effects " $p \le .05$, **** $p \le .001$.

conserve space.) In three of the four decades we assess, the estimate of $MIXED_{ij}$ is negative and statistically significant, which indicates that the tendency for trade to be more open within democratic pairs than within mixed pairs is relatively stable over time. Also, it is interesting that any tendency for autocratic pairs to trade more extensively than their democratic counterparts has become attenuated over time, an issue that warrants attention in future research.

Our sample includes all countries for which we were able to obtain data for the variables in equation 2. For various countries, however, data on GDP, trade, or both are missing in certain years analyzed here. Since a number of these are autocracies that are unlikely to trade extensively due to their severely distorted economies (e.g., Albania, Cambodia, Cuba, Laos, Libya, North Korea, and Sudan), we ran a series of Heckman selection models to ensure that there was no bias in our earlier findings (Greene 1993, 706-14). We found no evidence of such a bias.²⁵ In addition to the countries in our sample with missing data for certain years, there are other countries for which we were unable to obtain any economic data. These (e.g., Afghanistan, Bangladesh, Burundi, Liberia, Rwanda, Somalia, Uganda, Yemen) were excluded from our sample altogether. Virtually all of them are autocracies with highly distorted economies. Such distortions are likely to hamper commerce, so there is ample reason to expect that our results understate the actual extent to which democratic dyads trade more freely and extensively than mixed dyads. Equally, the omission of these countries probably helps explain why, for example, the estimate of AUT_{ij} is positive in Table 1 when the Jaggers and Gurr measure of regime type is considered.

Finally, we address whether our results are robust

with respect to the inclusion of various factors omitted from equation 2. Of chief importance in this regard are economic growth and the level of economic development. It is possible, for example, that democratic pairs conduct more trade than mixed pairs because democracies experience higher growth than autocracies, and growth promotes trade. Similarly, democracies may be more highly developed economically, and more highly developed states may engage in more trade than less developed countries. Our results do not accord with either of these possibilities; democratic pairs trade more extensively than mixed pairs, but so do autocratic pairs. Nonetheless, as a further test of the robustness of the results, we included in equation 2 the real percapita GDP of both i and j in year t-1. We then replace per-capita GDP with various other factors that have been used as measures of economic development in studies of international trade (e.g., Polachek 1980), including the number of highway vehicles per capita, per-capita school enrollment, per-capita university enrollment, and electrical production per capita. Finally, to examine the effects of growth, we include the change in per-capita GDP of both i and j between year t - 6and year t - 1.26 It should be noted that, for some of these variables, complete data are available for only a fraction of the states and years we analyzed, so the estimates generated after including them in equation 2 are not directly comparable to those in Table 1. But our findings indicate that including these variables has no substantive influence on the estimates in Table 1. Most important for present purposes, the estimate of $MIXED_{ij}$ is negative and statistically significant in each of these analyses.

Turning to the remaining variables in equation 2, the effects of GDP, population, and distance accord with expectations based on the gravity model. As shown in Table 1, the estimate of $\log(GDP_i \times GDP_i)$ is positive and statistically significant, and the estimates of both $\log(POP_i \times POP_i)$ and $\log(DIST_{ij})$ are negative and significant. In addition, the estimates of $ALLY_{ij}$, PTA_{ij} , MP_{ij} , $ALLY_{ij} \times PTA_{ij}$, and $ALLY_{ij} \times MP_{ij}$ are positive; and the estimate of $PTA_{ij} \times MP_{ij}$ is negative. All these estimates are statistically significant when the Jaggers and Gurr data are used, and all but the estimate of $ALLY_{ij} \times MP_{ij}$ is significant based on the Alvarez et al. data. These results indicate that alliances, PTAs, and the presence of a major power each promotes commerce. Moreover, countries that belong to the same alliance and the same PTA conduct more trade than countries that are either allies or parties to the same PTA, but not both. Alliances provide a much greater impetus to trade between states that include a major power than between other states. PTAs yield marginally more commerce between trading partners that include a major power than between other countries.

Our results also indicate that a former colonial relationship heightens bilateral trade flows, that states with command economies trade extensively with each

²⁵ The estimate of ρ (which is the correlation between the error term in the selection equation and the error term in equation 2) is quite small, and we found no evidence that it is significantly different from zero.

²⁶ These data are taken from Banks 1995, Maddison 1995, and Summers and Heston 1991.

other, and that warfare depresses commerce. The estimates of COL_{ij} and COM_{ij} are positive, the estimate of WAR_{ij} is negative, and all these results are statistically significant. GATT membership, however, has a weaker effect on bilateral trade flows. The estimate of $GATT_{ij}$ is positive, which suggests that trade flows between parties to the GATT tend to be somewhat larger than trade flows between nonmember states. But this estimate is relatively small, and it is not statistically significant when the Jaggers and Gurr data on regime type are used.

In sum, the results of this empirical analysis support the central prediction of our formal model. Holding constant a large number of economic and political factors—including variables pertaining to the dynamics of the Cold War—we find that pairs of democracies have conducted considerably more trade than mixed pairs since World War II. Furthermore, although our model generates no definitive predictions about autocratic pairs, it is noteworthy that, based on our sample, no systematic difference exists in trade relations between these pairs and democratic dyads.

CONCLUSION

It is frequently argued that the foreign policies of democracies are distinctive, but few studies have focused on the trade policy choices made by such states. Moreover, most work on trade policy overlooks the effects of regime type. By addressing the effects of regime type on trade policy, this article contributes to the literature on the links between democracy and foreign policy as well as that on the political economy of trade policy.

The model we developed highlights the legislature's role in making trade policy in democracies. Having a legislature that ratifies the chief executive's trade proposals may create a credible threat that allows executives in democracies to arrive at freer trade outcomes than would otherwise occur. The possible veto of a trade deal by one or both legislatures in the dyad may lead the executives to search for lower mutually acceptable levels of trade barriers. This, in turn, may explain why pairs of democracies are better able to lower their trade barriers than mixed pairs.

Our analysis focuses on variations between democracies and autocracies, not on variations within either regime type. Both our model and the data analysis treat each type of regime as homogeneous. Democracies are distinct from autocracies because they have a legislature that exercises ratification power, regardless of its preferences. The differences among regime types rest on institutional features, not on the policy preferences of decision makers. Varying the trade policy preferences of the executive and legislature does not alter the degree of democracy present; it only changes the extent to which government is divided within democracies. Furthermore, whether a government is divided or unified has no bearing on the degree to which a country is democratic. Hence, examining the influence of such divisions (i.e., as the trade policy preferences of the executive and legislature diverge or converge) does not

facilitate comparisons between democracies and autocracies, only between more divided and less divided democracies. Our model is capable of deriving outcomes when such changes occur, but in this article we have neither the space nor the data to explore such variations within democracies. It is interesting to note, however, the finding of others that increasing divisions in the preferences of executives and legislatures may result in more protectionism for a single country and less possibility of trade-barrier-reducing agreements between countries (e.g., Lohmann and O'Halloran 1994; Milner and Rosendorff 1996). Our results differ from but are not inconsistent with those earlier findings. We show that having a protectionist legislature with ratification powers may help democracies arrive at lower levels of trade barriers in their dealings with one

Our theoretical model generates two central predictions. First, aggregate trade barriers will be lower between democracies than between a democracy and an autocracy. Since a trade war between two protectionist legislatures is worse than a trade war involving just one such legislature, two democratic executives will choose significant trade liberalization. Our empirical results accord with this proposition. They indicate that trade between democracies tends to be more extensive than commerce within mixed pairs. On average, dyads composed of a democracy and an autocracy engage in roughly 15% to 20% less commerce than those composed of two democracies. Interestingly, this result seems to strengthen over time. By the 1990s, as Table 3 shows, the average volume of trade between a democracy and an autocracy was roughly 40% less than that of democratic dyads.

Our data overlap the bipolar period extensively. To control for any Cold War influences that might account for the effects of regime type on trade, we analyzed various international political factors. The Cold War was marked by the existence of two camps, with democracies concentrated in one and autocracies in the other. It could be that democratic pairs trade more because they tend to be allies, because they belong to the same commercial institutions (the GATT or PTAs), or because they are not involved in military conflicts. Even when we control for these factors, however, we find that trade flows are significantly greater within democratic dyads than within mixed dyads.

Second, whether the level of aggregate trade barriers will be higher within autocratic pairs than within either democratic or mixed pairs depends on the relative trade preferences of the actors involved. Although it was not possible to incorporate the preferences of political actors directly into our empirical analysis, our results conform with this conclusion. By and large, no significant difference exists between the volume of trade conducted within autocratic pairs and within democratic pairs, which suggests that both domestic institutions and the preferences of decision makers help shape trade policy.

Our model uses a very general institutional characteristic to distinguish democratic and autocratic regimes: the presence or absence of a legislature that can effectively constrain the executive. This simplified approach is appropriate given our purpose, but it may mask variations in trade policy within both democracies (e.g., among presidential and parliamentary systems) and autocracies (e.g., those governed by military leaders as opposed to those governed by hereditary monarchs). Examining the influence of such factors is an important step for future research. It is striking, however, that notwithstanding the institutional distinctions within each regime type, broadly defined differences across regime types exert a noticeable effect on trade relations. Exploring such broad institutional differences across autocracies and democracies, we believe, is an important and prior step toward examining the effect of differences within these two regime types.

Although our analysis hinges on one institutional feature assumed to be common among democracies (i.e., a legislature with the power to ratify executive proposals), it is obvious that many other institutional distinctions can be drawn between autocracies and democracies. These institutional differences may also influence international trade. Our empirical tests pick up the effect of the legislature as well as that of these other differences, so we cannot rule out the possibility that other elements of democracy are important factors. Measures of democracy that isolate the effect of the legislature simply do not exist, however, and given our large, longitudinal sample of countries, they would be prohibitive to construct. Moreover, our evidence and our model reinforce each other, lending credibility to our claims. Abstracting away from preferences and other institutional factors, the presence of popularly elected legislatures that must ratify policies may be an important difference between democracies and other regime types in international trade. Paradoxically, the protectionist threat provided by the legislature in democracies can result in more open trade, rather than less.

APPENDIX

Let the two executives who bargain internationally be identified as i and j.

Lemma 1. The Pareto frontier between i and j is $t^* = -t(t_j^*t_i) + t_j^*$.

Proof: Maximizing $U_i(t, t^*)$ subject to $U_i(t, t^*) \ge U$ and $(t, t^*) \ge 0$ yields the result. Q.E.D.

The TILI Game with Home Offers

In the case of the democratic pair, we require the home executive to offer the foreign executive an agreement that lies on their Pareto frontier (as given by lemma 1).

LEMMA 2. When two democracies negotiate under TILI, the agreement point is $(-1/2\,t_C+1/2\,t_P+1/2\,\sqrt{(t_C^2+2t_Ct_P-t_P^2)},$ $1/2\,t_P+1/2\,t_C-1/2\,\sqrt{(t_C^2+2t_Ct_P-t_P^2)})$, and aggregate barrers are t_P .

Proof: The no-agreement point is $(t_C, t_{C^*}^*)$. Under TILI, with home making the offer, home's problem is max $U_P(t, t^*)$

subject to $U_C(t, t^*) \geq U_C(t_C, t^*_{C^*}), \ U_{C^*}(t, t^*) \geq U_{C^*}(t_C, t^*_{C^*}), \ U_{P^*}(t, t^*) \geq U_{P^*}(t_C, t^*_{C^*}), \ t^* = t^*_{P^*} - t(t^*_{P^*}/t_P) \ \text{and} \ t, \ t^* \geq 0.$ The solution lies at point d in Figure 3, where foreign's indifference curve intersects the Factor fornier. The second constraint binds; we solve $t^* = t^*_{C^*} - \sqrt{t^2_C - t^2}$ and (from lemma 1) $t^* = t^*_{P^*} - t(t^*_{P^*}/t_P)$ for t, t^* . Recalling that we study only the symmetric case $t_P = t^*_{P^*}, t_C = t^*_{C^*}$ and solving, we get the solution above. Q.E.D.

In the case of the mixed pair, the home executive will offer the foreign autocrat a point on the Pareto frontier when such a point is ratifiable. If no such point exists, then P chooses the nearest point to its ideal point that is preferred by both A^* and C to the status quo.

LEMMA 3. When a mixed pair negotiate under TILI, the agreement point is

- 1. $(t_P t_C / \sqrt{t_P^2 + t_{A^*}^{*2}}, t_{A^*}^*, (\sqrt{t_P^2 + t_{A^*}^{*2}} t_C) / \sqrt{t_P^2 + t_{A^*}^{*2}})$, and aggregate barriers are $t_{A^*}^* + t_C (t_P t_{A^*}^*) / \sqrt{t_P^2 + t_{A^*}^{*2}}$ when $t_C^2 t_P^2 < t_{A^*}^{*2}$.
- 2. $(t_p, 0)$ and aggregate barriers are t_p when $t_C^2 t_p^2 \ge t_{A^*}^{*2} \ge (t_C t_p)^2$.
- $(t_C t_P)^2$. 3. $(t_C - t_A^*, 0)$ and aggregate barriers are $t_C - t_A^*$ when $t_A^{**} < (t_C - t_P)^2$.

Proof: The no-agreement point is $(t_C, t_{A^*}^*)$. Under TILI, with home, the democracy, making the offer, home's problem is max $U_P(t, t^*)$ subject to $U_C(t, t^*) \ge U_C(t_C, t_{A^*}^*)$, $U_{A^*}(t, t^*) \ge U_{A^*}(t_C, t_{A^*}^*)$, and $t, t^* \ge 0$.

At point m in Figure 2A, the foreign indifference curve binds, as does the constraint that $t^* \ge 0$. We solve $t^* = t_{A^*}^* - \sqrt{t_C^2 - t^2}$ and (from lemma 1) $t^* = t_{A^*}^* - t(t_{A^*}^*/t_P)$ for t, t^* , and the condition that $t^* \ge 0$ is $t_C^2 - t_P^2 < t_{A^*}^*$. Where $t_C^2 - t_P^2 \ge t_{A^*}^* \ge (t_C - t_P)^2$ neither constraint binds, and the solution is at the point $(t_P, 0)$. When $t_{A^*}^{*2} < (t_C - t_P)^2$, the domestic legislature's constraint binds, leading to the solution at point $(t_C - t_{A^*}^*, 0)$.

We assume that there is a meaningful difference between the ideal points of C and P (and C^* and P^*).

Assumption. $t_C > 2t_P$ and $t_{C^*}^* > 2t_{P^*}^*$.

PROPOSITION 1. Under TILI (when home makes the offers), the mixed dyad has at least as high aggregate barriers as does the democratic dyad.

Proof: Let B^D , B^M , and B^A denote the aggregate barriers for the democratic dyad, the mixed dyad, and the autocratic dyad, respectively. Then $B^D = t_P$, and

$$B^{M} = \begin{cases} \frac{t_{C}(t_{P} - t_{A^{\bullet}}^{*})}{\sqrt{t_{P}^{2} + t_{A^{\bullet}}^{*2}}} + t_{A^{\bullet}}^{*} & \text{if } t_{C}^{2} - t_{P}^{2} < t_{A^{\bullet}}^{*2} \\ t_{P} & \text{if } t_{C}^{2} - t_{P}^{2} \ge t_{A^{\bullet}}^{*2} \ge (t_{C} - t_{P})^{2} \\ t_{C} - t_{A^{\bullet}}^{*} & \text{if } t_{A^{\bullet}}^{*2} < (t_{C} - t_{P})^{2} \end{cases}$$
(A-1)

Suppose $t_C^2 - t_P^2 < t_{A^*}^{*2}$ which implies $B^D - B^M = (t_P - t_{A^*}^*)((\sqrt{t_P^2 + t_{A^*}^{*2}} - t_C)/\sqrt{t_P^2 + t_{A^*}^{*2}})$. Now $t_C^2 - t_P^2 < t_{A^*}^{*2} \Rightarrow \sqrt{t_P^2 + t_{A^*}^{*2}} - t_C > 0$. By the assumption, $t_C > 2t_P > \sqrt{2t_P} \Rightarrow t_C^2 > 2t_P^2$. Then $t_C^2 - t_P^2 > t_P^2$. Now $t_C^2 - t_P^2 < t_{A^*}^{*2}$, so $t_P < t_{A^*}^{*2}$, and $B^D - B^M < 0$. Suppose that $t_C^2 - t_P^2 \ge t_{A^*}^{*2} \ge (t_C - t_P)^2$. Then $t_A^D - t_A^D = 0$. If $t_A^{*2} < (t_C - t_P)^2$, then $t_A^{*3} < t_C - t_P$ or $t_P < t_C - t_A^*$. Hence, $t_A^D - t_A^D = t_A^D =$

TILI When Foreign Makes the Offer

Once again we compare the democratic dyad case with the mixed case. Home is in both cases the democracy. Foreign in

the first case is a democracy and in the second an autocracy. We now let foreign make the offers. Foreign offers the home executive an agreement; if the home executive agrees, then the offer is put to the legislature for a possible veto.

PROPOSITION 2. Under TILI (when foreign makes the offer), the mixed dyad has higher aggregate barriers as does the demo-

Proof: In the democratic dyad the outcome is at point d^* in figures 3 and 5; $B^D = t_P$. In the mixed dyad the outcome is at m^* in Figure 2A and B and at point λ in Figure 2C.

$$B^{M} = \begin{cases} -[-t_{c}t_{F}^{2} - t_{F}t_{A}^{*2}] \\ + t_{A}^{*}(t_{F} - t_{A}^{*}) \sqrt{2t_{C}t_{F} + t_{A}^{*2} - t_{C}^{2}} & \text{if } t_{A}^{*2} > (t_{C} - t_{F})^{2} \\ - t_{F}^{2}t_{A}^{*} + t_{C}t_{A}^{*} - t_{F}]/(t_{F}^{2} + t_{A}^{*2}) \\ t_{C} + t_{A}^{*} \frac{t_{C} - t_{A}^{*2}}{\sqrt{t_{C}^{2} + t_{A}^{*2}}} & \text{if } t_{A}^{*2} \leq (t_{C} - t_{F})^{2} \end{cases}$$

Suppose $t_{A^*}^2 > (t_C - t_P)^2$. Then $B^D - B^M = (t_P - t_{A^*}^*)(t_P^2 - t_C t_P + t_{A^*}^* \sqrt{2t_C t_P + t_{A^*}^{*2} - t_C^2})/(t_P^2 + t_{A^*}^{*2})$. Now $(t_P^2 - t_C t_P + t_{A^*}^* \sqrt{2t_C t_P + t_{A^*}^{*2} - t_C^2})/(t_P^2 + t_{A^*}^{*2}) > 0$, and $t_{A^*}^2 > (t_C - t_P)^2 = t_C(t_C - 2t_P) + t_P^2 > t_P^2$ by the assumption. Then $t_P < t_{A^*}^*$ and $B^D - B^M < 0$. If $t_{A^*}^{*2} \le (t_C - t_P)^2$ then $t_C - t_P \ge t_{A^*}^* > t_{A^*}^* (t_{A^*}^* - t_C)/\sqrt{t_{A^*}^{*2} + t_C^2}$ since $(t_{A^*}^* - t_C)/\sqrt{t_{A^*}^{*2} + t_C^2} < 1$. Then $t_P < t_C - t_{A^*}^* (t_{A^*}^* - t_C)/\sqrt{t_{A^*}^{*2} + t_C^2}$ and $t_C^D > t_C^D > t_C^D$.

Proposition 3. $B^D \le B^A \Leftrightarrow t_p \le t_{A^*}^*$

Proof: This follows from $B^D = t_P$ and $B^A = t_A^*$. Q.E.D.

The Effect of More Divided Democracles

COROLLARY. Irrespective of which country makes the first offer, as the legislatures become more protectionist (or less), the aggregate level of barners to which a pair of democracies agree is unchanged.

Proof: Aggregate barriers are always t_P in the joint democracy case which is unaffected by changes in t_C . Q.E.D.

Comparing Barriers of Joint Autocracies and the Mixed Pair

The outcomes here are not independent of the preferences of the players.

PROPOSITION 4. Under TILI, when home (foreign) makes the offer, $B^A \le B^M$ when $2t_A \le t_C(t_A \le t_C - t_P)$.

Proof: Note that in the autocracy case, $t_A = t_{A^*}^*$. (a) When home makes the offer, $B^A = t_A$, and B^M is as in equation A-1 home makes the offer, $B^A = t_A$, and B^M is as in equation A-1 above. If $2t_A \le t_C$, then $2t_A - t_C \le 0 < t_C - 2t_P$ (by the assumption), which means $t_A < (t_C - t_P)$ and equivalently $t_A^2 < (t_C - t_P)^2$. From equation A-1, $B^M = t_C - t_A$ when $t_A^2 < (t_C - t_P)^2$. Then $B^A - B^M = 2t_A - t_C \le 0$ when $2t_A \le t_C$. If $2t_A > t_C$, then either (1) $t_A^2 < (t_C - t_P)^2$, or (2) $t_C^2 - t_P^2 \ge t_A^2 \ge (t_C - t_P)^2$, or (3) $t_C^2 - t_P^2 < t_A^2$. In the first instance, $B^A - B^M = 2t_A - t_C > 0$; in the second instance, $B^A - B^M = t_A - t_P$. By the assumption that $2t_P < t_C$, we have $t_A^2 \ge (t_C - t_P)^2 > t_P^2$. So $B^A - B^M > 0$. In the third instance, $B^A - B^M = -[t_C(t_P - t_A)/\sqrt{t_P^2 + t_A^2}]$. Again, $t_A^2 \ge t_C^2 - t_P^2 > t_P^2$, so $t_A^2 = t_A^2 = t$ $t_A \sqrt{t_A^2 + t_C t_P - t_C^2}) / (t_A^2 + t_P^2);$ it is straightforward to show that $t_A^2 + t_C t_P - t_A \sqrt{t_A^2 + t_C t_P - t_C^2} > 0$. By the assumption that $2t_P < t_C$, we have $t_A^2 > (t_C - t_P)^2 > t_P^2$. So $B^A - B^M > 0$. If $t_A \le t_C - t_P$, then $B^A - B^M = (t_A - \sqrt{t_A^2 + t_C^2})[(t_C - t_A)/\sqrt{t_A^2 + t_C^2}]$. Now $(t_A - \sqrt{t_A^2 + t_C^2}) < 0$, and $t_A \le t_C - t_P \Rightarrow t_A \le t_C$, and, hence, $B^A - B^M \le 0$.

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Mobile Capital, Domestic Institutions, and Electorally Induced Monetary and Fiscal Policy

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The literature on global integration and national policy autonomy often ignores a central result from open economy macroeconomics: Capital mobility constrains monetary policy when the exchange rate is fixed and fiscal policy when the exchange rate is flexible. Similarly, examinations of the electoral determinants of monetary and fiscal policy typically ignore international pressures altogether. We develop a formal model to analyze the interaction between fiscal and monetary policymakers under various exchange rate regimes and the degrees of central bank independence. We test the model using data from OECD countries. We find evidence that preelectoral monetary expansions occur only when the exchange rate is flexible and central bank independence is low; preelectoral fiscal expansions occur when the exchange rate is fixed. We then explore the implications of our model for arguments that emphasize the partisan sources of macroeconomic policy and for the conduct of fiscal policy after economic and monetary union in Europe.

wo literatures in political economy investigate the constraints imposed upon those who determine macroeconomic policy. One school argues that domestic institutions influence both the character of the policy process and the outcomes. Rules that require more than a simple majority for a measure to pass, such as the unanimity requirement on many issues within the European Union's Council of Ministers, or an increase in the number of institutional veto players make change in policy difficult (Garrett and Tsebelis 1996; Tsebelis 1995). Particular combinations of labor market institutions and the partisan composition of government have been linked to macroeconomic performance (Alvarez, Garrett, and Lange 1991). Voters are less likely to punish incumbents for poor macroeconomic performance when responsibility for outcomes is blurred by such factors as minority or coalition government (Powell and Whitten 1993). For similar reasons, governments in majoritarian systems find it difficult to maintain fixed exchange rates (Bernhard and Leblang 1999). Central bank independence

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has been associated with price stability, higher real interest rates, lower variability in those rates, higher growth, greater real output per worker (Alesina and Summers 1993; Cukierman, Kalaitzidakis, Summers, and Webb 1993; Cukierman and Webb 1994; Cukierman, Webb, and Neyapti 1992; De Long and Summers 1992), and a propensity to put external adjustment before internal adjustment (Simmons 1996b). More recently, studies by a number of political scientists suggest that the effect of central bank independence on macroeconomic outcomes depends upon the structure of labor market institutions (Al Marhubi and Willett 1995; Franzese and Hall 1998; Garrett and Way 1995; Hall 1994; Iverson 1998). The common theme in this diverse literature is that institutions, in whatever form, alter the dynamics of the policymaking process and influence the outcomes it produces.

A second school explores whether the international environment limits states' freedom of action (Andrews 1994; Cerny 1994; Kurzer 1993; Moses 1994; Notermans 1993; Pauley 1995; Wallerstein and Przeworski 1995; Webb 1995). Some analysts maintain that increased globalization requires governments to be more sensitive to the demands of fickle markets. Capital will flee states that levy high taxes on business and provide generous social benefits to labor in favor of states with lower tax burdens. States compete for mobile capital by lowering the tax burden and making deep cuts in welfare spending. Over time this competition will lead to the demise of the European social democratic model in favor of the Anglo-American liberal model (Cerny 1994; Sinn 1992; Strange 1996). Others recognize the growing integration of world trade and capital markets but disagree with the globalists about the consequences of these changes. They argue that states retain considerable room for maneuver in more open economies and, indeed, may respond to the risks of international openness by expanding the use of compensatory measures (Garrett 1995, 1998; Rodrik 1997).

These two literatures are not necessarily mutually exclusive. A third body of work examines the interaction between the two schools and considers how institutions refract changes in the international system.

Domestic political institutions can insulate policymakers from systemic changes or, conversely, can leave policymakers more exposed (Garrett and Lange 1995). A large number of domestic veto players also may inhibit states from adjusting policies to changes in their competitors' positions (Hallerberg and Basinger 1998). The general conclusion is that internationalization does not translate frictionlessly into policy change.

Following this last group of authors, we examine how the interaction of the international environment with domestic political institutions constrains a government's ability to engineer preelectoral macroeconomic expansion. It has long been argued that incumbents who want to win the next election may manipulate economic tools at their disposal in an attempt to satisfy the electorate enough to ensure their reelection (Keech 1995, chap. 3; MacRae 1977; Nordhaus 1975; Tufte 1978). Clark and Reichert (1998) find that political business cycles (PBCs) are almost entirely absent in states with independent central banks. They also find that opportunistic cycles are not likely to occur when capital is mobile and the exchange rate is fixed.

One aspect of this finding is curious, however. There are good reasons to expect the identified constraints to make it difficult to use monetary policy for electoral purposes, but there is no a priori reason to expect fiscal policy to be constrained under such circumstances.1 Even when a central bank has complete control over monetary policy, the government can still attempt to affect macroeconomic outcomes through cuts in taxes or increases in expenditures. Similarly, although monetary policy may be constrained when exchange rates are fixed and capital is mobile, incumbents are free to use fiscal policy to create preelectoral expansions. The temptation to use these instruments before elections in order to sway undecided voters may be especially high: in particular, discussions of and attempts to reduce taxes often seem to dominate political campaigns.

Open economy models often privilege monetary policy over fiscal policy, but the literature on the domestic determinants of fiscal policy tends to ignore the international environment in which policy is made (an exception is Oatley [1999]). This may be one reason these studies have produced inconclusive results. For example, Burdekin and Laney (1988) and Franzese (1996) find that central bank independence is related to lower deficits, but several others have not confirmed the relationship (De Haan and Sturm 1994a; Grilli, Masciandaro, and Tabellini 1991; Pollard 1993 [the first and last of these as quoted in Eijffinger and De Haan 1996]). The Mundell-Fleming model indicates that governments can, at best, pursue only two of the following three: capital mobility, fixed exchange rates, and independent monetary policy.² The absence or

presence of each of these conditions affects the nature of the game between a government, which we assume wants to use fiscal and monetary instruments to stimulate the economy shortly before elections, and a central bank, which, so long as it is independent from the government, is immune from electoral pressures. In particular, an independent central bank may be able to prevent opportunistic business cycles when it has control over monetary policy. Such control is possible when capital is immobile, as was generally the case in the industrialized world in the 1960s, or when exchange rates are flexible, as is the case in several countries since the collapse of the Bretton Woods system. When capital is mobile and exchange rates are fixed, however, monetary policy is ineffective, and a central bank, independent or not, cannot deter fiscal expansions. These different expectations based on the Mundell-Fleming conditions may explain why the empirical evidence so far has been mixed.

The article is organized as follows. To sort out the effects of elections, central bank independence, capital mobility, and exchange rate regime on the strategic interaction between fiscal and monetary policymakers, we develop a game theoretic model of the interaction between the government and a potentially independent central bank. We begin the analysis in a closed economy, in which capital is not mobile and exchange rates are fixed, and we find that central bank independence is important in preventing opportunistic monetary cycles but may exacerbate the size of fiscal cycles. We then consider how different Mundell-Fleming conditions affect the game. When exchange rates are fixed and capital is mobile, the international environment restrains governments from pursuing monetary opportunism, but states are expected to engineer preelectoral fiscal expansions. Once again, the independence of the central bank has no causal effect on the occurrence of fiscal cycles. We next examine the predictions of the model with quarterly data from the OECD from 1973 to 1989 for monetary policy and with annual data from the OECD and European Union sources from 1981 to 1992 for fiscal policy. The results largely confirm the theoretical model: There is evidence of preelectoral monetary expansions only when the exchange rate is flexible and central bank independence is low; preelectoral fiscal expansions occur when the exchange rate is fixed. We then consider whether there is evidence for partisan political business cycles that is consistent with our model. Based on the Mundell-Fleming model. Oatley (1999) contends that clear partisan differences existed before the 1990s for fiscal and monetary policy depending upon the underlying economic conditions. Differences between the Left and the Right were present for fiscal policy in the absence of full capital mobility and flexible exchange rates and were present

¹ The authors thank Lawrence Broz, Benjamin Cohen, Peter Hall, and Louis Pauly for their thoughts on this point.

² Discussion of the Mundell-Fleming framework can be found in any open economy macroeconomics text. The foundational works are Fleming 1962 and Mundell 1963. Frieden 1991 and Cohen 1993 are two important extensions of the framework into political economy. The standard Mundell-Fleming model has been criticized for lacking

adequate microfoundations (Dornbusch 1976). Attempts to correct that problem (Corsetti and Pesenti 1997; Dornbusch 1976; Obstfeld and Rogoff 1996) have produced positive conclusions broadly consistent with the standard model At any rate, it is clear that the standard model has guided policy for the last thirty years. See Ghironi and Guavazzi 1998 for a model of interdependence that emphasizes the size of the economy.

for monetary policy in the absence of capital mobility and fixed exchange rates. Unlike Oatley, we find no evidence either for monetary or for fiscal policy. Finally, we argue that political business cycles are likely to become more frequent and worrisome under the fixed exchange rate regime established by the European Economic and Monetary Union.

THE MODEL

Assumptions

Domestic Institutions. "Government" broadly construed can use some combination of monetary and fiscal policy to affect the economy. Monetary policy instruments (the discount rate, purchase and sale of government securities and foreign currencies) can be aimed at policy targets (such as interest rates, the money supply, and exchange rates) in an attempt to produce desired macroeconomic outcomes (growth, price and exchange rate stability, and/or stable balance of payments). The central bank (CB), not elected politicians, ultimately sets the policy instruments. This is not to say that elected officials are irrelevant to the conduct of monetary policy. The extent of their influence varies from country to country and across time. In Germany and the United States, for example, central banks have considerable latitude both in selecting instruments and in choosing targets and goals, whereas in Britain and Norway the central banks traditionally have had much less freedom of action. Although statute and tradition may support a high degree of autonomy, even the Federal Reserve and the Bundesbank are only relatively autonomous. Central bankers are appointed by elected officials, and any latitude they enjoy is granted (and potentially taken away) by elected officials.3

Fiscal policy manipulates taxation and expenditure in an attempt to produce desired macroeconomic outcomes.4 The taxing and spending powers of the central government are controlled by elected officials, but they rely upon the finance ministry for crucial information, analysis, and day-to-day implementation. We adopt the simplifying assumption that the minister of finance is the dutiful agent of the government. The central banker and the minister of finance not only have separate responsibilities and information but also differ in how they evaluate alternative macroeconomic outcomes. We adopt the standard political business cycle assumption that elected officials derive sufficient benefits from electorally timed increases in growth and employment that they heavily discount the negative effect of such behavior on inflation rates. In contrast, as much of the literature argues, decision makers in central banks tend to be more inflation averse than

their principals and, therefore, are likely to discount the value of electorally induced expansions.⁵

We represent actor preferences over macroeconomic outcomes with the following quadratic loss function, which describes how actors assess deviations from their ideal values for unemployment and inflation:

$$L_i = (y - y^{*i})^2 + \alpha^i (\pi - \pi^{*i})^2,$$
 (1)

where π and y are inflation and output, π^* and y^* are the actor's ideal points, α is the weight the actor attaches to inflation stabilization relative to achieving his or her output goals, and i indexes the actor. Deviations from ideal points are squared to capture the intuition that small deviations result in a proportionately smaller utility loss than large ones.

Differences between and among actors in their macroeconomic preferences can be captured by varying assumptions about particular parameters in the model. We emphasize differences between the government and the central bank concerning their ideal rate of growth, but we assume that actors share an ideal point of zero inflation. In addition, both central bankers and governments would like to stabilize the economy at the natural rate of growth, that is, the rate consistent with price stability. The opportunistic political business cycle argument, however, suggests that incumbents are tempted to try to push output above the natural rate just before elections in order to increase electoral support before inflationary consequences are felt. Thus, elections induce a change in preferences, such that

$$y^{*gov} = k^{gov}y^n, (2)$$

where y^n is the natural rate of growth, and k is a coefficient that equals 1 during nonelectoral periods and is greater than 1 in electoral periods. In the absence of central bank independence, these inflationary pressures are transmitted to the central bank through rewards and punishments the government has at its disposal. The central bank then acts "as if" it were minimizing the government's electorally induced loss function. In the absence of independence, the central bank's growth target is as defined in equation 2. If the bank is independent, we assume that it is insulated

³ This creates the opportunity for conflict between government and even the most independent central bank. Berger and Thum (n.d.) analyze this friction, and Berger and Schneider (n.d.) provide evidence that such conflict occurs in Germany

⁴ Fiscal policy has direct distributional consequences, but we will focus on macroeconomic effects in the main body of the article, then return to distributional consequences in the conclusion.

⁵ The same may be true for ministers of finance, who tend to be more concerned about the general health of the economy than are other ministers (Hallerberg and von Hagen 1998).

⁶ Two ways to capture cross-actor differences are found in the literature. Actors may differ in ideal points for inflation and/or output $(\pi^{*i} \neq \pi^{*-i})$ and $y^{*i} \neq y^{*-i}$ (Svensson 1995), or they may differ in the importance they place on inflation stabilization relative to achieving output targets $(\alpha' \neq \alpha^{-i})$ (Lohmann 1992; Rogoff 1985). We adopt the former approach.

⁷ If one is uncomfortable with deriving explanatory power through arbitrary assumptions about changing preferences (and good reasons are provided in Stigler and Becker 1977), then assume that the government is engaged in strategic behavior—it is acting "as if" it were minimizing a loss function, where k > 1 in preelectoral periods in order to maximize its utility in the game it is playing with voters.

⁸ This is the flip side of the logic employed by Blinder (1998), who recommends that politicians instruct the central bank to act as if their ideal point were the natural rate of growth.

from government pressure. Consequently, the bank's growth target will be the same in electoral periods as in nonelectoral periods, such that

$$y^{*cb} = k^{cb}y^n, k = 1.$$
 (3)

The government and central bank both effect changes in macroeconomic outcomes through the manipulation of a short-term, expectations-enhanced Phillips curve:

$$y = y^n + \mu(\pi - \pi^e) + \phi g, \tag{4}$$

where π^{σ} is the rate of inflation expected to prevail at time t, embodied in sticky wage contracts signed at time t-1; g is net government spending; and μ and ϕ are coefficients that characterize the effectiveness of fiscal and monetary policy transmission. The central bank is assumed to control inflation directly, ¹⁰ whereas the government controls spending.

The International Environment. Based on the standard Mundell-Fleming model, the effectiveness with which a government or central bank can manipulate these instruments depends upon the level of capital mobility as well as the exchange rate regime. The Mundell-Fleming model indicates that both instruments are somewhat effective when there is no capital mobility. When capital becomes mobile, however, fiscal policy is effective only when exchange rates are fixed, and monetary policy is effective only when exchange rates are flexible.¹¹

Our model captures the salient aspects of the international environment in the following ways. When capital is immobile, fiscal and monetary policy are only partially effective ($0 < \mu < 1$, $0 < \phi < 1$). When capital is fully mobile and exchange rates are fixed, monetary policy is assumed to be ineffective, and fiscal policy is assumed to be hypereffective (standardized here so that $\mu = 0$ and $\phi = 1$). When capital is fully mobile and exchange rates are not fixed, fiscal policy is ineffective, and monetary policy is hypereffective ($\mu = 1$ and $\phi = 0$).¹²

Order of Play. At the start of the game, the players learn about the structure of the game, that is, they learn whether it occurs in an electoral period, the central bank is independent, capital is mobile, and the exchange rate is fixed. The government then chooses the level of net spending by passing a budget. The central bank observes the budget and then chooses the rate of inflation by deciding whether to raise or lower interest rates or to expand or contract the money supply.

Equilibria

We will solve the game for electoral and nonelectoral periods under alternative assumptions about the independence of the central bank. We first address the closed economy case, then consider the conditional effects of capital flows.

Proposition 1. In general, the government adopts a budget (g), such that net spending is an increasing function of the propensity to push growth above the natural rate and a decreasing function of unanticipated inflation:

$$g = \frac{1}{\Phi} [y^{n}(k-1) - \mu(\pi - \pi^{o})].$$
 (5)

The central bank responds by setting inflation (π) , such that it increases with inflationary expectations and the propensity to push output above the natural rate and decreases with the net budget deficit:

$$\mu = \frac{1}{\mu + \frac{\alpha}{\mu}} [\mu \pi^{e} + y^{n}(k-1) - \phi g].$$
 (6)

Derivation: See Appendix A.

Proposition 1 can be used to derive predictions about central bank and government behavior under different sets of (ideal typical) structural conditions. Specifically, we consider the effect of elections on government and bank behavior and examine whether this effect is modified in important ways by central bank independence and the satisfaction of Mundell-Fleming conditions for national policy autonomy. To establish a baseline, we first discuss the strategic interaction between the government and the central bank in a hypothetical closed economy. We will then consider the effects of different exchange rate regimes in the context of fully mobile capital.

Strategic Interaction under Imperfectly Mobile Capital. In a closed economy, both fiscal and monetary policy are assumed to have some effect on output $(0 < \mu < 1, 0 < \phi < 1)$. In the absence of central bank independence, both fiscal and monetary actors feel pressure to push output above the natural rate (k > 1) during electoral periods. Under these circumstances, both actors' reaction functions are as described in proposition 1. The government anticipates the central bank's response and incorporates it into the budget, setting net government spending so that it is a function of the

⁹ For simplicity, we assume that central banks are either dependent or not.

¹⁰ This is merely a cursory way of saying they manipulate the money supply and/or short-term interest rates in an attempt to effect changes in inflation.

¹¹ See Branson and Butter 1983, Dornbusch 1976, and Mundell 1963. The reasoning is that fiscal expansion (or any other increase in autonomous expenditure) leads to an increase in both moome and interest rates. When capital is mobile, the rise in interest rates attracts capital, which leads to a currency appreciation. When the exchange rate is fixed, the central bank has to expand the money supply to offset the effects of the capital inflow on the exchange rate. Thus, under fixed exchange rates and mobile capital, a fiscal expansion induces a reinforcing monetary expansion. Dooley (1996) suggests in his literature review of capital controls that they be removed in some cases to make fiscal policy more effective in stabilization

¹² These stark conclusions are drawn from a version of the Mundell-Fleming model in which prices are held constant, capital is fully mobile, and the short-term effects of policy are ignored. More measured and realistic conclusions are produced by relaxing these strong assumptions. Nevertheless, the strong conclusions are adopted as a starting point.

TABL	E 1. Comp	parative S	tatic implications of the Model		
	International Conditions B	Central Bank Indepen- dence C	Policy in Electoral Period D	Policy in Nonelectoral Period E	Electorally Induced Policy Change F
1	Closed economy	No	$g = \frac{1}{\phi} [y^n (k-1) + \mu \pi^{\phi}]$ $\pi = 0$	$g = \frac{1}{\Phi} \mu \pi^{\bullet}$ $\pi = 0$	$\frac{1}{4}y^n(k-1)$
2		Yes	$g = \frac{1}{\phi} \left[\left(\frac{\mu^2}{\alpha} + \mu \right) y^n \left(k^G - 1 \right) + \mu \pi^{\bullet} \right]$ $\pi = -\frac{1}{\mu + \alpha' \mu} \left[\left(\frac{\mu^2}{\alpha} + \mu \right) y^n (k^G - 1) \right]$,	$\frac{\frac{1}{\phi} \left[\left(\frac{\mu^2}{\alpha} + \mu \right) y^{\alpha}(k-1) \right]}{-\frac{1}{\mu + \alpha/\mu} \left[\left(\frac{\mu^2}{\alpha} + \mu \right) y^{\alpha}(k-1) \right]}$
3	Fixed rates and mobile	No	$g = y^n(k - 1)$ $\pi = 0$	g = 0 $\pi = 0$	$y^{n}(k-1)$
	capital	Yes	$g = y^n(k-1)$ $\pi = 0$	g = 0 $\pi = 0$	$y^n(k-1)$
4	Flexible rates and mobile	No	$g = 0$ $\pi = \frac{1}{1+\alpha} (\pi^{\bullet} + y^{n}(k-1))$	$g = 0$ $\pi = \frac{1}{1+\alpha} \pi^{\bullet}$	0 $\frac{1}{1+\alpha}y^n(k-1)$
	capital	Yes	$g = 0$ $\pi = \frac{1}{1+\alpha} \pi^{\bullet}$	$g = 0$ $\pi = \frac{1}{1+\alpha} \pi^{6}$	0 0

propensity to push the economy above the natural rate of growth and inflationary expectations (g = (1/ ϕ)[$y^{n}(k-1) + \mu \pi^{e}$]). The central bank observes the level of spending chosen by the government and responds by setting inflation equal to zero ($\pi = 0$). Since k = 1 for both actors during nonelectoral periods, the closed economy nonelectoral equilibrium is a function of expected inflation $(g = (1/\phi)\mu\pi^{e})$ while inflation is zero ($\pi = 0$). The effect of elections on monetary and fiscal policy is simply the difference between the equilibrium policies in the electoral and nonelectoral periods, as shown in column F of Table 1. Hypothesis 1 summarizes the model's primary predictions regarding the effect of elections on fiscal and monetary policy during electoral periods when capital is immobile and the central bank is dependent.

HYPOTHESIS 1. When capital is immobile and the central bank is dependent, the government initiates a fiscal expansion during electoral periods, and the central bank maintains its nonelection period policies. Fiscal, but not monetary, expansions are expected during electoral periods.

When the central bank is independent, equation 6 is reduced to $\pi = (1/(\mu + \alpha/\mu))[\mu \pi^{\sigma} - \phi g]$ during electoral periods. The government anticipates this and adopts a budget that increases with both inflationary expectations and the propensity to push growth above the natural rate $(g = (1/\phi)\mu[((\mu + \alpha)/\alpha)y^n(k-1) + \pi^{\sigma}])$. The central bank responds in an offsetting manner by choosing a rate of inflation that is a decreasing function of the government's propensity to push growth above the natural rate $\pi_t = (-1/((\mu + \alpha)/\mu))[\mu((\mu + \alpha)/\alpha)y^n(k-1)]$. Once again, the absence of elections implies that k = 1, so in the nonelectoral

equilibrium with an independent central bank, the government sets the net budget deficit in response to inflationary expectations $(g = (1/\phi)\mu\pi^{\sigma})$, and the central bank sets inflation equal to zero $(\pi_{\ell} = 0)$, which is the same as the nonelectoral period with a dependent central bank. As before, the effect of elections on monetary and fiscal policies in the presence of central bank independence can be seen in column F of Table 1 and is summarized in the following hypothesis.

HYPOTHESIS 2. When capital is immobile and the central bank is independent, the government initiates a fiscal expansion during electoral periods, and the central bank responds with a monetary contraction. Fiscal expansions and monetary contractions are expected in electoral periods.

A number of further implications can be drawn from the closed economy equilibrium. First, when the central bank is under the influence of the government, it sets inflation equal to zero in both electoral and nonelectoral periods and allows the government to pursue its electoral goals unimpeded. This particular solution to the "assignment" problem is fairly arbitrary and is the result of the way our model is constructed. It is possible to construct a model in which the government "gets out of the way" and lets a subservient central bank achieve the government's electoral goals through interventionist monetary policy or a model in which the government and bank coordinate their actions around whatever policy mix is optimal, given the relative effectiveness of fiscal and monetary instruments. What is significant, however, is that when the bank is subservient, the government's goals are pursued as if one actor were making policy with two goals and two instruments.

The policy dynamic is starkly different when the central bank is independent. The bank initiates a monetary contraction during electoral periods in order to offset the government's electorally induced fiscal expansion. The size of the expansion increases in accord with the effectiveness of monetary policy and decreases with the weight policymakers assign to price stabilization compared to growth targets. To understand why, it is useful to think of the effectiveness of monetary policy (µ) as the "price" (in terms of inflation) policymakers pay for an additional increment of output (refer to equation 5). Consequently, an independent central bank is more willing to use contractionary measures to discipline the government when monetary policy is effective than when it is not. Since the government anticipates a more aggressive stance by the central bank when monetary policy is effective, it counters with a more expansionary policy than it would otherwise adopt.13 When monetary policy is relatively ineffective, the government cannot count on a large offsetting monetary contraction, so it restrains its expansionary impulse. Since fiscal expansions decrease in accord with the weight policymakers place on price stabilization (α) , the net effect of central bank independence on an electorally induced fiscal expansion depends on the relationship between these two parameters. Specifically, if $(\mu^2/(\alpha + \mu))$ is greater than 1, then preelectoral fiscal expansions are larger when the central bank is independent than when it is not.

Equilibrium behavior under imperfectly mobile capital also suggests that net spending decreases as a result of increases in fiscal policy effectiveness (ϕ). As fiscal policy becomes less effective, the levers of policy have to be moved farther in order to hit any given set of targets. This result has profound implications for empirical attempts to gauge the effect of increased capital mobility on the use of fiscal instruments. Garrett (1995), for example, argues that global financial integration has not reduced the effectiveness of fiscal interventions aimed at shielding domestic populations from the vagaries of the international economy (Katzenstein 1985). In fact, Garrett believes there is evidence of increased activism, presumably as the result of governments responding to the increased risks posed by globalization (see also Rodrik 1997). Our model suggests that the apparent evidence of increased fiscal activism may be as much a sign of growing policy ineffectiveness as of continued support for the "compensatory hypothesis."

The results of the model under imperfectly mobile capital are sufficiently provocative to warrant future research, but our concern here is with how the international environment, and more specifically capital

mobility, constrains policymakers. We now turn to the sharper conclusions of the model under the assumption that capital is fully mobile.

Fixed Exchange Rates and Fully Mobile Capital. According to the Mundell-Fleming approach to the balance of payments, national monetary policy is ineffective when exchange rates are fixed and capital is fully mobile, but fiscal policy increases in effectiveness under these conditions. We have standardized the parameters of monetary and fiscal policy effectiveness so that, when these conditions are met, $\mu=0$ and $\phi=1$. To deduce the behavior of the government and the central bank, we need only substitute these values into the reaction functions given in proposition 1 and solve for Stackelberg equilibria, 15 as in the previous case of imperfectly mobile capital.

When exchange rates are fixed, capital is fully mobile, and the central bank is dependent, the government's reaction function reduces to its propensity to push growth above the natural rate $(g = y^n(k-1))$, and the central bank sets inflation equal to zero. During nonelectoral periods the government's reaction function reduces to g = 0, and the central bank continues to adopt a policy of zero inflation. As before, the effect of elections on monetary and fiscal policy under these conditions can be seen in column F of Table 1 and is summarized in the following hypothesis.

HYPOTHESIS 3. When capital is mobile and the exchange rate is fixed, the government initiates a fiscal expansion during electoral periods, but the central bank maintains its nonelection period policies (whether or not it is independent). Fiscal, but not monetary, expansions are expected during electoral periods.

This result has an important implication for the interaction between the international environment and the central bank. Since the bank does not have effective control over domestic monetary policy when the exchange rate is fixed, it no longer conditions its behavior on that of the government. Thus, it is not surprising that a change from a dependent to an independent central bank is not expected to affect the behavior of either fiscal or monetary policymakers.

Flexible Rates and Fully Mobile Capital. The situation is markedly different under flexible exchange rates. In that case, the Mundell-Fleming model tells us that fiscal policy will be ineffective, and monetary policy will be more effective ($\mu = 1$ and $\phi = 0$). If we insert these

¹³ Using a similar model, Berger (1997b) argues that the West German government exploited just such a position as a Stackelberg leader during the 1950s. The combination of a closed economy and a conservative central bank would give the government the large budgets that it wanted but would force the Bundesbank to fight inflation through monetary policy. The only way the bank could escape this trap was to find a credible external commitment device in the return to convertibility in the context of the Bretton Woods pegged exchange rate system (Berger 1997a).

¹⁴ Chronic deficits may induce capital flight and undermine the fiscal expansion. This flight is expected only when investors lose confidence in the government's ability to repay. Short-term increases in the debt due to electoral cycles are unlikely to cause such flight as long as the government is expected to bring the deficit back to its level in the previous nonelectoral period. See Canzoneri and Diba 1999 for a similar argument applied to the Stability and Growth Pact within Europe.

¹⁵ A Stackelberg equilibrium is a set of strategies such that each player chooses its best response given that one player (in this case the government) chooses first.

values into the government's loss function, the first-order condition becomes

$$\frac{\partial L^{gov}}{\partial g} = 0. {(7)}$$

Thus, the ineffectiveness of fiscal policy leads the government to abandon the manipulation of budgets for electoral purposes.

The central bank pursues its best policy in view of zero net spending by government. During nonelectoral periods, the bank sets inflation in response to inflationary expectations $(\pi_t = (1/(1+\alpha)) \pi^e)$ (refer to Table 1). During electoral periods, inflation is an increasing function of the government's propensity to push growth above the natural rate, if the central bank is dependent $(\pi_t = (1/(1+\alpha))(\pi^e + y^n(k-1)))$, but not otherwise $(\pi = (1/(1+\alpha))\pi^e)$ for both electoral and nonelectoral periods if the bank is independent.

HYPOTHESIS 4. When capital is mobile and the exchange rate is allowed to fluctuate, the government maintains its nonelection period policies throughout the electoral period. If it is independent, then the central bank also maintains its nonelection period policies; if it is dependent, the central bank initiates a monetary expansion during electoral periods. Fiscal expansions during electoral periods are not expected. Monetary expansions are expected during electoral periods only if the central bank is dependent.

Summary and Comparison of Results under Alternative Structural Conditions

The model's predictions regarding the effects of elections on monetary and fiscal policies under various structural conditions were reported in column F of Table 1. Comparisons across the rows in that table produce comparative statics related to the effects of structural change on the policy consequences of elections. According to hypotheses 1 and 2, net government spending will be higher in electoral periods than in nonelectoral periods if there are substantial barriers to capital mobility. This is true regardless of central bank independence. Similarly, hypothesis 3 indicates that net government spending will increase in electoral periods when capital is mobile and exchange rates are fixed (the increase is likely to be greater than in the closed economy case). In contrast, hypothesis 4 suggests that no fiscal expansion is expected when capital is mobile and exchange rates are allowed to float. Note that central bank independence is not important in determining the absence or presence of fiscal cycles when capital is fully mobile.

Unlike fiscal policy, electorally induced monetary cycles are very sensitive to the degree of central bank independence as well as the nature and degree of integration with the international economy. Hypotheses 1 and 2 tell us that electorally induced monetary expansions are not expected when capital mobility is limited. Indeed, monetary contractions are expected when capital is immobile and the central bank is

independent. When capital is mobile, both the degree of central bank independence and the nature of the exchange rate regime condition the effect of elections on monetary policy. Hypotheses 3 and 4 indicate that elections are expected to induce monetary expansion only when the exchange rate is flexible and central bank independence is limited. Otherwise, monetary policy is expected to be free from electoral pressures when capital is fully mobile.¹⁶

A TEST OF THE MODEL'S IMPLICATIONS UNDER FULL CAPITAL MOBILITY

The standard method for testing opportunistic political business cycle arguments is to examine the relationship between various macroeconomic outcomes (such as growth, unemployment, and inflation) and the occurrence of elections. Because our focus is on the strategic interaction between fiscal and monetary policymakers, we examine the instruments that the respective agents are presumed to control. In the preceding theoretical discussion, we maintained the fiction that the central bank controls the rate of inflation directly. We now make the more realistic assumption that the central bank controls the money supply. The government is assumed to control the size of the budget surplus (or deficit).

The model put forth earlier suggests that the relationship between elections and policy instruments depends upon the degree of capital mobility, the exchange rate regime, and the degree of central bank independence. Although we modeled the strategic interaction of monetary and fiscal actors under the hypothetical case of a closed economy to provide an analytical baseline, we will test only hypotheses 3 and 4, which assume fully mobile capital. Following Clark and Reichert (1998), we presume that capital was highly mobile when the Bretton Woods System collapsed. Consequently, our empirical examination concentrates on the period 1973-95. This allows us to treat capital mobility as essentially constant and examine the effects of exchange rate regimes and central bank independence on monetary and fiscal cycles. This coding has limitations, but it follows a tradition that treats capital mobility as a systemwide, rather than country by coun-

¹⁶ We are assuming that these variables are exogenous. It is possible, of course, that capital mobility, the exchange rate, and even central bank independence are themselves choice variables and are endogenous to the model. If the exchange rate regime prevents a policymaker from using an instrument for electoral purposes, and if the exchange rate regime is also under the control of the politician, why would not an opportunistic politician just drop the commitment to the exchange rate regime when it becomes inconvenient? The answer provided by the Mundell-Fleming model is that doing so would not mean gaining an electorally useful instrument but a change in which instrument would be effective for electoral purposes. An election may make policymakers more eager to manipulate the economy, but that alone is not enough to create the kind of endogeneity problem that would bias our tests of the link between policy and elections. More generally, we simply do not observe instances in which governments systematically change the exchange rate regime, the level of capital mobility, or central bank independence before elections. We thank two anonymous reviewers for their comments on

TABLE 2. Electorally Induced Cycles In Macroeconomic Policy under Various Structural Conditions

	No Central	Central
	Bank	Bank
	Independence	Independence
Capital mobility and fixed exchange rates Capital mobility and flexible exchange rates	Flscal cycles, no monetary cycles Monetary cycles, no flscal cycles	Fiscal cycles, no monetary cycles No fiscal or monetary cycles

try, variable (Andrews 1994; Frieden 1991; Hallerberg and Clark 1997; Kurzer 1993; McNamara 1998; Webb 1995). To be sure that this assumption is not what drives our results, we also report regressions that consider the effects of country-specific restrictions on capital movements.

Again following Clark and Reichert (1998), we use a dummy variable interaction model to examine the modifying effects of fixed exchange rates and central banking institutions. We use their codings of participation in fixed exchange rate regimes as well as their dummy variable for central bank independence, updating where necessary. The standard test for political business cycles employs a multivariate regression model aimed at isolating the relationship between elections and macroeconomic variables. We will examine two sets of models to evaluate hypotheses related to budgetary and monetary cycles. Table 2 summarizes the predictions of Table 1 regarding electorally induced monetary and fiscal cycles under full capital mobility.

Monetary Cycles

Our theoretical discussion suggests that the existence of electorally induced monetary cycles is likely to be sensitive to the environment. We will test this using a pooled cross-sectional time-series model that extends the empirical work of Alesina and Roubini (1997).¹⁸ They test for monetary cycles using the following equation:

$$m_{u} = \beta_{0} + \beta_{1} m_{u-1} + \beta_{2} m_{u-2} + \dots + \beta_{n} m_{u-n} + \beta_{n+1} PBCN_{u} + \varepsilon_{t},$$
 (8)

where m_{ii} is the growth rate of money for country i at time t, and PBCN is an electoral dummy variable that equals 1 during electoral quarters and in either (depending on the specific test) the three or five quarters before the election. Adding dummy variables and

interaction terms to capture the context-specific effects of our model, equation 8 becomes:

$$m_{t} = \beta_{0} + \beta_{1}E_{t} + \beta_{2}Cbi + \beta_{3}Fixed_{t} + \beta_{4}E \cdot Cbi_{t}$$

$$+ \beta_{5}E \cdot Fixed_{t} + \beta_{6}CBI \cdot Fixed_{t} + \beta_{7}E \cdot CBI \cdot Fixed_{t}$$

$$+ \Sigma(\beta_{1}m_{t-1}) + e_{t}.$$
(9)

Table 3 reports the pooled cross-sectional time-series results for four specifications. Following Alesina and Roubini (1997), models A and B use the annual rate of change in M1 as an indicator of change in the money supply. To ensure that the results are not driven by the way we operationalize the dependent variable, models C and D use the natural log of a monetary aggregate that includes M1 and quasimoney as the dependent variable.19 Columns C and D also differ from Alesina and Roubini's specification in that we control for macroeconomic conditions that are expected to influence monetary policy. Specifically, it is expected to respond in a countercyclical manner to increased unemployment and inflation with a one-period lag.20 Columns A and C use qualitative indicators of the hypothesized constraints on monetary policy. These specifications implicitly assume that capital was uniformly mobile during the period examined and that central banks were either independent or not. The models in columns B and D relax these assumptions by using continuous measures of capital mobility and central bank independence.21 All models include country dummies to control for country-specific fixed effects as well as a single lagged dependent variable to control for autocorrelation. Since the coefficients on the lagged dependent variables are several standard errors away from one, we are confident that unit root and cointegration problems do not exist. Standard errors were calculated using Beck and Katz (1995) panel-corrected standard errors.

Note that the results of the models are qualitatively similar. Our model suggested that electorally induced monetary expansions should occur if and only if the

¹⁷ Clark and Reichert based their codings of exchange rate restrictions on Coffey 1984; IMF, *International Financial Statistics* (various years b); and OECD 1985. Their dummy variable for central bank independence equals 1 when the score for legal independence is above the median, (see Cukierman, Webb, and Neyapti 1992 for an extensive discussion of this measure).

¹⁸ See Appendix B for data sources and descriptions of indicators.

¹⁹ Unlike M1, quasimoney includes such assets as savings accounts, small-denomination time deposits, money markets accounts, and money market mutual fund shares. When added to M1, their sum is a broader monetary aggregate, often referred to as M2. Given the tendency of asset holders to substitute among them, some scholars argue that the broader indicator is a better measure of the money supply. We use both indicators as a robustness test. Also, use of the broader aggregate allows us to include a wider set of countries in our analysis than would have been possible for M1 alone.

²⁰ Unemployment is measured as the quarterly rate of change in the number of persons unemployed. Inflation is the year-to-year rate of change in the consumer price index. See Appendix B.

²¹ To create a continuous measure of eroding monetary policy autonomy as a result of the interaction between exchange rates and capital mobility, we averaged, inverted, and rescaled on the interval from 0 to 1 the eight capital control variables used in Simmons (1996a) and multiplied this figure by the dummy variable for fixed exchange rates. The latter equals one when the exchange rate is fixed, zero otherwise, so the resulting variable equals zero when the exchange rate is flexible and is an increasing function of the degree of capital mobility when the exchange rate is fixed. This variable and the Cukierman, Webb, and Neyapti legal measure of central bank independence appear as Fixed and CBI in columns B and D of Table 3, respectively.

	$\underline{\qquad} m = (m1_t - 1)$	m1 _{t-4})/m1 _{t-4}	m = Log(M1)	+ Quasimoney)
	Qualitative	Continuous	Qualitative	Continuous
	Modifiers	Modifiers	Modifiers	Modifiers
	(16 countries)	(16 countries)	(18 countries)	(18 countries)
	A	B	C	D
Intercept	2.322**	1.646*	0.0683**	0.0293*
	(0.488)	(0.679)	(0.0083)	(0.0116)
Election	1.070*	1.296*	0.0042*	0.0078*
	(0.4 99)	(0.716)	(0.0019)	(0.0037)
CB i	1.6 9 8*	4.323*	0.0056	0.0319
	(0.777)	(2.019)	(0.0059)	(0.0280)
Fixed	1.650	-1.553	-0.0082*	0.0030
	(1.056)	(1.111)	(0.0033)	(0.0046)
Election × CBI	-1.189*	-2.436	-0.0010	-0.0109
	(0.665)	(1.770)	(0.0030)	(0.0105)
Election × Fixed	-0.211	-0.946	-0.0027	−0.0148 *
	(0.641)	(1.316)	(0.0030)	(0.0074)
CBI × Flxed	-0.419	-4.756	0.0226**	-0.0632*
	(1.341)	(5.566)	(0.0047)	(0.0313)
Election \times CBI \times Fixed	1.195	4.297	0.0025	0.0351*
	(0.999)	(3.447)	(0.0059)	(0.0207)
M_{t-1}	0.797**	0.79 6**	0. 99 34**	0.9978**
	(0.027)	(0.028)	(0.0013)	(0.0013)
Lagged change in unemployment			-0.0001 (0.0001)	-0.0001 (0.0001)
Lagged Inflation			-0.0848 (0.0500)	-0.0022 (0.0487)
N	928	927	1,148	1,147

Note: Numbers are coefficients and panel-corrected standard errors using pair-wise deletion to estimate covariances. Columns B and D use the Culderman, Webb, and Neyapti (1992) measure of legal independence and a continuous measure of eroding monetary policy autonomy (see note 20). Columns A and C use a categorical variable that equals 1 if the country's score is above the sample median, 0 otherwise, and a categorical measure of eroding monetary policy that equals 1 if the exchange rate is fixed, 0 otherwise *p < .05, **p < .01; one-tailed test used for coefficients involving "CBI = central bank independence

central bank is not independent and the exchange rate is allowed to fluctuate. Because the electoral coefficient in Table 3 describes the relationship between elections and the money supply when both modifying variables equal zero, it describes the situation when central banks are dependent and exchange rates are flexible. The statistically significant positive coefficient for *Election* in all four specifications in Table 3 is evidence that monetary expansions occur, as expected, when these conditions prevail.

Furthermore, specifications of equation 9 that use qualitative modifying variables make it easy to test all four implications of hypotheses 3 and 4 related to monetary policy by calculating the conditional coefficients for each relevant institutional combination (refer to Table 2).²² The conditional coefficient for the effect of elections on the money supply when exchange

rates are fixed and the central bank is independent can be determined by substituting the appropriate values of the institutional variables into equation 9.

$$m_{t} = \beta_{0} + \beta_{1}E_{t} + \beta_{2}(1) + \beta_{3}(1) + \beta_{4}(E \cdot (1))$$

$$+ \beta_{5}(E \cdot (1)) + \beta_{6}(1) + \beta_{7}(E \cdot (1) \cdot (1)) + \sum_{i}(\beta_{i}m_{i-j})$$

$$+ e_{t}, \qquad (10)$$

which simplifies to

$$m_{t} = \beta_{0} + \beta_{1}E_{t} + \beta_{2} + \beta_{3} + \beta_{4}E + \beta_{5}E + \beta_{6} + \beta_{7}E + \Sigma(\beta_{t}m_{t-1}) + e_{t},$$
(11)

and then comparing the electoral (E=1) and nonelectoral (E=0) periods in the presence of both constraints. Were a monetary cycle to occur under such conditions, the change in the money supply should be greater during electoral (left side of equation 12) than nonelectoral (right side of equation 12) periods:

²² See Friedrich 1982 and Jaccard, Turnsi, and Wan 1990 for a useful introduction to the conditional interpretation of multiplicative interaction models.

(12)

TABLE 4.	Conditional	Effects	of	Elections on
Monetary	Policy			

Central Bank	Exchang	ge Rates
Independence	Flexible	Flxed
Calculated from column C In Table 3		
High	0.0032 (0.0022)	0.0030 (0.0045)
Low	0.0042* (0.0019)	0.0014 (0.0023)
Calculated from column A in Table 3		
Hlgh	-0.118 (0.429)	0.866 (0.655)
Low	1.071* (0.499)	0.859* (0.413)

$$\beta_1(1) + \beta_2 + \beta_3 + \beta_4(1) + \beta_5(1) + \beta_6 + \beta_7(1)$$

$$> \beta_1(0) + \beta_2 + \beta_3 + \beta_4(0) + \beta_5(0) + \beta_6 + \beta_7(0),$$

parentheses *p < 05.

which simplifies to $\beta_1 + \beta_4 + \beta_5 + \beta_7 > 0$. By analogous reasoning, a finding that $\beta_1 + \beta_4 > 0$ or $\beta_1 + \beta_5 > 0$ would be evidence of electorally induced monetary expansion under dependent central banks with fixed exchange rates and independent central banks with floating exchange rates, respectively. Hypotheses 3 and 4, however, predict that monetary cycles will not occur under such circumstances, so we expect that $\beta_1 + \beta_4 + \beta_5 + \beta_7$, $\beta_1 + \beta_4$, and $\beta_1 + \beta_5$ will each be indistinguishable from zero. In contrast, hypothesis 4 predicts that electorally induced monetary expansions will occur when the exchange rate is flexible and the central bank is dependent. As noted above, the test for this last proposition is simply $\beta_1 > 0$.

The conditional coefficients, and their associated standard errors, based on the results in column C of Table 3, are reported in the upper portion of Table 4. Note that the coefficients in Table 4 are estimates of the relationship between elections and monetary policy under the various open economy conditions presented in Table 1. As hypothesis 4 predicts, when capital is mobile, there is evidence of electorally induced monetary expansions if the exchange rate is allowed to fluctuate, but only when central bank independence is limited. As hypothesis 3 predicts, there is no evidence of such expansions when the exchange rate is fixed, regardless of central bank independence.

The lower portion of Table 4, which reports the conditional coefficients and standard errors calculated from column A, suggests that the relationship between elections and monetary policy may be more complicated than our model suggests. As in the upper portion of the table, the results are consistent with hypothesis 4: There is evidence of electorally induced monetary cycles when the exchange rate is flexible, but only when the central bank is dependent. Yet, because they

suggest that electorally induced monetary cycles occur when central bank independence is limited and the exchange rate is fixed (lower right cell), the results in the lower part of Table 4 are not consistent with the monetary implications of hypothesis 3.

One explanation for this anomalous finding is that the Mundell-Fleming model rules out monetary policy as an independent instrument when the exchange rate is fixed, but it also explains why the money supply may nonetheless be tied to the electoral calendar.²³ If, as our model predicts, preelectoral fiscal expansions occur under a fixed exchange rate regime, then the resulting increase in interest rates will put upward pressure on the domestic currency. The central bank is obligated by the exchange rate commitment to counteract this pressure by selling domestic currency, thereby expanding the money supply.²⁴ If this link between monetary and fiscal policy is sufficiently strong, the electorally induced fiscal expansions that, according to hypothesis 3, are expected to occur under fixed exchange may also be reflected in monetary aggregates. The central bank could sterilize the effects of the foreign exchange intervention by engaging in offsetting sales of government securities. Since this would reduce the expansionary consequences of the government's electorally induced fiscal policy, we would expect dependent central banks to be less apt to sterilize than independent ones. The insignificant conditional coefficient for the case of fixed exchange rates and an independent central bank is consistent with this conjecture.

In sum, the evidence supports the main implications of our model for monetary policy. The existence of opportunistic monetary cycles is conditioned by central bank independence and the exchange rate regime. Furthermore, there is evidence of monetary expansion when the government retains national monetary policy and influence over the central bank. There is no evidence of such cycles when the central bank is independent or the exchange rate is allowed to float. Contrary to our expectations, there is some evidence of electorally induced cycles in monetary policy when the exchange rate is fixed. But, in contrast with our other findings, this evidence is not robust with respect to the choice of the aggregate used to measure the money supply, and it could be interpreted as indirect support

²³ We are indebted to Tom Willett on this point.

²⁴ This automatic monetary expansion makes fiscal policy particularly effective when capital is mobile and the exchange rate is fixed. As Fablo Ghiron points out (personal communication), an examination of foreign exchange interventions may allow future researchers to distinguish between electorally induced monetary cycles (as understood in this article) and the monetary consequences of electorally induced fiscal cycles.

²⁵ Alesina and Roubini (1997) find evidence of electorally induced monetary expansion in their sample of 18 OECD countries, but they assume that the occurrence of monetary cycles is uniform within their sample. Our evidence suggests that they inappropriately pooled cases where monetary cycles are expected to occur with those where they are not. Although we did not conduct country-specific tests, our finding is in tension with evidence of an electoral monetary cycle in the United States (Grier 1989) and consistent with studies that find a lack of evidence for such a cycle (Beck 1987). For more on the U.S case, see contributions to Mayer 1990.

for the model's predictions about fiscal policy.²⁶ We now turn to a direct examination of the fiscal policy results.

Fiscal Cycles

In contrast to monetary policy, hypothesis 3 indicates that electorally induced fiscal cycles are likely when exchange rates are fixed and that the effectiveness of fiscal policy on output increases under fixed exchange rates when capital is mobile. Hypothesis 4 predicts that fiscal policy is ineffective when exchange rates are flexible and capital is mobile, and under these conditions there should not be fiscal cycles.

Previous research generally asks whether deficits are likely to increase in electoral years. Unlike the empirical work on monetary cycles, the literature codes data on a yearly rather than quarterly basis.²⁷ The evidence is decidedly mixed. Some authors indicate support for opportunistic fiscal cycles in some subset of industrialized countries (Alesina and Roubini 1997; Hallerberg and von Hagen 1999; Franzese 1996 finds such cycles when the replacement risk to a sitting government is high), and others find no support for such cycles (De Haan and Sturm 1994b). One reason may be that the studies do not differentiate between the effects of fixed and flexible exchange rates or mobile and immobile capital.

We rely on data sets from two recent articles, De Haan and Sturm (1997) and Hallerberg and von Hagen (1999). Our rationale is as follows. Each covers a somewhat different group—De Haan and Sturm (1997) examine 19 OECD countries, and Hallerberg and von Hagen (1998, 1999) use the 15 current members of the European Union.²⁸ Much of the economic data also come from different international organizations.²⁹ If

26 The somewhat different findings with the two monetary measures complicate the interpretation of the results, but the predictions of the model are broadly supported. First, our model predicts that central bank independence will influence electorally motivated monetary, but not fiscal, policy. The lower part of Table 4 supports the notion that central bank independence disturbs the link between elections and monetary policy when the exchange rate floats but not when the exchange rate is fixed, which is consistent with the contention that the observed link under fixed exchange rates is driven by fiscal policy. Second, the conditional electoral coefficients are smaller and their standard errors are larger when the exchange rate is fixed than when the exchange rate floats and the central bank is dependent. This is consistent with the idea that the results for the case of floating exchange rates and a dependent central bank reflect the deliberate manipulation of monetary instruments for electoral purposes, whereas the fixed exchange rate results reflect reverberations from fiscal policy. Finally, the coefficients calculated from tests based on a broader monetary aggregate, which is less likely to reflect exchange rate interventions driven by fiscal policy, show no evidence of electorally induced fiscal policy when the exchange rate is fixed.

²⁷ Quarterly data on expenditures and tax collections can have a seasonal element both within and across countries that is difficult to control for, even with sophisticated econometric techniques.

²⁸ The original Hallerberg and von Hagen data set covers 1981–94. We thank Rolf Strauch for providing updated data for the economic variables. See Appendix B for original sources.

²⁹ De Haan and Sturm rely on *OECD Economic Outlook* for most of their data, whereas Hallerberg and von Hagen drew mostly from *Statistical Annex of European Economy* (see Appendix B). There is a difference in particular in the debt coding rules. The European

we find evidence of politically induced fiscal cycles in both data sets, then we will have more confidence in the robustness of the results. We also restrict the period to 1981-92 for the Hallerberg and von Hagen data set and to 1982-92 for the De Haan and Sturm data set. We did this for two reasons, both of which concern policy in Europe: In fall 1992 the Exchange Rate Mechanism suffered a severe crisis in the markets, which put in doubt the credibility of fixed exchange rates; and after 1992 states that wished to join the Economic and Monetary Union faced restrictions on their debt levels, which presumably had a direct effect on fiscal policy. The beginning date of the respective data sets is the same as in the published articles, and we want to emphasize that this period is theoretically interesting. Several European Community countries reestablished credible fixed exchange rates with the Exchange Rate Mechanism in 1979, and both data sets allow strong tests of whether a country's exchange rate regime affects the likelihood of fiscal expansion before an election.

The model upon which these articles build, and which has become a standard in the field, is that of Roubini and Sachs (1989). Their regression equation is

$$db_{u} = \beta_{0} + \beta_{1}db_{u-1} + \beta_{2}dU_{u} + \beta_{3}dy_{u} + \beta_{4}b_{u-1}d(r_{u} - y_{u}) + \beta_{5}pv_{u},$$
(13)

where the dependent variable, db_u , is the change in the ratio of gross debt to GDP. The equation contains a set of economic variables and a set of political variables (represented as pv_{μ}). The economic variables are: db_{u-1} , the lagged debt ratio; dU_{i} , the change in the unemployment rate; and $b_{u-1}d(r-y)$, the change in debt servicing costs, which is computed as the change in the real interest rate minus the change in the growth rate times the gross deficit in the previous year. In addition, some authors include the change in real GDP, dy_i . To be consistent with previous work, we include that change in our regression using the data of Hallerberg and von Hagen, as they do, and we follow De Haan and Sturm by not including it in our regression with their data.30 These economic variables are expected to influence the budget in a given year; higher levels of unemployment and debt servicing costs should increase government debt levels, and higher levels of economic growth should decrease debt levels.31

The standard political variables generally include codings for institutional differences or the partisanship orientation of the government. Roubini and Sachs (1989) received much attention for their finding that

Commusion uses Maastricht definitions for debt and inflation, which have minor accounting differences with the OECD data.

³⁰ There is some controversy about including growth in GDP as an independent variable because real GDP appears in the denominator on the left-hand side of the equation (see also Borelli and Royed 1995). We are not interested in this controversy per se, so we follow the practice of the respective authors.

³¹ These expectations are only valid for industrialized nations. Talvi and Végh (1997) find that the sign on economic growth is reversed in Latin American countries. They hypothesize that governments can only justify painful cuts in expenditures or increases in taxes to their constituencies when economic conditions worsen

the type of government affects the size of budget deficits. One-party majority governments maintain the tightest fiscal discipline, two- or three-party majority governments less so, and four- or five-party governments even less; minority governments, regardless of the number of parties in the coalition, are the most undisciplined. Many other researchers have examined government type and other political variables. As control variables, we use the political variables in De Haan and Sturm (1997) as well as Hallerberg and von Hagen (1999). De Haan and Sturm found that Roubini and Sachs made several coding errors and, based on the original coding procedures, argue that the type of government does not affect deficit levels.

Hallerberg and von Hagen, following Edin and Ohlsson (1991), break the government type variable into three separate dummies and add a variable—the percentage of cabinet portfolios held by leftist parties—to control for partisan effects. They also add variables for election years and for two fiscal institutions, a strong finance minister and negotiated targets. Among their findings were a connection between government type and a fiscal institution that is meant to reduce the size of deficits, but no effect of government type per se on the size of deficits. In particular, delegation to a strong finance minister who can monitor spending ministers and punish those who "defect" is feasible in states with one-party majority governments. In multiparty and minority governments, the coalition members are not willing to delegate such power to one actor. Commitment to numerical targets negotiated among the coalition partners for each ministry provides an alternative in multiparty govern-

In order to test our contention that political business cycles are most likely to occur when exchange rates are fixed, we structure the regression equations to include the economic and political factors noted above as well as variables to consider the effects of exchange rate regime and of elections. Our equations are:

$$db_{u} = \alpha + \beta_{1}Election + \beta_{2}Flexible + \beta_{3}Election$$

$$\times Flexible + \beta_{4}db_{u-1} + \beta_{5}dU_{u} + \beta_{6}b_{u-1}d(r_{u} - y_{u})$$

$$+ \beta_{7}Government Type, \qquad (14)$$

when employing the De Haan and Sturm data, and $db_{u} = \alpha + \beta_{1}Election + \beta_{2}Flexible + \beta_{3}Election \\ \times Flexible + \beta_{4}db_{u-1} + \beta_{5}dU_{u} + \beta_{6}b_{u-1}d(r_{u} - y_{u}) \\ + \beta_{7}dy_{u} + \beta_{8}Two\text{-Three Party Govt.} \\ + \beta_{9}Four\text{-Five Party Govt.} + \beta_{10}Minority Govt. \\ + \beta_{11}Strong Finance Minister \\ + \beta_{12}Negotiated Targets$

+ β_{13} Strong Finance Minister × Election + β_{14} Negotiated Targets × Election + β_{15} Left, (15)

when the Hallerberg and von Hagen data are used, where β_1 is the coefficient when there are elections under fixed exchange rates, β_2 is the coefficient for flexible exchange rates and no elections, and β_3 captures the effect of a change to a flexible exchange rate on the relationship between the debt and elections. This specification of the variables follows closely the model provided in the section on monetary policy. Our expectations about the effects of these variables differ, however. Based on hypotheses 3 and 4 presented in Table 1, we expect fiscal expansions to occur only when exchange rates are fixed (i.e., when Flexible is equal to zero). When exchange rates are flexible, we expect that fiscal policy is ineffective and a government will not initiate a preelectoral fiscal expansion.

Because of our interest in the effect of elections under different exchange rate regimes, we consider two alternative methods for coding elections. Hallerberg and von Hagen (1999) calculate their election variable as a dummy for years in which an election is held. This follows standard practice in the literature but is at best inexact and at worst inaccurate. Some countries hold elections in early spring, others in late autumn. The same country may even hold elections in different times of the year over different electoral cycles; for example, in the United Kingdom, there were two elections in 1974, in February and October, and just five years later elections were in May. A variable that makes no distinction among these presumably will understate the effect of elections as well as increase the standard error.

We supplement the standard measurement of elections with Franzese's (1996) more exact definition. He calculates Election as the proportion of preelectoral year; for example, a February 1 election would be coded as 1/12 in that year and 11/12 in the previous year.33 We applied this coding to the two data sets to see whether the coding rules affected the results. Our model predicts that fiscal cycles should occur when exchange rates are fixed, which in our regressions is the case when election equals 1 and flexible equals 0. The sign of election should therefore be positive. The model also predicts that the coefficient for the interaction term election × flexible should be negative; indeed, since governments have no incentive to use fiscal cycles when exchange rates are flexible, one expects that the coefficients for election and election × flexible will sum to zero. To test directly for the effect of flexible exchange rates on fiscal cycles, we also calcu-

³² Hallerberg and von Hagen (1999) refer to a strong finance minister as a "delegation" approach and to negotiated targets as a "contract" approach.

³³ In particular, Franzese codes elections for relevance to economic policy. Policy-relevant elections can include, for example, presidential elections in Finland, France, and the United States as well as upper house elections. He then divides the value of the variable between those separate elections, so that, for example, a French presidential election and a French parliamentary election are each coded. 5.

	DeHaan and Sturm Data*			Hallerberg and von Hagen Datab		
	1982–92 A°	1982–92 B ^d	1982–92 C⁴	1981–92 D°	1981–92 E ²	1981– 9 2 F ^o
Vanables of Interest	_		_			
Election	0.49 (0.60)	1.52* (0.75)	1.71 * (0.76)	1.25 (0. 90)	2.97** (1.13)	2.81 ** (1.04)
Flexible	-0.20 (0.60)	0.14 (0.64)		0.90 (0.96)	0.78 (1.11)	
Election × Flexible	-0.26 (1.18)	-1.42 (1 <i>.</i> 25)		-1.44 (1.60)	-1.12 (2.56)	
No fiscal policy autonomy			0.11 (0.59)			-0.09 (1.10)
Election × No fiscal policy autonomy			-1.99 (1.32)			-0.78 (2.57)
Conditional Coefficients						
Election (FlexIble = 0)	0.49 (0.60)	1.52 * (0.75)		1.25 (0.90)	2.97** (1.13)	
Election (Flexible = 1)	0.22 (0.85)	0.10 (0.98)		-0.20 (1.88)	1.84 (2.73)	
Control Variables						
Intercept	0.67 (0.59)	0.35 (0.63)	0.42 (0.56)	1.97 (1.01)	1.45 (1.14)	1.99 (1.23)
d Debt _{r-1}	0.47** (0.10)	0.48 ** (0.10)	0.47 ** (0.10)	0.56** (0.10)	0.59 ** (0.10)	0.59 ** (0.10)
d Unemployment	1.27 ** (0.22)	1.27** (0.22)	1.26** (0 <i>.2</i> 2)	0.04 (0.05)	0.04 (0.05)	0.04 (0.05)
d GDP				-0.88** (0.13)	-0.85 ** (0.14)	-0.84 ** (0.13)
d Debt costs	0.38* (0.15)	0.39** (0.14)	0.39 ** (0.13)	0.18 (0.23)	0.19 (0.23)	0.19 (0.23)
Government type	-0.17 (0.24)	-0.16 (0.25)	-0.21 (0.23)			
2-3 party govt.				0.93 (0.79)	0.90 (0.85)	0.74 (0.90)
4–5 party govt.				0.96 (0.82)	0.91 (0.83)	0.51 (0.91)
Minority govt.				0.36 (1.04)	0.10 (1.08)	−0.22 (1.18)
Strong finance minister				-1.55* (0.92)	0.90 (1.04)	−0.75 (1.01)
Negotlated targets				0.00 (0.79)	0.47 (0.83)	0.18 (0.85)
Strong finance minister \times Election				1.29 (1.61)	-0.57 (2.65)	−0.74 (2.51)
Negotiated targets \times Election				-1.11 (1.33)	-2.51 (1.59)	-2.33 (1.64)
Left				0.06 (0.87)	0.08 (0.75)	0.09 (0.75)

Note: The dependent variable is the change in the ratio of gross debt to GDP. Panel-corrected standard errors are represented in parentheses. Following the original procedures, we do not include country dummy variables, although their inclusion does not affect the qualitative results. Note that the political variables (election, the three variables for the type of government, strong finance minuters, and negotiated targets) are evaluated according to a one-tasked test $^{\circ}p < .05$, $^{\circ}p < .01$.

*19 OECD countries

*15 Furnoses I linear members

206

206

175

206

175

175

⁶15 European Union members.

[°]Standard coding °Franzese coding.

TABLE 6. Conditional Coefficients for Elections with the Hallerberg and von Hagen (European Union) Data Set under Fixed Exchange Rates and Different Fiscal Institutions

Neither targets nor strong finance minister	2.97** (1.13)
Targets only	0.45 (1.09)
Strong finance minister only	2.40 (2.37)
**p < .01.	

lated the conditional coefficient for elections when exchange rates are flexible and when they are fixed.

Table 5 reveals strong evidence that countries with fixed exchange rates experience fiscal cycles and that flexible exchange rates eliminate these cycles. In all the equations with the more precise measure for elections, the positive and significant conditional coefficient means that preelectoral fiscal expansions occur when the exchange rate is fixed but not when it is flexible. For example, in column B the coefficient for election indicates an increase in the gross debt level of roughly 1.5 points during electoral periods when the exchange rate is fixed, compared to no increase during electoral periods when the rate is flexible, that is, when the sum of the coefficients for election and election \times flexible is close to zero (for convenience, we present the conditional coefficients for each exchange rate regime and their associated standard errors in Table 5). The effect of exchange rate regime on the relationship between budgets and elections is most dramatic with the second equation using the Hallerberg and von Hagen data set (column E): States with fixed exchange rates will increase their ratio of gross debt level to GDP almost three percentage points in the year before an election.

This difference in the effect of the electoral period in the two data sets may be the result of the institutional variables added by Hallerberg and von Hagen. Most important, because models D, E, and F employ interactions between election and Strong Finance Minister and election and Negotiated Targets, the interpretation of the conditional coefficient changes. The coefficient for election now summarizes the relationship between elections and gross debt when the exchange rate is flexible in the absence of a strong finance minister or the use of negotiated targets.

Table 6 computes conditional coefficients for the effect of fiscal institutions. In both cases the fiscal institution eliminates the increase in debt before elections. It appears, therefore, that these domestic institutions constrain fiscal policy in much the same way that central bank independence constrains monetary policy.

There are additional nuances in the results given in Table 5. As columns A and D indicate, the standard election variable has the correct sign but is not significant when we do not use the Franzese coding for

elections. In both data sets, however, the effect almost doubles, and the standard deviation decreases, when Franzese's more exact measure is used (columns B and E). Once again, fiscal electoral cycles are completely absent when exchange rates are flexible.³⁴

Models C and F in Table 5 allow us to test the robustness of our results by relaxing the assumption that capital was fully mobile in every country during the entire observation period. Similar to the coding of "no monetary policy autonomy" in the previous section, we created a variable called "no fiscal policy autonomy (nofpa)" and substituted it into the regressions for flexible. This continuous variable is the product of the Simmons (1996a) coding of capital controls and the exchange rate regime dummy variable. It ranges from zero (when flexible = 0 and/or there are no capital controls) to 1 (when flexible = 1 and all capital controls are present). The findings are qualitatively the same as for the regressions that use a rougher measure of capital mobility: Elections are associated with an increase in government debt when fiscal policy autonomy is retained (compare the coefficient on election in columns C and F), but this relationship is diminished when fiscal policy autonomy erodes (compare the coefficient on the interaction term). This provides indirect support for our earlier contention that capital was more or less mobile during the period examined.

The results fit our expectations. There is evidence of preelectoral fiscal expansion when the exchange rate is fixed, which confirms hypothesis 3. The conditional coefficient for elections is positive and highly significant. In comparison, when we hold relevant domestic institutions constant and move to a flexible exchange rate, in all cases the size of the conditional electoral coefficient is reduced. This result holds for all combinations of domestic fiscal institutions, and it confirms hypothesis 4: All else equal, the conditional coefficient for elections is always greater under a fixed exchange rate than a flexible regime.

EXTENSIONS OF MODEL 1: PARTISAN POLITICAL BUSINESS CYCLES

We have concentrated on the effects of international conditions as well as domestic political institutions on the propensity of governments to engineer preelectoral expansions of either the money supply or the budget deficit. Our model also allows us to consider how these international and domestic factors can be integrated into a second tradition in political economy, namely, whether partisan differences systematically translate into differences in macroeconomic policy. One school contends that there is a clear divide between the policies that left and right governments pursue. Compared to rightists, leftists are expected to prefer higher inflation and the higher employment that accompanies it and to support a consistently higher level of the

³⁴ In unreported results, we restricted the De Haan and Sturm data just to European Union countries to test whether the differences between the two data sets were due to regional factors, but the gap remained.

money supply. This view is largely based upon the assumption of a stable Phillips curve tradeoff between inflation and unemployment. In terms of fiscal policy, leftist governments run larger budget deficits than rightist governments, again to boost employment. The reasoning behind these arguments is straightforward—leftist governments appeal to their supporters, who are usually laborers, whereas rightist governments appeal to their backers, who are usually capital owners (the classic article is Hibbs 1977).

There is some debate about how internationalization affects the likelihood of partisan cycles. As we stated earlier, several scholars insist that global capital mobility will end the ability of leftist governments to act differently from rightist governments (in addition to earlier citations, for literature surveys see Garrett 1998, chap. 1, and Clark n.d., chaps. 4 and 6). Capital will simply move to the country where it is best treated, and there will be an inevitable harmonization of policy as capital becomes more mobile. Others, such as Garrett (1995, 1998) and Rodrik (1997), insist that partisan differences will remain or even increase in the face of more internationalization. Populations become more uncertain in a global world and demand an expansion of the welfare state rather than a contraction. Clark (n.d.) finds no evidence of partisan cycles before or after an increase in internationalization. In a study with some similarity to ours, Oatley (1999) examines how fixed and flexible exchange rates together with increasing capital mobility affect partisan cycles. Based on a Mundell-Fleming framework, he argues that there are partisan monetary cycles in the form of real money market interest rates that go away when there is open capital and a fixed exchange rate regime, and there are partisan fiscal cycles in the form of larger budget deficits that disappear when there is open capital and a flexible exchange rate regime.35

A useful feature of the formal model we developed earlier is the ease with which it can be applied to the partisan debate. Recall that the variable which differentiated between periods before and not before elections was k, which we assumed was greater than 1 in election periods and equal to 1 in nonelection periods. Since k is the parameter that captures the propensity of policymakers to push output above the natural rate, it can be interpreted as a function of the expansionary tendencies of the government. Consequently, an extension of the model is to replace "election" with "leftist government" in the model and to test the resulting hypotheses with data that interact partisanship with the relevant structural conditions.

Hypothesis 5. Our expectations about the effect of partisanship are as follows.

- A. When capital is immobile and the central bank is dependent, leftist governments will create fiscal but not monetary expansions.
- B. When capital is immobile and the central bank is independent, leftist governments will create fiscal expansions and monetary contractions.
- C. When capital is mobile and the exchange rate is fixed, leftist governments will create fiscal but not monetary expansions, regardless of central bank independence.
- D. When capital is mobile and the exchange rate is flexible, leftist governments will create monetary but not fiscal expansions, unless the central bank is independent.

If the partisan literature is correct and there is a fundamental difference between the preferences of leftist and rightist governments, then we expect the following for monetary policy under leftists: monetary contractions when capital is immobile and the central bank is independent; monetary expansions when capital is mobile, the exchange rate is flexible, and the central bank is dependent; and no change in policy under the remaining conditions. For fiscal policy, under leftists there will be fiscal expansions when capital is immobile and central banks are dependent as well as when capital is mobile and exchange rates are fixed, but otherwise no partisan differences should be evident.³⁶

To test these hypotheses, we begin with the same empirical models used earlier. This facilitates a ready comparison with the opportunistic political business cycle results as well as with the literature that established these models. In place of the electoral variables we use a variable for partisanship based on Woldendorp, Keman, and Budge (1998). The variable takes a value of 1 for right-wing dominance and 5 for left-wing dominance, and it follows Oatley's (1999) coding.

The results for the test of partisan differences in monetary policy—conditioned on both the exchange rate regime and the degree of central bank independence—are presented in Table 7. Since the coefficient for Partisanship describes the expected causal effect of a one-unit increase in leftist governance when central bank independence is limited and the exchange rate is flexible, hypothesis 5D predicts that this coefficient should be positive and statistically significant. The fact that it is positive but not significant in columns A and B and significant but negative in columns C and D means that, under the conditions in which partisanship is likely to matter the most, there is no evidence for the predicted effect of the ideological orientation of government.

As was the case with our examination of the consequences of elections, it is possible to test the partisan

³⁵ Oatley notes that his results disappear in the 1990s, which, because that is the period when capital arguably became most mobile, would seem to be prima facie evidence against his argument. More generally, he finds supporting evidence for approximately half the hypotheses.

³⁶ Note that the inclusion of the central bank as an important actor leads to somewhat different predictions from those of Oatley (1999). In particular, we expect independent central banks to prevent monetary expansions, and they even may lead to monetary contractions under leftist governments when capital is immobile. Moreover, under conditions of immobile capital and a dependent central bank, the partisan cycle shows up in our model only on the fiscal side.

	$m = (m1_t - m)$	$m = (m1_t - m1_{t-4})/m1_{t-4}$		+ Quasimoney)
	Qualitative	Continuous	Qualitative	Continuous
	Modifiers	Modifiers	Modifiers	Modifiers
	(16 countries)	(16 countries)	(18 countries)	(18 countries)
	A	B	C	D
Partisanship	0.221	0.416	-0.00190**	-0.00352**
	(0.224)	(0.316)	(0.000 6 7)	(0.00137)
СВІ	1.338	4.986	0.00761	0.02748*
	(1.209)	(2.643)	(0.00420)	(0.01359)
NoMpa	-1.235	-0.518	-0.01502**	-0.01024
	(1.111)	(1.448)	(0.00383)	(0.00538)
Partisanship × CBI	-0.035	-0.603	0.00382**	0.00863**
	(0.285)	(0.708)	(0.000 9 5)	(0.00356)
Partisanship × NoMpa	-0.212	-0.662	0.00072	0.00363
	(0.276)	(0.586)	(0.00104)	(0.00258)
CBI × NoMpa	0.359	-4.641	-0.00707	-0.11476**
	(1.5 6 5)	(6.138)	(0.00664)	(0.03148)
Partisanship × CBI × NoMpa	-0.064	1.097	0.00276	-0.01372*
	(0.517)	(1.825)	(0.00224)	(0.00791)
Lagged money supply	0.796**	0.794**	0.99860**	0.99905**
	(0.028)	(0.028)	(0.00090)	(0.00091)
Lagged unemployment			-0.00005 (0.00008)	-0.00005 (0.00008)
Lagged Inflation			0.02346 (0.05369)	-0.03031 (0.05092)
Constant	1.914*	0.864	0.05089**	0.0 444 3**
	(0.895)	(1.084)	(0.00841)	(0.00 9 87)
Observations	916	915	1,136	1,135

hypothesis under each of the logically possible structural conditions by calculating conditional coefficients and standard errors. These are reported in Table 8. The results in the upper portion of the table, which are calculated from column C in Table 7, are particularly surprising. As noted above, when the government is expected to have the greatest control over monetary policy (i.e., when the exchange rate is flexible and the central bank is dependent), leftist government leads to a monetary contraction rather than a monetary expansion. The upper left-hand cell, however, shows that when monetary policy is controlled by a highly independent central bank, leftist government is associated with monetary expansion. Thus, the results for the case of flexible exchange rates are exactly the opposite of what we might expect; the sort of differences in monetary policy predicted by the partisan model are found only when the central bank is highly independent. Consistent with our argument about the constraining effects of fixed exchange rates on monetary policy, there is no evidence of partisan differences in monetary policy when the exchange rate is fixed—regardless of central bank independence.

The results calculated from column A of Table 7 also

are not very supportive of the partisan hypothesis. As predicted, there is no evidence of partisan differences in monetary policy when the central bank is independent, the exchange rate is fixed, or both. Yet, there is no evidence of partisan differences when they should be most readily observed—when the exchange rate is flexible and the central bank is dependent. To summarize, the only indication that leftist governance results in an expansion of the money supply occurs when the central bank is highly independent, and this result is not robust with respect to the choice of indicator for the dependent variable.

The fiscal policy results also have idiosyncrasies that are not consistent with the standard partisan argument. Table 9 presents regressions that parallel those we reported in the opportunistic business cycle case. Note that columns A and C assume full capital mobility during the period, but columns B and D relax this assumption. Because, in the presence of the interactive term, the coefficient on the partisanship variable indicates the effects of partisanship when the exchange rate is fixed, our expectation from hypothesis 5C is that this coefficient should be positive. We also anticipate that this effect should disappear when the exchange rate is

Central Bank	Exchang	je Rates
Independence	Flexible	Flxed
Calculated from column	C in Table 7	
Hlgh	0.00192** (0.00075)	-0.00012 (0.00214)
Low	-0.00190** (0.00067)	-0.00118 (0.00077)
Calculated from column	A In Table 7	
Hlgh	0.186 (0.169)	-0.090 (0.488)
Low	0.221 (0.224)	0.009 (0.166)

flexible (columns A and C) or when there is an erosion of fiscal policy autonomy (columns B and D). In the first two columns the variable is not significant. There appears to be no evidence of partisan cycles in the OECD data set. In the last two columns, which add domestic fiscal institutions and are restricted to current EU members, partisanship does seem to have an effect, but the sign is the opposite of what the model predicts. Based on hypothesis 5D, the results indicate that a move to the left reduces the size of growth in the debt burden. Moreover, the conditional coefficients reported in column C indicate that this effect goes away when flexible exchange rates are in place.

To be able to interpret these coefficients, we must consider the relevance of the domestic institutions in the regressions. As in the opportunistic cycle results, the conditional coefficients for partisanship are for the absence of both a strong finance minister and budgetary targets. Table 10 calculates the conditional coefficients for partisanship under different domestic institutional configurations when exchange rates are fixed, which is when we expect partisanship to have an effect. When both fiscal institutions are absent, a move to the left decreases the growth of the debt. When targets are in place, however, a move to the left increases the debt, and there is no partisan effect at all under a strong finance minister.

In general, for both monetary and fiscal policy the predictions of partisan cycles are not borne out. In no case is there evidence of partisan cycles consistent with the predictions about the effectiveness of either monetary or fiscal cycles under given structural conditions. A move to the left leads to an expansion in the money supply only in the most unlikely cases, when the central bank is independent or when spending is constrained by negotiated targets. In the fiscal regressions, there are no partisan effects at all with the OECD data set, and the results are the opposite of those predicted with the EU data set.

EXTENSIONS OF MODEL 2: IMPLICATIONS FOR EUROPEAN ECONOMIC AND MONETARY UNION

Our evidence from Europe in the 1980s and early 1990s indicates that opportunistic fiscal cycles are especially prevalent when capital is mobile and exchange rates are fixed. This result has an important implication for the likely conduct of fiscal policy under the Economic and Monetary Union in Europe. Fixed exchange rates and mobile capital will be the norm in all EU countries that adopt the common currency, either in the first wave on January 1, 1999, or thereafter.³⁷

Although preparations for EMU may have reduced fiscal electoral cycles in the 1990s, it is by no means clear that EMU will restrict fiscal cycles. More generally, there is some concern that governments, once accepted into EMU, will have an incentive to renege on their commitment to lower deficits. Excessively high deficits can both increase inflation and depreciate the euro, and a state can hope to gain the political benefits from high deficits under a common currency without suffering the costs as long as the other states maintain their budget discipline. Another concern is that the deficits can become so large that a state cannot hope to pay back its debt without a bailout from the EU. Because the bailout would be paid for by all EU citizens but would reduce directly just the deficit of the country threatening default, states may have an incentive to run larger deficits than they would if the EU did not exist.38

The member governments are not ignorant of this situation. The Treaty of Maastricht explicitly bans any EU bailout of members, and the states have established a procedure both to monitor the participants and punish any defectors under the Stability and Growth Pact to which the European Council agreed in June 1997. The European Commission will monitor the fiscal health of member states and will report to the Council of Ministers on whether, in its opinion, a deficit greater than 3% is "excessive." The Council of Ministers will then decide whether to accept the commission's assessment. If a country's deficits are deemed excessive, the transgressor will be sanctioned. The offender must make a noninterest bearing deposit with the European Commission equal to .2% of GDP plus an additional .1% per 1% over the 3% limit. The entire process should take ten months; if the country has not

³⁷ We want to be clear about one important difference between the EMU case and the more general cases examined in the empirical section of the article. As explained in note 10, when countries control both fiscal and monetary policy, a fiscal expansion under a fixed exchange rate leads to a reinforcing monetary expansion to maintain the exchange rate. With monetary policy controlled by the European Central Bank, however, states face a "fixed" exchange rate, but they do not anticipate a similar expansion of the money supply Therefore, the effect of the fiscal expansion may be less pronounced in EMU countries than in others that still have a central bank that controls the money supply. We thank Fabio Ghironi for thoughtful comments on this issue.

^{**} Elchengreen and Wyplosz (1997) provide a good discussion of the different rationales for the Stability and Growth Pact.

TABLE 9.	The Conditional Effects of Partisanship on Changes In Gross Debt in the 1980s and
Early 1990	

	DeHaan and	Sturm Data*	Hallerberg and v	on Hagen Data ^t
	1982–92 A	1982–92 B	1981 -9 2 C	1981–92 D
Variables of Interest				
Partisanship	-0.02 (0.19)	0.02 (0.17)	−0.87 ** (0.28)	-0.85** (0.30)
Flexible	-0.07 (0.7 9)		-1.56 (1.54)	
Partisanship × Flexible	−0.21 (1.18)		0.62 (0.56)	
No fiscal policy autonomy		0.11 (0.81) -0.47		-3.06 (2.02) 0.91
Partisanship × No flacal policy autonomy		(0.39)		(0.63)
Conditional Coefficients				
Partisanship (Flexible = 0)	-0.02 (0.19)		-0.87** (0.28)	
Partisanship (Flexible = 1)	-0.23 (0.27)		-0.24 (0.79)	
Control Variables				
Intercept	1.10 (0.84)	1.18 (0.86)	6.16** (1. <i>2</i>)	6.43* (1.68)
d Debt _{t−1}	0.48** (0.11)	0.48** (0.10)	0.48** (0.0 9)	0.50** (0.09)
d Unemployment	1.37** (0.21)	1.39** (0.21)	0.06 (0.05)	0.06 (0.05)
d GDP			−0.84 ** (0.12)	−0.82 ** (0.11)
d Debt costs	0.3 4* (0.15)	0.34* (0.15)	0.18 (0.19)	0.19 (0.18)
Government type	-0.17 (0.24)	-0.37 (0.28)		
2–3 party govt.			0.01 (0.69)	-0.23 (0.77)
4-5 party govt. Minority govt.			-0.90 (0.88) -0.21	-1.26 (0.95) -0.50
Strong finance minister			(0.99) -3.72*	(1.09) -2.60
Strong finance minister × Partisanship			(2.03) 0.80 (0.51)	(2.11) 0.53 (0.52)
Negotiated targets			-5.61 ** (1.91)	-5.65 ** (1.60)
Negotlated Targets × Partlsanship			1.87** (0.50)	1.85** (0.52)
N	196	196	168	168

Note: The dependent variable is the change in the ratio of gross debt to GDP. Panel-corrected standard errors are represented in parentheses. *p < 05, * $^*p < 01$.
*20 OECD countries * b14 European Union members.

TABLE 10. Conditional Coefficients for Partisanship with the Hallerberg and von Hagen (European Union) Data Set under Fixed Exchange Rates and Different Fiscal Institutions

Neither targets nor strong finance minister	-0.87** (0.28)
Targets only	1.00** (0.40)
Strong finance minister only	-0.06 (0.44)
p < .01.	

reformed its budget to the European Council's satisfaction within that time, the deposit becomes a fine.39

Whether the pact will deter states from running chronic deficits is debatable, but clearly it will not discourage opportunistic political business cycles because of the timing of its different procedures. Consider Germany, which traditionally has elections in the fall. A government could promote a large fiscal expansion in the spring and summer that leads to large deficits. In principle, the European Commission could immediately inform the European Council and begin proceedings, but in practice the commission is likely to wait until the final annual budget figures are published in March of the following year, it would fear losing credibility if the preliminary figures prove incorrect.40 An additional ten months would pass before fines can be imposed, and only if the government is not taking corrective measures. In other words, any government can push up the deficit shortly before elections so long as it is cut back again after the elections.41

An alternative is for states themselves to place domestic institutional constraints on their spending behavior. Consistent with Hallerberg and von Hagen (1999), our empirical results indicate that either commitment to negotiated budget targets or delegation to a strong finance minister reduces preelectoral fiscal cycles. A move to one of these institutions may be in order. More generally, because the temptation to use fiscal policy for short-term electoral gain will be greater under EMU, states may have the incentive to develop such institutions only if they are sure that the EU will not bail them out. Indeed, based on an analysis of federal systems, Eichengreen and Von Hagen (1996a) argue that a formal bailout rule is not even needed as long as states maintain their ability to raise revenues.

39 The text of the agreement appears in Committee on Economic and Monetary Affairs and Industrial Policy 1997.

40 Personal interviews with two members of Directorate General II,

June and July 1997.

The EU can simply say "no," much as California did in the mid-1990s when Orange County's creditworthiness plummeted. States may then learn through trial and error to introduce more prudent fiscal policy management. A possible precedent again comes from the United States, where fiscal mismanagement eventually led 47 of 50 American states to pass some form of fiscal policy restriction (Eichengreen and von Hagen 1996b). These changes obviously were not driven by concerns about what state budget deficits could do to the value of the dollar, but by "domestic" concerns about the effects on home populations. These institutional rules also seem to have benefits: States with tighter antideficit rules as well as restrictions on the authority of the legislature pay lower interest rates on their bonds (Poterba and Rueben 1998).42

CONCLUSION

Our model makes fairly weak assumptions about the preferences of monetary and fiscal agents but very strong assumptions about the institutional constraints under which they operate. The strong assumptions about the Mundell-Fleming conditions are intended in part to make the analysis tractable, but they have the added benefit of stacking the cards in favor of falsification: If the predictions based on such a stark model withstand provisional tests, then there are good reasons to believe further refinement will be fruitful. Indeed, the empirical results support the insights of the model. Preelectoral monetary expansions are likely in states with mobile capital, a flexible exchange rate, and a dependent bank and are absent otherwise; preelectoral fiscal expansions are likely under mobile capital and fixed exchange rates.

These results have several interesting implications for the relationship between institutions and the international environment. In the presence of mobile capital, the exchange rate regime constrains the ability of policymakers to use one of the two macroeconomic instruments before elections. When governments change their exchange rate regime, one should expect a change in the incumbents' behavior before an election. If a government fixes its currency when capital is mobile, for example, one would expect a shift from monetary to fiscal policy instruments. It can be argued that such a shift is occurring in Britain. Under Stage III of EMU, EU members anticipate the eventual creation of Exchange Rate Mechanism II for countries that do not join the common currency. The expectation is that the band around the euro for these currencies will progressively narrow, so that these countries will also experience essentially fixed exchange rates. Preelection monetary expansions will become less effective than fiscal expansions.

⁴¹ One additional tool that could be used to restrict governmental abuses will also be absent under EMU. In this article we have assumed that the decisions to pursue capital mobility or fixed exchange rates are exogenous. One could argue, of course, that when banks are independent and governments are especially fiscally irresponsible the bank may decide to stop defending the currency and simply let it float, putting the country in the flexible exchange rate capital mobility scenario that is advantageous to the bank. Under EMU, however, this is extremely unlikely.

⁴² The ultimate form these institutions should take is still open to discussion Strauch (1998) finds that numerical restrictions on the size of the deficit do restrict the size of the deficit. Von Hagen (1998) notes, however, that such numerical restrictions can merely encourage politicians to be more creative with their accounting, and he suggests more centralized procedures to ensure that someone monitors the actors.

There also are implications for the selection of institutions by governments. When considering the tradeoffs involved in enhancing central bank independence, a government may consider what it gives up in terms of survival in the next election if it relinquishes monetary policy autonomy in exchange for greater general price stability with a more independent central bank. A government in a flexible exchange rate economy will lose its ability to influence outcomes with macroeconomic policy before an election if it makes the bank more independent. If the government moves the country to a fixed exchange rate regime, however, it can gain some price stability and still maintain the ability to manipulate the economy before an election. Our work suggests, for example, that Chancellor of the Exchequer Brown would have lost little maneuverability for his government before the next election by granting the Bank of England greater independence in May 1997. The government presumably can initiate a fiscal expansion before the next election so long as Britain has joined Exchange Rate Mechanism II by then. Counter to Bernhard and Leblang (1999), in the absence of central bank independence, governments that peg their exchange rates when capital is mobile do not sacrifice their ability to pursue electoral goals. Instead, they change the instrument with which they can do so.

Our study suggests why previous empirical research on fiscal cycles has yielded ambiguous results. Few studies consider the effects of different exchange rate regimes on the likelihood of fiscal cycles. States engineer fiscal expansions when exchange rates are fixed but not when they are flexible, and works that do not include the exchange rate regime are missing this critical variable. One article that does consider the exchange rate factor, by Clark and Reichert (1998), appears to contradict the results reported here. The authors find that states with either fixed exchange rates or independent central banks are less likely to experience opportunistic political business cycles. Yet, they concentrate on outcomes, such as the unemployment rate and economic output, rather than on policy instruments, such as the money supply or budget balances. Our research produces an interesting puzzle: If fiscal cycles exist when monetary policy is constrained by fixed exchange rates or a highly independent central bank, why do Clark and Reichert find no evidence of cycles in unemployment or growth under precisely these conditions? A possible explanation is that politicians use monetary policy before elections to affect the general economy but manipulate fiscal policy to sway specific constituencies. Indeed, although an increase in the money supply may help certain groups—such as home buyers—more than others, it is a blunt instrument for cultivating specific clienteles. Fiscal policy, in contrast, is more suited to targeted use, whether through greater spending, tax cuts, or both. The implication is that these different strategies have markedly different macroeconomic consequences.

Finally, it is clear that the choice of exchange rate regime and the degree of capital mobility have complex effects on the ability of politicians to influence the economy for electoral purposes. Our study does not necessarily contradict the growing literature on the effects of increased capital mobility on national policy autonomy, but it shows that the effects of capital mobility vary a great deal across cases, both because they are refracted by the institutions through which they are transmitted, such as central banks, and because exchange rate regimes interact with capital mobility in important ways.

APPENDIX A. DERIVATION OF PROPOSITION 1

The reaction functions in proposition 1 follow directly from the actor's loss functions and the Phillips curve mechanism. The government's problem is to choose g so as to minimize its loss function. If we substitute in the Phillips curve process of equation 4, which determines y, and the right-hand side of equation 2 for the government's ideal point for output, and if we assume (without loss of generality) that $\pi^{\bullet}=0$, the loss function becomes:

$$L_1 = (y^n + \mu(\pi - \pi^e) + \phi g - k^{\mu\nu\nu}y^n)^2 + \alpha \pi^2.$$
 (A-1)

To find the minimum, we take the partial derivative with respect to g:

$$\frac{\partial L}{\partial g} = 2\phi(y^{n} + \mu(\pi - \pi^{n}) + \phi g - k^{n}y^{n}), \quad (A-2)$$

which, when set equal to zero and solved for g, becomes

$$g = \frac{1}{\phi} [y^{n} (k^{p \cdot r} - 1) - \mu(\pi - \pi^{r})]$$
 (A-3)

as proposed.

An analogous process can be used to determine the central bank's optimal response. The problem is to find the level of inflation that minimizes its loss function. Once again, substituting the Phillips curve process of equation 4 as well as the right-hand side of equation 3 for the bank's ideal point for output, and assuming (without loss of generality) that $\pi^* = 0$, the bank's loss function is:

$$L_{ab} = (y^n + \mu(\pi - \pi^n) + \varphi g - k^{ab}y^n)^2 + \alpha \pi^2$$
. (A-4)

Differentiating with respect to π yields:

$$\frac{\partial L}{\partial \pi} = 2\mu (\mathbf{y}^{\mathbf{a}} + \mu(\pi - \pi^{\mathbf{a}}) + \phi \mathbf{g} - \mathbf{k}^{\mathbf{c}\mathbf{b}}\mathbf{y}^{\mathbf{a}}) + 2\alpha\pi, \quad (\mathbf{A}-\mathbf{5})$$

which, when set to zero and solved for π , becomes

$$\pi = \frac{1}{\mu + \frac{\alpha}{\mu}} [\mu \pi^{e} + y^{e}(k^{cb} - 1) - \phi g] \qquad (A-6)$$

as proposed.

APPENDIX B

We will briefly describe indicators used in the empirical section. These fall into two categories: institutional/political and macroeconomic.

Institutional/Political Variables

Given the qualitative nature of our theoretical discussion, we elected to classify our observations primarily according to

TABLE B-1. Variables	Classification	n of Struct	ıral
		Mobile	Mobile
		Capital and	Capital and
		Fixed	Flexible
	Independent	Exchange	Exchange
Country	Central Bank	Rates	Rates
Australla	Yes		1973 -9 2
Austria	Yes	1973 -9 2	
Belglum	No	1973 -9 2	
Canada	No		1973 -9 2
Denmark	Yes	1973 -9 2	
Finland	No	1977– 0 2	1973–76
France	No	197 9–9 2	1973–78
Great Britain	No	1991– 9 2	1973 -9 0
Greece	Yes		1973–92
Iceland	n/a	1981– 9 2	
ireland	Yes	1973 -9 2	
Italy	No	197 9-9 2	
Japan	No		1973 -9 2
Luxembourg	n/a	1973 -9 2	
Netherlands	Yes	1973 -0 2	
Norway	No	1973 -9 2	
New Zealand	No		1981– 9 2
Portugal	No		1973– 9 2
Spaln	No	1989 -9 2	1973–87
Switzerland	No	1981– 9 2	
Sweden	No	1973– 9 2	
United States	Yes		1973 -0 2
W. Germany	Yes		1973 -9 2

categorical distinctions. Central banks are viewed as independent or not; exchange rates are fixed or flexible. We have neither strong theoretical arguments about such operational choices nor reason to believe that the strategic calculations of policymakers are influenced in a linear fashion by changing degrees of capital mobility or central bank independence. Table B-1 reports the classifications we used.

Central Bank Independence: The Cukierman, Webb, and Neyapti (1992) measure of legal independence was used to construct a categorical variable that equals 1 if the country's score is above the sample median, 0 otherwise.

Elections: For monetary tests, quarters in which a general election is held, and the three preceding quarters, are coded as electoral periods. Dates for elections are taken from Mackie and Rose (1982) and Europa World Year Book (various years). For fiscal tests, we used either the year in which an election was held, with the data provided in Hallerberg and von Hagen (1999) and based on the yearly data appendix in European Journal of Political Research (various years), or the percentage of time in a given year before an election for a body important for macroeconomic policy as coded by Franzese (1996) and updated by the authors May 1998. (More details for this data source are provided in note 33.)

Fixed Exchange Rates: This categorical variable equals 1 when a country either belongs to a fixed exchange rate regime or pegs its currency to another national currency, 0 otherwise. Codings are based on the IMF's Exchange Arrangements and Exchange Restrictions (various years a).

Fiscal Institutions—Strong Finance Minister and Negotiated Targets: These variables are coded 1 when the institution is present, 0 otherwise. Strong finance ministers generally serve as agenda-setters on the budget, have monitoring functions over the budgets of other ministries, and can strike

out spending on some occasions when it is deemed excessive. Negotiated targets are present when coalition partners negotiate budgets for every ministry. The alternative, termed the "fiefdom" approach, is for coalition partners to negotiate the distribution of cabinet portfolios and cabinet meetings among all ministers who determine the budget. Data appear in Hallerberg and von Hagen (1999).

Left: This variable is the percentage of cabinet seats held by leftist parties. It is coded from the data on cabinet compositions in Woldendorp, Keman, and Budge (1993) and from the yearly data appendix in European Journal of Political Research (various years).

Government Type: Roubini and Sachs (1989) code this variable as follows: 0 = one-party majority government, 1 = 2-3-party majority government, 2 = 4-5-party majority government, 3 = minority government. Our data for this variable come from De Haan and Sturm (1997), who update the data set of Roubini and Sachs. Edin and Ohlsson (1991) break this variable into three separate dummies. Hallerberg and von Hagen (1999) use this formulation, and we received the data which are based on the yearly data appendix in European Journal of Political Research (various years), from them.

Partisanship: Data for this variable come from Woldendorp, Keman, and Budge (1998). The coding they use is for the political complexion of parliament and government. The values range from 1 for right-wing dominance to 5 for left-wing dominance. Note that they do not have data for Greece, Portugal, Spain, and the United States. With the exception of Portugal, these countries have governments that are clearly on the Left or Right, and we code them ourselves (1 if a right-wing government was in place, 5 if a left-wing government was in place).

Macroeconomic Variables

Change in Debt Costs: The change in debt servicing costs is computed as the change in the real interest rate minus the change in the growth rate times the gross deficit in the previous year. Real interest rate data for the De Haan and Sturm regressions came from OECD Economic Outlook (various years a), while the data for the Hallerberg and von Hagen regressions came from Statistical Annex of European Economy (various years).

Change in Gross Debt: This variable is expressed in terms of gross government debt over GDP. The data for the De Haan and Sturm regressions came from OECD Economic Outlook (various years a), and the data for the Hallerberg and von Hagen regressions came from Statistical Annex of European Economy.

Growth: For monetary tests, growth in output was measured by seasonally adjusted total industrial production, except for Canada (where data were not seasonally adjusted) and Australia (where seasonally adjusted real gross product volume was used), from OECD Main Economic Indicators (various years b). Other studies have used change in real GDP, but seasonally adjusted quarterly data were unavailable for several countries. For fiscal tests, figures for the De Haan and Sturm regressions came from OECD Economic Outlook (various years a), and data for the Hallerberg and von Hagen regressions came from Statistical Annex of European Economy (various years).

Inflation: This variable was based on the consumer price index for all goods, except for Japan (all goods less food). It was calculated as $(Cpi_t/Cpi_t - Cpi_{t-4})$. The source was International Monetary Fund, International Financial Statistics, CD-ROM version.

Long-Term Interest Rates: Data for the De Haan and

Sturm regressions came from OECD Economic Outlook (various years a), and data for the Hallerberg and von Hagen regressions came from Statistical Annex of European Economy (various years).

Money Supply: M1 and M1 + quasimoney came from OECD, OECD Main Economic Indicators (various years b). Unemployment: For monetary tests, the quarterly percent-

Unemployment: For monetary tests, the quarterly percentage change in the seasonally adjusted number of unemployed was used, from OECD, OECD Main Economic Indicators (various years b). For fiscal tests, unemployment figures for the De Haan and Sturm regressions came from OECD Economic Outlook (various years a), and data for the Hallerberg and von Hagen regressions came from Statistical Annex of European Economy (various years).

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Reflections on Fear: Montesquieu in Retrieval

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ccording to most scholars, Montesquieu argues that fear threatens a loss of self. Disconnected from the exercise of reason, fear is an emotion that is supposed to prevent the individual from acting with any kind of moral or rational agency. Fear is also premised on the liquidation of civil society; intermediate institutions and plural social structures are destroyed so that despots can act with unmitigated power and violence. I argue that this view does not capture Montesquieu's theory. In my alternative account, fear is intimately connected to our capacity for reason and to our sense of self. It is built on a network of elites, the rule of law, moral education, and the traditional institutions of civil society. I conclude that twentieth-century social science remains too indebted to conventional interpretations of Montesquieu's views, and contemporary theorists would be better served by the alternative analysis proposed here.

ear," wrote Raymond Aron (1968, 20-1), "needs no definition. It is a primal, and so to speak, subpolitical emotion." Many might agree. Living in a liberal democracy, we do not celebrate fear's entrance into the political sphere; it signals a rush of unwelcome emotion. Fear is allowed to lurk at the outer limits of modernity (Bouwsma 1980; Naphy and Roberts 1997) or to stalk the political periphery.1 But in our own midst, fear is supposed to be confined to right-wing militias, apocalyptic religious sects, or some other manifestation of "the paranoid style in American politics" (Hofstadter 1964). We segregate fear by the boundaries of time and space, relegating it to the occasional episode of collective anxiety or to subcultures of eccentric conviction on the far Right and Left. When these boundaries blur, when fear disrupts the quiet hum of institutional politics, we treat it as an emotional upsurge of the untutored and the untamed (Bell 1963; Bennett 1988; Brownstein 1994; Cantril 1940; Davis 1960, 1971; Lipset and Raab 1971).

This view of fear as a denizen haunting civilization is relatively recent. It is rooted in a selective reading—and misreading—of Montesquieu's famous analysis of despotism. Almost since the time that Montesquieu wrote *The Spirit of the Laws*, his followers and critics have interpreted him as arguing that fear is a totalizing emotion that strips individuals of their capacity for reason or any other motivation besides fear. Fearful men and women cannot conceive any particular goals or goods beyond mere survival. They cannot reason about means and ends. They have none of the attributes of selfhood. Fear is an experience unto itself, a wholly primitive passion that can be reduced to the apprehension of raw, unmediated, physical danger (Aron 1968, 20–1; Cohler 1988, 40; Hulliung 1976, 119,

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¹ Three recent analysts, for example, title their study of state terror in Latin America *Fear at the Edge* (Corradi, Fagen, and Garretón 1992).

122; Keohane 1980, 399; Shackleton 1961, 270; Shklar 1987, 83–5). Despotic fear preys upon a society in which the rule of law is weak or nonexistent, political power is monopolized in the hands of a single and arbitrary ruler, there are no secondary or mediating institutions, and what we would now call civil society is entirely absent. Arising out of a sociological void, fear finds its ideal environment atop a flattened plain of atomized particulars (Althusser 1972, 79; Boesche 1990, 743–9; Richter 1973, 9; 1995, 338; Shklar 1987, 85; Venturi 1963, 134–6; Wolin 1989, 102–4, 106, 108–9; Young 1978, 398–9).

I challenge this account of fear, both as it pertains to Montesquieu's own views and as a coherent theory in its own right. The assumptions that fear strips the self of reason and other emotions and that it thrives on sociological and political simplicity can be found in Montesquieu's work, but they obscure an alternative analysis whereby fearful men and women do not conform to the terrorized stereotypes of totalitarian lore. In this alternative account, fearful individuals do not lose all capacity for rational agency. As rational beings, they attempt to maximize their interests, albeit in bad circumstances. They are not fearful because they have been stripped of their humanity but because they desperately cling to it in the face of appalling choices. They are afraid because they fear the loss of some desired good or the frustration of some envisioned end. Selfhood is thus a precondition for fear.

Montesquieu also argues that despotic fear can coexist—and even depend upon—mediating institutions, the rule of law, and a dense civil society. In his most searching and incisive accounts of fear, he dispenses with the now familiar trope of the violent despot laying waste to all social formations beyond his own limited retinue of vizirs and slaves. Instead, Montesquieu envisions fairly complicated political arrangements, including concentric circles of elites, separate spheres, and pluralist institutions, all of which the despot manipulates—rather than destroys—and presses into his service. Despotic fear, in this alternative account, emerges as an intricate political development generated and sustained by diverse social practices.

In the first section of the article, I set out the standard account of Montesquieu's theory of despotic

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fear. In the second section, I explore an alternative account of that theory, in which fear is shown to be reconcilable with rationality and selfhood, and flourishes within a highly complex and socially stratified civil society. I argue that fear tracks the individual calculations men and women make regarding their own interests, and it rises and falls in response to their evaluations of the costs and benefits of pursuing one course of action over another. At a political level, fear preys upon the relationship between elites and their followers. Far from destroying these social connections, the despot depends upon them as auxiliaries to his own power. In the final two sections, after assessing the legacy of the standard interpretation of Montesquieu's theory of fear, I show how my alternative analysis may better serve us, both theoretically and practically.

THE TRADITIONAL ACCOUNT OF DESPOTIC FEAR

According to most scholars, Montesquieu offers the following account of despotic fear. Fear is a "physiological reaction" of the self to excessive physical force. It is an "involuntary," uncontrollable response to threatening power. Fear is rooted in an immutable physical reality—our physicality as sentient beings and the physicality of what threatens us—which leaves us no choice but to fear those intimidating objects that confront us. Fear is so unalterable precisely because it is so physiological. There is little room for individual agency or judgment in determining what we fear or how we respond to fear. Despotic fear is not mediated by our desires or our values, nor do they influence how we cope with fear. In the face of physical cruelty, men and women can only react in a prescribed, automatonlike fashion. "This is where our physical and moral impulses meet and struggle, and where the former triumph" (Shklar 1987, 84-5). This kind of intensely physical fear usually induces paralysis. Instead of flight, withdrawal, or crafty evasion, the fearful individual stands frozen, mutely surveying the horror that awaits (Althusser 1972, 80–1; Cohler 1988, 71–5; Shackleton 1961, 270).

Fear takes over our entire selves. It is immoderate. It induces a "permanent state of foreboding" (Shklar 1987, 84), which makes it nearly impossible for us to experience any other emotion. The fearful person cannot love, desire, reflect, or imagine. She is incapable of honor or virtue. Fear is "far too imperious" for other feelings, thoughts, or aspirations. Because fear is "so tyrannical a passion" (p. 84), its victims can only experience fear. Among the fearful, then, one will find a dull sameness. Incapable of feeling or doing anything but fear, the fearful do not exhibit differences among themselves. They are not really individuals at all. They lack any vestige of those things—personal idiosyncrasies, tastes, spontaneity, character—that make an individual an individual (Shklar 1987, 84; also see Boesche 1990, 745; Richter 1973, 9; 1977, 47; Todorov 1993, 362–3; Wolin 1989, 108–9; Young 1978, 399).

From these basic characteristics—fear's intensity

and extremity; its physiological dimensions; its unthinking, unreflexive, automatic qualities; its capacity to eliminate all vestiges of rational agency and personality; its antiindividualist tendencies—one can deduce the type of politics that would give rise to it. Because fear is aroused by intense physical force and excessive violence, despotic governments are designed to unleash that force and violence against hapless victims. Concentrated power is crucial. Separated power not only diminishes the amount of power a despot can wield but also helps the despot's victims resist (Cohler 1988, 40; Keohane 1980, 401–2; Richter 1973, 9; Shklar 1987, 85-6). Despotic power must not be constrained by any rule or law. It must be arbitrary because fear is aroused by the sudden, whimsical cruelty of the despot. If the despot's victims were able to adjust their actions to avoid his wrath, they would cultivate those spaces or zones of security that limit fear (Aron 1968, 25; Cohler 1988, 71-5; Keohane 1980, 406; Richter 1995, 338; Young 1978, 401). For the same reason, no moral principle should check the exercise of power. Even concentrated, intense, and cruel power can be made less fearful when it is subject to moral strictures. Thus, despotic power dispenses with all appeals to moral legitimacy. It is founded only on coercion and the fear that coercion arouses (Hulliung 1976, 45; Richter 1977, 47; 1995, 338).

These political sources of despotic fear have a sociological corollary: the absence of organized forms of association within civil society. To maximize its fearsomeness, despotic power seeks to isolate every individual from everyone else. Isolated men and women are powerless. They lack the concerted resources of a community to help them resist despotic power and thereby overcome paralyzing fear (Aron 1968, 25–6; Richter 1973, 9; Young 1978, 402). Despotism destroys nonstate institutions and social classes because their mere existence poses a challenge. In other words, despotism seeks to eliminate not only organized political forms of opposition but also all forms of social cohesion, even when they present no formal challenge to its authority.

Of special concern are the influential classes—in Montesquieu's day, the aristocratic elites whose authority had been curtailed by Louis XIV. Regardless of the views of these elites, regardless of whether they see themselves as adjutants or opponents of centralized power, their mere existence is threatening. Despotic power inevitably aligns itself against the upper classes, leveling society, eliminating textured social formations so that it can apply its power with lethal effect anywhere and everywhere. Sociological complexity and pluralism are thus the inherent enemies of despotism (Althusser 1972, 69-79; Berlin 1980, 155-8; Berman 1970, 10-1; Keohane 1980, 398, 407; Neumann 1957, 111-2; Wolin 1989, 108-9). At its core, despotism thrives on cultural primitivism and social simplicity; the "unplotted conspiracy of differences" gives way to ignorance, barbarism, and homogeneity (Wolin 1989, 107; also see Boesche 1990, 743-9; Hulliung 1975, 39-40; Richter 1973, 9; 1995, 338; Shackleton 1961, 270).

This scholarly account of Montesquieu's theory of despotic fear has been influential for two reasons. First, as I shall explain later, it has been adopted and adapted by many political theorists as a coherent analysis of fear in its own right. Influenced by their own preoccupations with the various tyrannical regimes that have disfigured the twentieth century, many theorists have found in Montesquieu a valued source of reflection and citation. Second, his texts, particularly *The Spirit of the Laws*, provide much evidence for such interpretations.

Montesquieu formulated his theory of despotic fear with the avowed polemical purpose of rousing elite opinion in France against creeping royal absolutism. If he could depict a form of fear so absolute that it destroyed all that was good and valued, then he might establish an indisputable moral rationale against those features of French rule that smacked of despotism: political centralization, arbitrary power, and the destruction of traditional privileges. Montesquieu's account was thus meant as a contribution to a tradition of constitutional rhetoric, a rhetoric of defense-sometimes aristocratic, sometimes liberal-against encroaching royal power (Aron 1968, 26; Keohane 1980, 403, 407; Koebner 1951, 293-302; Neumann 1957, 111-2; Ranum 1969, 609-12, 617-22; Richter 1973, 7-9; 1977, 31-2, 45-6; 1995, 334-7; Shackleton 1961, 15-7, 202, 226-8; 1988, 239; Shklar 1987, 2-5, 18-9, 69, 74-5, 79-81, 85; Venturi 1963, 134-6; Young 1978, 404-5; for a contrary view, see Hulliung 1976).

This conviction was reflected in Montesquieu's writing and activity throughout his adult life. It did not flag with time, and it found its fullest expression in The Spirit of the Laws (Shklar 1987, 67-9). But there was a cost to his boldness: In Montesquieu's late masterpiece, polemic occasionally managed to eclipse analysis. In his desire to launch a full-scale broadside against French absolutism, Montesquieu sometimes lost sight of his more subtle and often more piercing insights about despotism. He overstated his case about fear's self-sustaining and all-encompassing dimensions, a failing instantly noted by his critics. Inspired by his own and equally polemical support for enlightened absolutism, Voltaire remarked that Montesquieu "satirizes more than he judges." He "makes us wish that so noble a mind had tried to instruct rather than shock" (Voltaire [1764] 1962, 508). Montesquieu's most dramatic account of despotic fear did not reflect Montesquieu at his most searching or probing but, rather, the liberal imagination at its most politically engaged and morally aroused. It is no accident, then, that scholars have seized on this portrait of despotic fear in The Spirit of the Laws. It has a tremendous rhetorical force, but a force that can obscure as much as it reveals.

MONTESQUIEU'S ALTERNATIVE ANALYSIS OF DESPOTIC FEAR

I challenge this traditional account of Montesquieu's theory through a close reading of his *The Persian Letters*, published more than twenty-five years before *The Spirit of the Laws*. Among Montesquieu scholars there is an overwhelming consensus that the earlier

work prefigures the later, particularly in its analysis of despotic fear (Boesche 1990, 742; Hulliung 1976, 138; Kra 1963, 11; Neumann 1957, 102; Richter 1973, 9; 1977, 45; Shackleton 1961, 45; Shklar 1987, 67; Todorov 1993, 353). To some degree, that consensus is justified. As I have suggested, Montesquieu had a lifelong concern about the effects of despotic fear. The Spirit of the Laws was a more mature, more radical, more comprehensive indictment of a phenomenon he had been decrying since the beginning of his writing life. The same intention—to construct a portrait of despotism in the East in order to cut short its career in the West—animated both The Persian Letters and The Spirit of the Laws.

If one reads the earlier work carefully, however, one can piece together a different account of despotic fear from that discussed in the previous section. In The Persian Letters, we discover a fear that works most effectively when it is wielded by multiple political actors and elites, each inspired by different considerations and motivations and even conflicting interests. We find a regime structured on a comprehensive system of moral education, rational calculations of self-interest, overweening personal ambition, and fairly traditional ideas of virtue, duty, law, and morality. The victims of despotic fear are not incapable of familial love and affection. In fact, affection and mutual commitment finance the transactions of the despotic economy. In depicting these various elements of despotism, Montesquieu manages to capture in brilliant detail the intricate social universe of the Versailles court, with all its hierarchy and flattery, its conniving ambition, and its penchant for absolute submission.

To be sure, the distinctions between the two works should not be overstated. Similar evidence can also be found in The Spirit of the Laws. For instance, in the later work, Montesquieu occasionally suggests that despotic fear does not entirely destroy rational calculations and all forms of reason. The victims of despotism, after all, can build houses that last at least a lifetime (Montesquieu [1748] 1988, V.14.61). They are savvy enough to keep silent in the presence of the despot (V.14.60). What Montesquieu seems to be pointing to here is a limited kind of rationality that undergirds despotism. Responding to short-term needs, the victims of despotism act rationally by adjusting their actions to secure the immediate goal of survival. But their quick-fix mentality leads them to reproduce the conditions that keep them in perpetual thrall. They cannot see how their actions, designed to respond to short-term needs, undercut their long-term interests: "When the savages of Louisiana want fruit, they cut down the tree and gather the fruit. There you have despotic government" (V.13.59). Silence may buy protection in the short term, but it sustains the despot's power, which threatens survival over the long term. The victims of fear are not without reason. They merely cannot escape the conundrums of a limited instrumental rationality.

These bits of evidence can be found in *The Spirit of the Laws*, but they are often eclipsed by more dramatic portraits of total terror. Fear is held to be so omnipres-

ent in despotism that "there is neither honor nor virtue" (V.17.67). "How could honor endure the despot" (III.8.27)? Fear makes it impossible for individuals to experience other sensations or thoughts beside fear. The fearful cannot love, desire, or hope. One cannot appeal to their sense of loyalty or their commitments to others because "it is useless to counter with natural feelings, respect for a father, tenderness for one's children and women, laws of honor, or the state of one's health" (III.10.29). Fear strips a person of all worldliness. To instill fear in someone, "one must take everything away" (IV.3.35). "In despotic countries one is so unhappy that one fears death more than one cherishes life" (VI.9.82). A person's fear of death, in other words, has little or nothing to do with the things that make life worth living. Fear has been disconnected from those ends or purposes that make one's life one's own. Fear no longer stands in any proximity to the constituent elements of the self. It annihilates the self. To understand fear—both at the individual level and in a larger society—we need not understand the familiar passions of human nature or the normal rules of politics but, rather, what happens when those rules and passions disappear. In describing Montesquieu's theory of fear, scholars have resorted to such phrases as "void," "desert," "subpolitical," the "social equivalent of death" (Althusser 1972, 79; Aron 1968, 21; Shklar 1987, 85) metaphors that reflect, almost unselfconsciously, a certain tendency in Montesquieu's own thinking about fear. Montesquieu himself uses the word "monstrous" to describe despotic regimes (III.9.28), an allusion both to the cruelty of despotism and to the notion that fear belongs to a world entirely different from our own.

In The Persian Letters there is a more discerning and subtle account of despotic fear, an account that allows for a fuller range of emotions, sympathies, and distributions of power than Montesquieu's later account suggests. One reason for this distinction between the two works may be attributed, as I have argued to Montesquieu's increasing confidence and radicalism in his mature indictment of royal absolutism. Less constrained in his vision and imagination, Montesquieu was able to give voice in The Spirit of the Laws to his most blistering antipathies and far-reaching aspirations. But as those antipathies came into their own. they assumed a more aggressive, and occasionally more tendentious, shape. We must turn to the earlier work if we are to discover a less polemical and more discerning account of fear.

The Persian Letters is the story of Usbek and Rhedi, two Persian gentlemen who leave their homes and families to travel to France in search of knowledge. In their travels, they encounter many novel ideas and practices, and in a series of letters they recount these pieces of exotica to each other and to their Persian friends. Usbek has a harem of wives and eunuchs back in Persia, and a chief subtext of the novel is the parallel between his tyranny at home and the tyranny he finds abroad. The harem is not a full-blown regime of the sort that Montesquieu describes in The Spirit of the Laws, but it offers a portrait in miniature of the

micropolitics of despotism. Through the literary conceit of the harem, Montesquieu provides a concrete and vivid account of the underlying structures and passions of despotic fear.

Despot

The first challenge that *The Persian Letters* poses to our standard interpretations of despotic fear concerns the figure of the despot himself. For many scholars, the despot is a simple creature of desire with a solipsistic temperament and an insatiable thirst for violence or instant gratification (Althusser 1972, 82; Behdad 1989; Boesche 1990, 749; Cohler 1988, 71–2; Shklar 1987, 83–5). In *The Spirit of the Laws*, Montesquieu provides much evidence for such interpretations. The despot rules, he writes, "without law" because restrictions of any sort would prevent him from fulfilling his whims and from acting capriciously and arbitrarily (II.1.10).

Usbek, however, is a gentleman and a scholar. He is by turns an intellectual seeking wisdom and truth, a charming husband surrounded by loving wives, a faithful friend, and a teacher admired and respected by his students. He also happens to be a purveyor of fear. One moment he is speculating about the foundations of international law, the next he is ordering his servants to terrorize his wives. He aspires to a pluralist vision of the universe, claiming that no one set of principles is objectively superior to another, but at the same time he forces his wives to accept a regime of moral purity. He speaks with the measured tones of a moderate skeptic but acts with the conviction of a fanatic. He is a humanitarian and a rapist, a rationalist and a terrorist.

In explaining his decision to leave Persia, Usbek writes: "Rica and I are perhaps the first among the Persians who have been moved by a desire for knowledge to leave their country and to give up the savors of a peaceful life that they might go seek wisdom the hard way" (Montesquieu [1721] 1961, letter 1, 47). Usbek is an intellectual. He seeks wisdom, and he willingly disrupts the routines and comforts of his life to get it. He is committed to the strenuous search for truth, not the easy contemplation of ideals. He is convinced that one finds truth only by transcending the limits of one's own parochial world. "We were born in a flourishing kingdom," he says about himself and Rica, "but we did not believe that its borders should be those of our knowledge nor that Oriental insight alone should enlighten us" (letter 1, 47). He is a prototypical representative of one strand of the Enlightenment not a Condorcet or Helvetius (i.e., a rationalist who submits to the severities of reason) but a chastened skeptic, aware of the world's multiplicities and diversities and impatient with monistic models of the truth (Shklar 1987, 26-7, 30).

Usbek is also a moralist. Like all moralists, he finds his virtue tested by the demands of politics. He explains that early in his life he took part in the affairs of state and struggled valiantly to hold onto his principles. He sought to expose lies, counter flattery and vice, and speak truth to power. At the royal court, he "spoke there a hitherto unknown language" (letter 8, 54) but

gained more enemies than converts. Like so many moralists who founder on the rocks of compromise and expedience, Usbek beat a hasty retreat from politics. He retired to a country home, turned to his books, and set up shop as a gentleman scholar (letter 8, 54). He does not, however, live in isolation. He retains a circle of friends and colleagues, learned men who argue about politics and morality. He is the presiding eminence of this circle, respected for his rare wisdom. He teaches a modified Aristotelianism, arguing that men are by nature virtuous, that they find their utmost happiness in leading a virtuous life (letter 10, 58). Despite his exemplary virtue, Usbek is ill at ease in the world of religion. He is too much a doubter, too much a skeptic, to be a believer. His religious commitments are challenged by a scientific bent and a quasi-Lockean epistemology.

Usbek stands for everything that, according to received wisdom, is opposed to despotic practice. He is wise, learned, lawful, and moral. Moreover, he is not dogmatic. He displays an openness, a willingness to engage with the strange and new, and he has a facility for understanding and penetrating beneath the surface. He delights in the diversity of modern life, and he scorns Rhedi's primitivism and obsession with Spartan virtue. In a fascinating exchange of letters, Usbek and Rhedi anticipate the debate Rousseau would later inspire with his First Discourse. Rhedi tells Usbek that progress and civilization have meant only that science has produced more deadly forms of warfare and that artistic excellence has corrupted popular morals. A return to Spartan virtue would serve society better than would continuing cultural progress. Usbek issues a powerful rejoinder on behalf of the Enlightenment: "Have you ever reflected upon the unhappy and savage condition into which the loss of the arts would drag us?" Instead of bemoaning scientific progress, says Usbek, Rhedi should be advocating international arms control and treaties. Gunpowder and modern weaponry have made military battles "much less bloody than they were before, because there is practically no direct engagement now." He challenges Rhedi's association of cultural simplicity with physical fortitude. The Greeks, after all, "conquered and subjugated [the Persians] so many times" but nonetheless "cultivated the arts with infinitely more care than [the Persians]." Usbek praises dynamic, open societies, in which commerce and culture promote a more humane existence. He condemns Persian slavery because it promotes "an eternal lethargy." Against this crippling society, he envisions a society of "abundance and industry" (letter 17, 68-9; letters 105-6, 195-9; letter 115, 210-2).

Despite his openness and embrace of cultural diversity and pluralism, Usbek is very much a despot. He castrates men so that they will serve him more faithfully. At the slightest sign of disobedience or challenge from his wives, he orders the eunuchs to punish them harshly. He will not tolerate disagreement, and he does everything he can to ensure that his universe is ordered in accordance with his principles. How is it possible that this learned, skeptical individual could preside over such a regime of fear?

Usbek's capacity for compartmentalizing his life into separate spheres—the brilliant intellectual, the political moralist, the vicious husband—is one key to his despotism. He does not demand the absolute reconciliation of opposing principles. Unlike the stereotypical rationalists of the Enlightenment, Usbek tolerates contradictions and accepts the inherent plurality of disparate worlds. But it is precisely this genial tolerance of other cultures that facilitates his exercise of violence at home and his speculation about peace abroad. He can dominate his wives and advocate tolerance because the harem demands a different cultural logic than does the Persian court. In other words, it is precisely his ideas about cultural pluralism that enable him to rise above mere hypocrisy or self-delusion, which is how some scholars have interpreted his behavior (Hulliung 1976, 123; Shklar 1987, 31, 34; Todorov 1993, 355-6). He can be both intellectual and despot because he divides the world into distinct spheres with particular rules and practices. He tolerates ambiguity and incoherence, the very values that Montesquieu and his adherents claim are antithetical to despotism (Berlin 1980; Shklar 1987; 1989; Wolin 1989).

Montesquieu's followers often argue that despotisms thrive in the absence of the rule of law and that they dispense with the need for moral justifications. Law and morality only hamper the workings of absolute power. Montesquieu makes just these claims in The Spirit of the Laws (II.1.10; II.4-5.17-20; III.8-10.26-30; V.10-16.56-66). What is most striking about the harem, however, is its obsession with morality and lawfulness. The harem is nothing but rules and endless homilies about upholding the rules. In the set of instructions that Usbek leaves to his chief eunuch, he claims that the eunuch must preside over a strenuous moral regime based on law. "Your tireless attentions uphold virtue when it vacillates." The eunuch is the "scourge of evil and the pillar of fidelity." He is charged with ensuring that the wives "carry out the laws of the harem." Not only is he responsible for maintaining strict, clear laws, but also he must use his power to uphold them. "You command as a master like myself," Usbek tells the eunuch, "whenever you fear a weakening of the laws of decency and modesty" (letter 2, 47-8). Far from promoting arbitrary violence, Usbek ordains strict rules and conditions for the exercise of violence. Punishments are severe, to be sure, but they are meted out under precise circumstances and with exquisite care. The eunuch must use his power only in response to a specific transgression. He is not to act out of whimsy or passion.

At one point during his travels, Usbek finds out that Zachi, one of his most beloved wives, has allowed a white eunuch into her room, which is one of the cardinal crimes of the harem. Black eunuchs are allowed to enter the rooms of the wives, but white eunuchs are not.² When Usbek hears of Zachi's transgression, he immediately writes her: "You have of-

² Although black sexuality will later come to symbolize a particularly potent threat in Western racial imagery, in Montesquieu's account, blacks are not as sexually menacing as whites.

fended me, Zachi, and I feel in my heart emotions that should cause you to tremble if my remoteness did not allow you the time to change your conduct and calm the violent jealousy that torments me (letter 20, 72). He asks: "How can you have forgotten yourself to the point of not realizing that you are not permitted to receive a white eunuch in your room when you have black ones provided to attend on you?" Were she to argue that the white eunuchs were not real men and therefore could not excite her, he would not be convinced, because her actions violate both "the laws of the seraglio" and his "honor." Should she respond that she has never broken previous rules, he would not be impressed. Guarded by the eunuchs twenty-four hours a day, she never had the chance not to be virtuous. She has affected "a virtue that is not free" (letter 20, 73).

As an instrument of fear, this letter is a revealing document. It shows that although Usbek is impassioned and angry, he does not lose his capacity for reason. If anything, the letter is a triumph of Thomistic logic, formulating postulates, posing questions, offering answers. With its prosecutorial back-and-forth, Usbek's letter suggests a despot quite different from our conventional accounts of the voluptuous, pleasureseeking prince. At various points in the letter, Usbek shows calculated restraint, indicating that he is willing to give Zachi another chance. He holds out the threat of punishment and the possibility for mercy as a means of making her frightened enough to do what he wants but hopeful enough not to forsake the regime. If Zachi were to decide that Usbek would kill her no matter what she did, then she might conclude that open revolt was a more reasonable path to her own preservation.

Usbek also attempts to create fear in Zachi through a denunciatory moralism. He repeatedly comes back to Zachi's betrayal of him, her flouting of the law, her false virtue. These expressions of moral outrage and invocations of the law are not hypocritical or selfserving. They convey Usbek's sincere conviction that Zachi has transgressed against an entire moral order that will now come crashing down upon her. He claims that her fellow wives view the harem as "a happy shelter against the attacks of vice, a sacred temple where your sex loses its weakness and becomes invincible despite all the disadvantages of its nature" (letter 20, 73). The harem is an oasis of morality and purity that has protected Zachi and her friends. She is thus implicated in the comforts of this order. She should value its protections and the safety it provides against far more dangerous predators. By challenging it, she not only has threatened Usbek and morality in general but also has betrayed herself, threatening the things most dear to her. Usbek writes:

What would you do if, left to yourself, you had for sole defense your love for me (which you have so grievously offended) and your duty (which you have so shamelessly betrayed)? How holy are the customs of the country where you live that you should be snatched from the attacks of the lowest slaves! You ought to thank me for the discomfort in which I make you live, for it is only because of it that you deserve to go on living (letter 20, 73–4).

Usbek appeals to Zachi's guilty conscience, which suggests that, at least for the despot, invoking moral strictures and moral values can work hand in hand with fear. Morality only serves as a tool of discipline to the extent that all parties in the despotism believe in it. That is why Usbek invokes it so often.

Enforcer

One obstacle Montesquieu faces in explaining regimes of terror is how to account for the fact that a single individual is able to terrorize so many. If these other individuals were to join together, they would have far more collective power at their disposal and could easily defeat the despot. The despot, like all agents of domination, must have allies and arms. Montesquieu's solution in The Spirit of the Laws is the "vizir." The despot seeks to fulfill his desires, but to ensure that his power is maintained while he is gratifying himself, he entrusts the means of violence to the vizir. The vizir works with a small circle of subvizirs, each of whom is dedicated to maintaining fear throughout the regime. It is not clear why these vizirs are afraid of the despot. Montesquieu suggests that they are psychologically impoverished characters. They are weak and culturally primitive, which might explain their subjugation to the despot even though they control more weaponry and command more physical force than he does. At a minimum, they cannot have either courage or ambition (the hallmarks of true monarchies), for that might induce them to subvert his authority.

The prince's immense power passes intact to those to whom he entrusts it. People capable of much self-esteem would be in a position to cause revolutions. Therefore, fear must beat down everyone's courage and extinguish even the slightest feeling of ambition (Montesquieu [1742] 1988, III.9.28).

And many scholars of Montesquieu have reiterated this claim (Althusser 1972, 70-4; Cohler 1988, 40, 71-2; Hulliung 1976, 39-40; Keohane 1980, 411).

The harem provides a fascinating counterpoint to these received views. Usbek relies upon his own vizirs, the eunuchs, to control his wives. But since he has entrusted them with total power over his wives, he must ensure that they remain afraid of him. Usbek must never allow the eunuchs to discover their own power, and he must never provide them enough incentives to attempt to overcome their fear. Contrary to those who claim that the only way Usbek can keep the eunuchs in check is to exercise force against them (Cohler 1988, 72), Montesquieu reveals two key weapons: first, the authority of the older, more senior eunuchs; second, the ambition of the younger eunuchs. By carefully manipulating a hierarchical world of advancement and promotion, Usbek is able to keep the eunuchs in line.

These paired realities—the mentorship of the senior eunuchs and the ambition of the younger eunuchs—suggest that despotic terror does not arise outside the normal structure of a society but instead depends upon it. It is precisely the connections among people, particularly those connections premised upon unequal power

and resources, that undergird fear. The older eunuchs have authority over the younger because of their experience and power, and the younger are willing to collaborate with the older ones because they see in their seniors a means of advancing their own interests and ambitions.

One of Usbek's senior eunuchs confesses that his life has entailed nothing but unmitigated fear. "I sigh, overwhelmed as I am by the weight of fifty years of cares and anxieties. In the course of a long life I can say that I have had not one peaceful day nor any tranquil moment" (letter 9, 55). The eunuch appears to embody the traditional understanding of despotic fear. He lives in a state of constant anxiety and insecurity, never sure what might happen to him, always dreading the worst. His personal history, however, discloses a different face of fear. When he was a young slave, he was asked by Usbek to become a eunuch so that he could assume responsibility over the harem wives. In taking the job, the eunuch made several calculations. First, Usbek threatened him, claiming that he would face severe punishment if he did not agree to be castrated. Second, he was tired of being a slave. The work was difficult and wearying, and he wanted a change. Third, he was ambitious and hoped to work his way up to a higher position. "I planned," he says, "to sacrifice my passions to tranquility and fortune" (letter 9, 55).

The desire for a promotion undergirded the eunuch's fear in several ways. It enabled him to accommodate his fear, to accede to its dictates because he believed he might gain something by doing so. But it also exacerbated his fear by suggesting to him not only that he would face unbearable pain and suffering if he resisted but also that he might miss out on a promising opportunity. We do not know what he would have decided had he been content with his own position. Perhaps the threats would have been enough. Yet, the mere fact that Usbek felt compelled to tempt him with possibilities, to remind him of how miserable his situation was, suggests that threats alone cannot create fear. Some additional desire is necessary to sustain fear. The promise of advancement made the capitulation to fear a sensible proposition. In fact, the eunuch admits that when he decided to be castrated he could "see the recompense but not the loss" (letter 9, 55). Utility maximization is thus crucial to the structure of fear among the eunuchs (see also letter 64, 134-7; appendix 2, 285-8). Fear takes hold of the individual not by destroying his aspirations but by stoking and manipulating them. It thrives on the highly idiosyncratic ambitions of each person.

The pursuit of ambition, however, is not an idea that occurs naturally among the slaves and eunuchs. They must learn it. Education thus turns out to be a central institution in despotism, but it is neither the education described by Montesquieu scholars (Richter 1973, 9) nor the despotic education described by Montesquieu in *The Spirit of the Laws*: "Education" in a despotism "is, in a way, null there." Because "education must bring about servility," the despot should only teach "the spirit of a few very simple religious principles." Genuine "knowledge will be dangerous" (IV.3.34-5).

In The Persian Letters, education does not make the eunuchs ignorant or downtrodden. It stokes their ambition and encourages them to think about the promise of power and status. It prescribes self-mutilation as a smart career move. One slave, Pharan, tries to resist castration. Like many of the younger men, he does not want to give up his "humanity"-no matter how tempting the career prospects (letter 42, 101). His mentors attempt to teach him otherwise. The first black eunuch writes to Usbek that he is attempting to persuade Pharan to "allow himself to be consecrated to that office" because the eunuch believes it would "be to [Pharan's] advantage" (letter 41, 100). The senior eunuchs teach their pupils that it is irrational to resist castration because they will reap untold benefits if they join the harem's managerial rank.

The senior eunuchs also assuage and soften the younger eunuchs' violent emotional reactions, which get in the way of rational calculation. When the younger eunuchs feel sadness or anger, the older ones soothe them. The first eunuch writes to Jaron, one of the younger black eunuchs, that he saw Jaron's "childhood advance with pleasure." The older eunuch was Jaron's mentor, advising him and helping him navigate the dangerous shoals of the harem. When the time came for "the blade" to separate "forever" Jaron from his "nature," the older eunuch was on hand both to soothe and to cut. "I quieted your tears and your outcries." He loved Jaron as a father loves a son. By playing this ostensibly humanitarian role, he inducted him into the despotic regime. Tenderness was used to temper Jaron's anger and sadness, making his life more normal and bearable and making him less of a threat to the regime. Kindness sustained fear by calming emotion and enabling the younger eunuchs to remember what they gain by castration. "I thought of you," the first eunuch writes to Jaron, "as having a second birth and taking leave of a servitude in which you always had to obey, to enter another kind of servitude, where you were to command" (letter 15, 67).

By reminding the eunuchs of what their capitulation to fear has earned them, and by tranquilizing their anger and resentment, these older eunuchs reveal the extent to which fear depends upon concentric circles of elites spread throughout society. Without the older eunuchs. Usbek could not wield power because he could not attend to all the unhappiness that his regime creates. The older eunuchs play a pacifying role for a variety of reasons. They have a genuine love and affection for their younger brethren. They hope to cushion their pain and diminish their anguish. They also have their own self-interest at stake. They have attained positions of power, status, and responsibility. Should the younger eunuchs get too angry or grow disruptive, the older ones might lose control of the harem—and the power and prestige that go with that control. Despotic fear is thus sustained by the eunuchs, who act in part for reasons that have little to do with their fear of Usbek. In many interpretations of despotic fear, victims and victimizers share the same motivation-a simple fear of violence-and they act for no other reason than that fear (Todorov 1993, 362). In this case, however, not only are the eunuchs capable of diverse motivations—including their own self-interest, a genuine compassion for the younger eunuchs, and a pride in their work—but also it is precisely the range and extent of their motivations that enable the despotism to survive.

Victim

After Usbek and the eunuchs, there are the wives. If anyone should display the characteristics scholars often attribute to fear—paralyzed will, crippled rationality, total loss of self—it should be the harem women. In fact, the wives maintain a high degree of independence and selfhood, and they also have their own particular concerns and motivations, which serve as props to the harem's stability and survival. The wives launch tiny rebellions and commit small acts of disobedience. One eunuch discovers that a wife, Zephis, has had an affair with a slave girl, Zelid. When she is found out, Zephis craftily drafts a letter to Usbek, protesting her innocence and appealing to his mercy. She paints herself as the unwitting victim of the eunuch's jealously and nastiness, thereby undermining his reputation in the eyes of Usbek and advancing her own (letter 4, 50). Other wives also have affairs and strategically break the rules of the harem, taking every opportunity to fulfill their own desires. Far from beaten down, they are resourceful and strategic about their cooperation and submission. This is not to say they are not frightened. They are frightened enough to avoid overt challenge to Usbek's authority. But short of that, they do what they can to construct a life for themselves.

One resource at their disposal is their ability to make the lives of the eunuchs miserable. They make incessant demands and send the eunuchs scurrying at all hours of the night for some desired pleasure. The wives use this power strategically. In return for not harassing the eunuchs, they receive special privileges (letter 9, 55-8). They also use Usbek's sexual desire as a weapon for their own advancement. If the eunuchs too zealously pursue the wives' transgressions, the women tempt Usbek to bed and, at the moment of his greatest pleasure, extract his promise to punish the eunuchs. One eunuch observes: "I have everything to fear from their tears, from their sighs, from their embraces, from their very pleasure. . . . Their charms can become terrible for me." His fear rests upon these rational transactions between Usbek and the wives—a pardon in exchange for pleasure—and he laments that the wives' "present services" to Usbek "wipe out in one moment all my services of the past." The eunuch concludes that his own fear is heightened by the "amorous negotiation" of the wives and their ability to craft a "treaty made with sighs" (letter 9, 57-8). With all their overtones of bargaining, exchange, and contract, these metaphors suggest that despotic terror rests more on a certain kind of economy of fear-discrete interests and goals are fulfilled through a trade of services—than on the total loss of self that we encounter in much of the secondary literature.

Even when the wives seem the most degraded, the

most stripped of their humanity, they reveal a desire for self-promotion that calls into question some of the rhetoric found in The Spirit of the Laws and among many Montesquieu scholars. Zachi reminds Usbek of a beauty contest he once conducted among his wives. Each was forced to strip in front of him, decorating her body with makeup, jewelry, and exotic accessories. Usbek examined each of them meticulously. His "curious regard" was "extended" to their "most secret spots." He had them "assume a thousand different positions—ever a new command and ever a new submission." In the history of political philosophy, it would be difficult to find a more painfully exacting portrait of personal degradation. The beauty contest seems the perfect metaphor for the stripping down of self that has come to be emblematic of despotism. Nonetheless, Zachi confesses that at this moment of submission, she longed for victory, for that would bring her greater status in the harem. If anointed the most beautiful, she would become the "mistress of [Usbek's] heart." Not unlike the eunuchs, she accepts domination in order to stand above others. She was not weighed down by the strain of total humiliation. Instead, she envisioned a trajectory of personal advancement (letter

Although self-serving, Zachi's ambition—and that of the other wives—has a cost: It keeps Usbek in power. These ambitions for success can only be satisfied if Usbek remains at the top of the hierarchy and then chooses one of the wives as his favorite. Being afraid, being submissive, and keeping an eye on one's personal prospects thus fit together perfectly. Their congruence reveals how fear is structured by and reinforces a limited, short-term rationality, one more focused on personal advancement than collective emancipation.

As in the case of the eunuchs, however, the wives do not automatically subscribe to this short-term rationality. They, too, must develop an elaborate network of authority among themselves in order to teach the younger wives the harem rules. Zelis, for example, writes Usbek that she has decided to enroll her daughter into the harem at the early age of seven. She does not want to wait until the child is ten, the standard age of introduction. Allowing her to run freely for a few more years would only make the girl love the freedom of youth rather than cherish the "holy education within the sacred walls." Thrust into the harem at the usual age, the girl might experience its strictures as a kind of violence. Zelis wishes to coax rather than plunge her daughter into the harem's treacherous seas. Zelis believes in Aristotelian education. The girl should learn slowly the practice of docility so that submission will acquire the "gentle effect of habit." Submission is not natural. It must be learned, and rebelliousness must be unlearned: "In vain do they talk to us of the subordinate position in which Nature has placed us. It is not enough to make us feel that. We must practice our role of subordinate so that it may hold us firm through the critical period when passions begin to appear and encourage us toward independence" (letter 62, 132-3).

We have come to think that familial love does not survive despotic fear, but it does. In fact, it can even sustain fear. Zelis is inspired by a deep love for her daughter. Her desire for her daughter's happiness and well-being leads her to try to indoctrinate the child so thoroughly that she will never challenge Usbek; she will come to accept a regime of fear not as an alien imposition but as part of the manners and mores of her culture. In this case, fear does not depend on violence or vicious acts of cruelty. It is stoked by the love of a mother for her child and by the mother's education of her child. The very humanness of this connection helps reproduce the despotic order. Far from proving that despotism relies upon an entirely different universe of feeling and desire, *The Persian Letters* shows how ordinary emotions—even love and compassion—can bolster the most terrible regimes of fear.

Contrary to some of the more partisan claims in *The Spirit of the Laws*, and contrary to the almost universal consensus among Montesquieu scholars, *The Persian Letters* gives the following account of despotic fear. First, fear is not antithetical to reason. It thrives on an instrumental, cost-benefit analysis, whereby the victims and purveyors of fear calculate what is lost and gained from defying or cooperating with threatening power. Fear does not just coexist with this kind of rationality. It is structured by this rationality. The eunuchs, for example, fear that if they do not accept castration they will miss a promising opportunity. Rationality lends a kind of moral legitimacy to capitulating to fear. Acting out of fear seems less dishonorable when it promises to secure a desired good.

As the calculations of both the wives and eunuchs demonstrate, however, this rationality is limited. It prevents men and women from achieving their longterm, higher-order interests. No one would claim that either the eunuchs or wives are well served by the despotic apparatus they help sustain. Moreover, given that Usbek is absent from home for a good part of the novel, the cooperation of the eunuchs and wives seems particularly irrational. They could easily decide as a group-without any threat from Usbek-that fulfilling their master's commands is not in their interests and that liberation is. But that would require them to see beyond the immediate goods they receive from cooperating, that is, the goods of survival, power, promotion, and position. Their fearful reasoning, then, follows a kind of truncated rationality. Fear is not opposed to rationality; it merely rests upon an incomplete rationality.

Second, implicit in this first point is that fear is intimately connected to the broad range of sympathies, desires, and aspirations that motivate ordinary men and women. The fearful do not lack virtue, honor, ambition, love, loyalty, or any of the other characteristics that make us human. They are fearful precisely because they are human, because they do have ambitions and loyalties, because they do love and have some semblance of honor. Zelis's attempts to indoctrinate her daughter so that she will not suffer from a violent induction into the harem is inspired by a perfectly honorable and virtuous parental impulse—to protect her child from harm. The outsider, of course, can see what the consequences are. But from Zelis's perspec-

tive, her attempts are neither craven nor disreputable. They come from a deep reserve of familial connection. One might even say that, given the options, Zelis has made a humanitarian gesture, albeit one with devastating consequences.

Third, fear is not aroused solely by the cruelty or excessive violence of the despot. Kindness and wellmeaning compassion, in fact, can be fear's most terrible adjutants. In Zelis's case, we see that fear will be generated in the child by a mother who has all the best intentions in the world. Like all good parents, Zelis will probably attempt to transmit hard-won lessons of life to her daughter, and these lessons will be framed as coping methods for the little girl to advance herself in the world. The senior eunuchs are likewise inspired, in part, by a desire for their younger colleagues to avoid some of the pain and suffering they themselves experienced. These small acts of kindness, however, only deliver everyone into Usbek's grip. The terrible irony of Montesquieu's novel is that, pace contemporary followers of Montesquieu (Shklar 1984, 1987, 1989), fear does not depend on cruelty. Fear is not solely the product of violence or actions explicitly designed to inspire fear. It just as easily follows from a humanitarian concern to lessen the suffering of one's own.

Fourth, despotic power need not be arbitrary, concentrated, and centralized, and it need not be unregulated by the rule of law or moral strictures. Usbek is cruel. His punishments can be excessive. But they do not just fall upon anyone. They are targeted at those who transgress against the harem. In that regard at least, despotic power is fairly predictable. The eunuchs understand that they can only inflict pain on those who have broken the rules. Individuals who obey the rules avoid punishment. Likewise, Usbek neither monopolizes all power nor delegates it to a small coterie of vizirs. As we have seen, each member of the harem has some kind of power, some resource to deploy against the blandishments of those above them. The wives can manipulate Usbek and the eunuchs. The eunuchs have some power as well against the wives. More important, among the eunuchs and among the wives, there are forms of power and authority that go beyond Usbek's control. The senior eunuchs rule over the junior eunuchs; the wives rule over their daughters. This sharing of power does not diminish the generation and transmission of despotic power. If anything, it is usually used to exacerbate it.

Furthermore, the power that Usbek and the eunuchs do exercise is not entirely freed of moral categories of legitimacy. Usbek, the eunuchs, and even the wives work hard at propagating the virtues of the harem. Power is supposed to be exercised against vice and on behalf of virtue. It is a virtue that we might view as impoverished and debilitating, as false and ultimately destructive of liberal values, but that does not mean power is wielded without any appeal to values and to ethical purposes. What is more, those values and purposes are often framed as principles designed to benefit not just one individual—Usbek—but everyone. All members of the harem partake of their own

particular virtue, and the power that is exercised is designed to enable all to fulfill their particular virtue.

Finally, the social universe underlying despotic power has all the characteristics of pluralist spheres, separated institutions, multiple associations, and hierarchical elites that we have come to think of as checks against despotic fear. The harem is not a simple arrangement of power whereby a violent individual strides across a wasteland, the flash of his sword illuminating the absence of opposition and social cohesion. Usbek's power rests atop many different kinds of relationships and hierarchies. There is the relationship between Usbek and the senior eunuchs, who in turn have a relationship with their junior eunuchs. The women stand beneath the eunuchs, and among the women there is also the latent hierarchy among the wives and between mothers and daughters. In one regard, then, the harem resembles nothing so much as the pyramidal structure of feudalism, with reciprocal obligations and duties arranged vertically. But the harem is even more complicated: The wives are also in some measure senior to the eunuchs, able to negotiate power and resources through their direct contact with Usbek. The world of the harem, in short, is one in which the avenues of social influence are as crooked and entangled as the streets of eighteenth-century Paris. It is not just that despotic power can thrive amid enormously confusing and complicated social arrangements. Despotic power is transacted entirely through those arrangements, which makes fear the product not of sociological simplicity but of pluralist density and sociological complexity.

THE THEORY IN PRACTICE

For the better part of three centuries, this alternative account of fear is not the one that has captured the attention of Montesquieu's readers. Instead, they have focused on two points: Fear is all-consuming and destructive of selfhood, and it thrives in the absence of civil society and countervailing powers. One reason for this preoccupation has to do with the rhetorical power of Montesquieu himself: The bracing qualities of his vision in The Spirit of the Laws are undeniable. Another reason has to do with his readers. Since the eighteenth century, the traditional account of despotic fear has been used as a political weapon, first by Enlightenment polemicists against the European state, then by social theorists such as Tocqueville against majoritarian democracy, and finally by twentieth-century opponents of totalitarianism (Arendt 1951; Friedrich 1954; Friedrich and Brzezinski 1965; Getty and Manning 1993, 1-18; Tocqueville [1835, 1840] 1969). Because despotic fear has played such a central role in modern political struggles, intellectuals of all stripes have tended to overstate its totalizing dimensions. In the specter of a despotism so horrifying that it destroys all semblance of civilization, theorists and intellectuals have found much ballast for their arguments. There can be no doubt that the shocking imagery of despotic fear has served a useful function. It has rallied men and women of humane sympathy to oppose tyrannies around the

world. But this imagery has a price: It has left us with caricature rather than insight.

When the theory of despotic fear first appeared, intellectuals decried its dehumanizing stereotypes. The most formidable challenge came from Abraham-Hyacinthe Anquetil-Duperron, who argued that the theory of "Oriental despotism" grew out of inaccurate travel reports about the governments of Turkey, Persia, and India. These accounts, he claimed, overlooked the rule of law, the role of private property, and other elements of sociological and political density that Montesquieu claimed did not exist in despotic regimes. More damningly, Anquetil maintained that, despite the best intentions, Montesquieu so distorted the East that he inadvertently justified European colonial expeditions. An emphasis on the allegedly backward or primitive elements of non-Western societies emboldened Europeans to seize control of them in the name of enlightenment and, later, progress. A theory designed to denounce despotism at home thus provided an excuse, according to Anquetil, for practicing it abroad (Behdad 1989; Richter 1973, 12-3; 1995; Shackleton 1988, 239; Venturi 1963, 137–9; Young 1978).

In our own time, we can find situations in which a portrait of fear intended to mobilize opposition to tyranny in one place enjoys the dubious irony of defending tyranny in another. Consider the strange career of Hannah Arendt's (1951) acclaimed account of totalitarianism. Although Arendt argued that totalitarian terror was of a radically different order than the despotic terror envisioned by Montesquieu, her analysis owed more to his description than she admitted. According to Arendt, terror keeps people in such a state of permanent dread that they cannot act in any but the most predictable ways. Its victims are stripped of the capacity for "spontaneous human action." They merely behave, repeating patterns of human motion prescribed in advance by nature or history. Under Stalin, they march as unthinking soldiers in an advancing column of history. Under Hitler, they are the germ-free bearers of a racially cleansed society. In either case, they have forsaken the last vestige of humanity—the ability to defy expectations, break patterns, forge new courses. There are no criminals in these regimes because no one is capable of transgression. "Guilt and innocence" thus "become senseless notions." Fear cannot even serve the function of selfpreservation. It loses "its practical usefulness" because "actions guided by it can no longer help to avoid the dangers man fears" (Arendt 1951, 465-7). As in Montesquieu's account of despotism, the social world of totalitarianism has been pulverized. Classes give way to masses, and individuals now find themselves without the protective contours of a hierarchical society. Limited government, the rule of law, decentralized power-all these checks against despotism are eliminated as the totalitarian state imposes an entirely new order of sentiment and being upon a population of rootless and exposed men and women. Arendt's description of the totalitarian state does feature some departures from Montesquieu, but it nonetheless recapitulates and

extends his basic defining elements of despotism (pp. 311-26, 392-437).

Like Montesquieu, Arendt was inspired by an astringent moral vision, a vision of opposition to tyranny anywhere and everywhere. But in the hands of her followers, the portrait of total terror came to justify a defense of "traditional" authoritarian regimes on the ground that these are less toxic than totalitarian regimes. Jeanne Kirkpatrick, the preeminent theoretician of American foreign policy during the 1980s, drew upon this familiar account of fear to defend American opposition to Marxist governments and support for regimes such as those in Chile, El Salvador, and South Africa. In a widely discussed article, Kirkpatrick (1979) claimed that Somoza's Nicaragua and the shah's Iran were soft tyrannies in which the state tolerated the multiplicity and diversity of traditional society. These autocracies were far less terrifying than Marxist regimes, which destroyed mediating institutions, emptied civil society of traditional elites and traditional values, and installed an all-powerful, all-knowing state. Standing in solidarity with friendly despots, then, could serve a humane function: It would help ward off the more lethal terror of communist totalitarianism.

More recently, the idea of total terror has inspired much of the revival in Europe and the United States of the concept of civil society. Contemporary theorists now envision intermediary, nonstate institutions as a counter to the statist tyranny that has disfigured so much of the twentieth century. Some theorists even claim Montesquieu as an intellectual forebear. It remains unclear what the consequences of this most recent incarnation of the idea of total terror will be, but we can see some of the same tropes. The family and other close circles of intimacy are presumed to be destroyed by totalitarianism, as are those spaces of civil society not directly under the control of the state. Therefore, to avoid the totalitarian temptation, we must cultivate them anew. Zones of familial and fraternal feeling must be revived to create space for an alternative logic. The dense spaces of a richly textured civil society will foster an ethic of personal loyalty and commitment; regardless of whether this ethic is political, by virtue of not being under the control of the state, it will serve as a counter to tyranny (Cohen and Arato 1992; Elshtain 1995, 38-9, 42-3, 45-52; Gray 1993, 158; Keane 1988; Taylor 1995a, 204-5, 214, 222; 1995b, 185, 211).

But how illuminating is this account of total terror? Is it true that fear destroys the person, that it liquidates the family and other institutions of civil society, that we lose all capacity for reason, emotion, and other defining elements of selfhood? The Persian Letters offers an alternative way of viewing the relationship between fear and the self, the state, and civil society. Many of those elements that we have come to believe are antithetical to fear can be reconciled with it. Some of those elements, in fact, can be crucial to the creation and maintenance of fear. Was this account merely the literary speculation of an eighteenth-century Frenchman? Evidence from one of the most notorious episodes of twentieth-century fear suggests otherwise.

For Western intellectuals, perhaps no single event of the twentieth century better captures Montesquieu's vision of total terror than the Moscow show trials of the late 1930s. In a series of staged public events, Stalin trotted out leaders of the Bolshevik revolution who confessed to fantastic crimes against Lenin, socialism, and the Soviet regime. The repression of political opponents and even show trials were hardly unprecedented, but the willingness of political luminaries to denounce themselves seemed to open a new chapter in the history of terror. The most stunning confession came from Nikolai Bukharin, the principal theoretician of the communist movement and beloved heir to Lenin, who admitted that he was the ringleader of a counterrevolutionary conspiracy. How could an individual of Bukharin's stature, a revolutionary leader hailed throughout the world, confess to such extraordinary crimes? It could only mean that he had lost all sense of self, that he was so engulfed by terror that he no longer cared enough to preserve his own life. Bukharin's willingness to cooperate in his own destruction appeared to signal the unhappy triumph of Montesquieu's vision: Terror was indeed the great nullifier of selfhood. In his famous novel, Darkness at Noon, Arthur Koestler (1941) turned Bukharin's actions into a parable of the relationship between totalitarianism and the eclipse of the individual. Bukharin's cooperation with Stalin also inspired a film by Jean Luc Godard, La Chinoise (Cohen 1980, 372, 473; Conquest 1968, 132-3; Koestler 1941; Tucker and Cohen 1965, xxv-xxvi). Bukharin's self-abasement gave credence to Montesquieu's notion that fear is so consuming that it destroys the self and the self's connection to all those institutions—the family, civic associations, and so onthat might lend sustenance to the struggle for freedom and personal dignity.

As we now know, the reasons behind Bukharin's confession were quite different from those suspected by intellectuals at the time. Before his trial, Bukharin was imprisoned for a year, subjected to brutal methods of interrogation, and repeatedly threatened. Despite the threats against his body and life, he managed to hold out for three months "with remarkable vigor." What finally induced his capitulation was Stalin's threat to kill his wife and son. More than anything else, this led Bukharin to make his public confession. Threats against family members were visited upon countless victims of Stalin and proved to be one of the most effective means of securing cooperation with the regime. Bukharin and many others had no hope of saving themselves, but they did hope to save their loved ones (Cohen 1980, 375; Conquest 1968, 142, 301).

Three elements of Bukharin's decision are worth noting, both for what they reveal about the actual nature of terror and for what they show about the limitations of our conventional analysis. First, Bukharin felt a strong connection to his family. In fact, it was precisely his love for his family that made him afraid enough to cooperate with Stalin. Second, Bukharin made a clear choice based on certain calculations. He believed his confession would lead to a desired good, whereas his refusal would only lead to a feared evil. In

order to make this choice, he had to understand the relationship between his actions and a desired result. He had to think of himself as an agent. Finally, Bukharin was inspired by a desire to outlive himself. He hoped that his family would survive to tell his story. He thus had to think about the future—beyond his own lifetime—and he had to have enough self-regard to believe that his name and his story were worth preserving. For that reason, he inserted all sorts of qualifications into his public confession, which provide close readers of the trial transcript multiple clues about his innocence, the evil of Stalinism, and his place as Lenin's true heir (Cohen 1980, 376–80; Tucker and Cohen, 1965, xlii—xlviii).

Despotic terror does not rob us of our capacity for reason, love, or selfhood. It preys upon these capacities to force us to act in ways we do not wish to act. When we love the members of our family, a despotic regime will ask us to choose between their survival and actions we do not wish to undertake. Montesquieu's followers might reply that every despotism has its share of Bukharins, that we always can find some incidental case of honor or familial love amid despotic terror. But, they might continue, these cases are not constitutive of the regime's power. They survive despite the regime.

It seems to me that quite the opposite is true. It was Bukharin's sense of honor and virtue—his desire to protect his family, his name, and the movement he worked to create—that motivated his cooperation with Stalin. If Bukharin had not cared about his family, would he have agreed to a public confession and public humiliation? He might just as well have chosen silence or a private execution. There were individuals who refused to make public confessions, and a number of them had no children (Conquest 1968, 142). Beyond Bukharin himself, it is these connections between human beings and the human capacity to make choices between the lesser of two evils that sustain fear. Particularly under despots such as Stalin who recognize no limits to what they will threaten, these capacities can be crucial to coercing even the most resolute opponents. Were it not for the human capacity to imagine and carry out the most horrifying evils-and were it not for the equally human capacity to choose between those evils—these regimes might not have such sway.

There is something almost perversely comforting about our conventional vision of terror. It suggests that the victims of fear somehow escape its horror because they are so dehumanized, so divested of civilization, that they cannot register the cruelty being visited upon them. They suffer, but it is only we, the spectators, who grasp the catastrophe. In reality, despotic terror seldom allows its victims the refuge of insanity or the grace of unawareness. It forces rational men and women to be conscious witnesses to and active participants in the carnage that surrounds them. The notion of total and absolute terror is a tribute to our desire to preserve the distinctions between barbarism and civilization, fear and selfhood, terror and reason. But it is also a conceit: The tragedy of Bukharin is not that he lost all sense of self. He might have been spared a great deal of suffering if he had. It is that he was forced to reason about the unreasonable and to choose among the unchooseable. Through these choices, through discrete decisions about whether to submit or to defy, despotic terror consolidates and exercises power. Stalin got his confession; Bukharin got his family and his legacy. It was a terrible bargain, a contract of self-destruction. But it was a contract nonetheless, one upon which Stalin built his regime (Cohen 1980, 373–4; Conquest 1968, 145–7).

THE FACES OF FEAR

Seventeen years ago, Norman Hampson (1983, 3) wrote that *The Persian Letters* was the "first major work of the French Enlightenment." With its racy Orientalism, sympathetic accounts of homosexuality and incest, and thinly veiled criticism of French political culture, it challenged prevailing ideas about sex, morals, politics, religion, and culture. Almost three centuries later, the novel still emits these dissident sparks of the Enlightenment. It is as subversive as ever, although perhaps not quite in the way Montesquieu intended.

The Persian Letters forces us to rethink our assumption that political fear depends on the state's liquidation of the traditional institutions of civil society and the familiar elements of human nature. Careerism, the system of apprenticeship, and moral rules that teach the virtues of obedience are aspects of despotic rule that depend on a set of hierarchical institutions, and they are manipulated rather than destroyed by the despot. These institutions contain leaders, men and women with authority over others. When these leaders choose to cooperate with the state, they will use their relationships with their followers to instill fear among them and to encourage them to submit to the regime's dictates. These relationships between leaders and followers in civil society are the building blocks of political fear. It is the presence of these institutions and leaders-not their absence-that makes a population so vulnerable to political fear.

The collaboration between the state and civil society need not correspond to our stereotypical image of revolutionary tyranny, of a state that mobilizes civil society and whips it into a frenzy. It many of these regimes, a far more quiet association is at work. Collaboration only requires that the state either threaten something of importance to elites or hold out to them some desired good. When faced with the possibility of attaining or losing a good, these elites will put themselves at the service of the regime. As several recent studies of tyrannical regimes in Eastern Europe and Latin America have shown, collaborators work with the state for a variety of reasons (Remnick 1994; Rosenberg 1991, 1995). They seldom follow a single logic or pursue the same goals. Yet, the very range of human sympathies and diversity of aspirations enable the state to penetrate areas of public and personal life that it might not otherwise be able to regulate. The regime need only be flexible in identifying what matters to specific elites in order to compel them to cooperate. Once the regime has mapped out this diverse geography of incentives and desires, it can use its power to develop a relatively cooperative relationship with these elites, who in turn will do what they must to deliver their followers into the regime's grip.

We have come to think that fear has only one face: the face of the despot. Everyone else is a shadow of his personality. His is also a face that has been disfigured beyond recognition. It is certainly not a human face. In painting this picture, Montesquieu deliberately invoked a caricature. His distortion was supposed to jolt readers out of their complacent regard for the torture and violence of the Old Regime, violence that Foucault (1979) would later describe in such memorable detail in the opening pages of Discipline and Punish. Montesquieu sought to portray despotic fear as so monstrous that his contemporaries would no longer accept it as customary, as part of the ordinary course of events. His intentions were wholly legitimate and understandable. If today, however, we are to remain true to the Enlightenment spirit of The Persian Letters, then we must see that regimes of fear depend not only on a despot but also on a web of collaborators, each of whom has individual concerns and aspirations. We must see that there is no one face of fear but many, the faces of tyrants, elites, leaders, and followers. Most important, we must realize just how ordinary, how very human, those faces can be.

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City Size and Civic Involvement in Metropolitan America

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iven the coincidence between America's recent migration to smaller, suburban cities and declines in civic participation, Dahl's speculations on the ideal-sized democratic polity have gained more pertinence. I explore the effects of city size on participation in four local civic activities using 1990 data. Controlling for both individual- and city-level characteristics, I find people in larger cities are much less likely to contact officials, attend community or organizational meetings, or vote in local elections. Lower civic participation is attributable partly to differences in social relations and psychological orientation between residents of larger and smaller places. People in big cities are less likely to be recruited for political activity by neighbors and are less interested in local affairs. These differences occur irrespective of the size of the surrounding metropolitan area and demonstrate the importance of municipal institutions for fostering civil society. The implications for studies of participation, suburbanization, and democratic political theory are discussed.

To end the alienation from government that is so prevalent in society today... the answer is not busting up a big city into a lot of small cities. You slice baloney, you get baloney.

Tom Hayden, Los Angeles mayoral candidate¹

ince the time of Aristotle, political theorists have puzzled over a difficult question: What is the optimal size for a democratic polity? In the 1967 presidential address to the American Political Science Association, Robert A. Dahl (1967, 960) offered an answer: Most democracies are too big to allow citizens actively to determine the "vital aspects of their lives in common." Small polities, however, often lack the capacity to address meaningful political issues. The idealsized unit, Dahl reasoned, must be able to achieve collective goals but avoid the "consummatory" participation of the modern nation-state. Since we seem "destined to live in cities," Dahl proposed dividing large metropolitan areas into federations of municipalities between 50,000 and 200,000 in size. These cities would be small enough to facilitate civic participation but large enough to generate meaningful political discourse.

Over the past fifty years, Dahl's vision has become a reality. Most Americans now live in small to medium-sized cities or "places" within large, densely populated metropolitan areas.² Since 1950, the proportion living in metropolitan areas has risen from 57% to 75%. Yet,

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¹ New York Times, May 29, 1996, p. A10.

proportionally fewer Americans reside in large cities: Less than 19% currently live in cities of more than 250,000, compared to 23% in 1950; 56% now live in metropolitan places smaller than 250,000, compared to 34% in 1950 (U.S. Bureau of the Census 1975, 1993). America has changed from a country bifurcated between isolated rural towns and big central cities to one that consists largely of small and medium-sized suburbs.

The civic consequences of this shift are unclear and the empirical research on the civic effects of city size is inconclusive. On the one hand, the migration to smaller, suburban places has coincided with a welldocumented decline in such activities as voting and organizational membership (Putnam 1995; Teixeira 1992; Wattenberg 1996). This suggests that participation is lower in smaller, suburban places, a finding supported by Fischer (1976). On the other hand, several studies report that residents of smaller places are more likely to participate (Kasarda and Janowitz 1974; Nie, Powell, and Prewitt 1969; Verba and Nie 1972). Yet, all these works fail to provide definitive evidence: Fischer's estimates do not control for many important individual-level characteristics; Kasarda and Janowitz and Nie, Powell, and Prewitt compare only "urban" and "rural" places; and Verba and Nie's results, from a sample of only 120 cases, are not statistically significant. Moreover, all these studies use data more than 30 years old, and city size has not been analyzed in the participation research since then, which means that the effects of city size are unmeasured relative to the recent trends in suburbanization.

America's metropolitan expansion also calls into question many key assumptions about city size in classical democratic theory. For instance, does it have the same effect in a metropolitan setting of contiguous municipal boundaries, uninterrupted land development, and interdependent local economies? Does Dahl's ideal democratic city of 50,000 foster as much

surrounding urbanized counties, that is, counties identified by the Census to have large portions of contiguous areas with high population density. Consequently, places of up to 50,000 population (the first two categories of city size used in this analysis) can be classified as rural or urban depending on their proximity to a metropolitan area.

² By "place" or city, I refer to all incorporated municipalities, although generally city refers to larger municipalities and place refers to smaller ones. Rural is measured as residence in any county that is not part of a metropolitan area. Borrowing from the 1900 U.S. Census (U.S. Bureau of the Census 1991), a metropolitan area is any county that contains a central city of at least 50,000 and the

civic engagement if it is in the middle of greater Los Angeles or isolated on the Kansas plains? Previous work has not examined these important questions. To complicate matters, most suburban places are not like large cities in miniature but are highly differentiated in social composition and land usage, characteristics that also shape civic participation. Oliver (1999) finds that people in affluent suburbs participate less than people in heterogeneous, middle-income places and that the effects of city affluence are greater than population size. Indeed, population size may no longer be the most important civic characteristic of a city.

The size issue also remains enigmatic partly because of the absence of any general theory on the relationship between social environments and political participation. Most social theorists either ignore civic participation (Simmel [1905] 1969; Tonnies 1988; Weber [1905] 1958; Wirth [1938] 1969) or do not link their speculations to models of why people participate in civic processes (Dahl 1967; Dahl and Tufte 1973; Montesequieu [1748] 1991; Rousseau [1772] 1994). Indeed, latter-day democratic theorists offer mostly vague and contradictory expectations about how polity size directly shapes civic involvement. Theorists of participation, meanwhile, mostly focus on individual-level factors and rarely take social context into account (Olson 1965; Rosenstone and Hansen 1994; Verba, Schlozman, and Brady 1995; Wilson 1972). Despite Lewin's (1935) nearaxiom that human behavior is a function of both individual and environmental characteristics, most studies of civic participation concentrate on models of isolated, rational actors or on hypotheses validated by individuallevel survey data. To establish causal linkages between city size and participation, a theoretical bridge between studies of context and participation is needed.

This article explores the civic ramifications of city size, particularly in contemporary metropolitan and rural settings. I start by outlining a theory about the relationship between social environments and civic activity in the metropolis using a "civic voluntarism" model. Then, based on a data set constructed from the 1990 Citizen Participation Study (Verba et al. 1995) and the 1990 Census, I estimate four types of civic activity and find them all to be lower in larger places, a relationship that occurs irrespective of the metropolitan context. Large size depresses participation partly because residents are less likely to be mobilized and are less interested in local political life. These findings demonstrate the civic relevance of municipal boundaries in an era of metropolitan expansion: City boundaries define communities, and smaller places are civically richer. Before celebrating the civic virtues of suburbanization, however, other city-level characteristics need consideration. Tom Hayden notwithstanding, smaller cities are not simply more baloney; rather, they are distinct cuts of meat, some more civically palatable than others.

CIVIC VOLUNTARISM AND SOCIAL CONTEXTS

Previous theories on the civic effects of city size are inconclusive partly because they fail to explain which factors influence participation and how they may be shaped by the social environment. For example, Dahl and Tufte (1973) make a series of deductive inferences about civic participation in large and small democratic units. They reason that "smaller democracies provide more opportunity for citizens to participate...but, larger democracies provide citizens opportunities to participate in decisions . . . to control the most important aspects of their situation" (p. 13). In their framework, a city's population does not affect the nature of the civic act: Casting a ballot or contacting a government official is essentially the same in sprawling Houston as in tiny Startzville. Rather, place size shapes participation indirectly by altering the opportunities for involvement. But Dahl and Tufte assume that smaller places provide more opportunities for participation and that availability stimulates involvement. These assumptions, however, are not based on any general theory or empirical tests. Dahl and Tufte do not demonstrate whether or how a polity's size changes the opportunities for participation, whether opportunities really do influence involvement, or what intervening characteristics may be operating. Such criticisms hold equally for other theoretical speculations (e.g., Montesequieu [1748] 1991; Rousseau [1772] 1994; Weber [1905] 1958; Wirth [1938] 1969). To overcome these deficiencies, the individual determinants of participation must be identified, and hypotheses then must be formulated on whether they differ between large and small places.

What are the determinants of political participation? A voluminous literature offers a wide range of theories (e.g., Olson 1965; Rosenstone and Hansen 1993; Teixeira 1992; Verba and Nie 1972; Wilson 1972; Wolfinger and Rosenstone 1980), but I use Verba, Schlozman, and Brady's (1995) "civic voluntarism" model. According to their framework, political participation is a function of individual resources, interest, and mobilization; people are more likely to participate if they have skills and knowledge, if they are more psychologically engaged, or if they are recruited by others. Although not explored in the original formulation of the model, each factor varies with a person's social environment. Psychological engagement in community life is clearly determined by context: When people feel they have more in common with neighbors or have a greater sense of efficacy, interest in local affairs is greater (Fischer 1976). Political mobilization varies with patterns of social interaction: When people are more familiar with one another, they are more likely to talk about politics and recruit others for action (Huckfeldt and Sprague 1995). Context even shapes the influence of individual resources: When participation is more difficult, the relevance of individual knowledge and skills grows (Wolfinger and Rosenstone 1980). By examining variations in political resources, interest, and mobilization between small and large municipalities, the causal connection between city size and participation can be specified.

Previous research in this area has arrived at strikingly contradictory conclusions. On one side are those who argue that both psychological engagement and mobilization increase with a city's population. Dahl (1967) and Deutsch (1961) believe larger places have more compelling issues to attract citizen attention; Fischer (1995) and Suttles (1972) find that larger places host more subcultures that mobilize citizens; Milbrath and Goel (1982) claim that greater media attention to big city politics stimulates citizen interest; and Dahl and Tufte (1973) speculate that larger polities have a higher level of political competition that mobilizes citizens and makes their participation more efficacious.

On the other side are those who suggest that a large population is a detriment. Early classics of urban sociology (Simmel [1905] 1969; Tonnies 1988; Weber [1905] 1958; Wirth [1938] 1969) argue that the size, density, and heterogeneity of larger places dissolves the social and psychological bonds that exist between neighbors in small towns. Surrounded by more strangers and greater social uncertainty, urbanites putatively seek psychic refuge in their primary social relations, shy away from formalized social contact, or feel content as "bystanders" to the political process (Finifter 1970; Latane and Darley 1970; Nie, Powell, and Prewitt 1969; Reisman 1953; Verba and Nie 1972). In addition, people in larger places are less likely to know their neighbors, have mutual friends, and see acquaintances in public settings (Fischer 1982; Lofland 1973), which in turn may inhibit political mobilization (Huckfeldt and Sprague 1995). Finally, even though the act of participating may be the same in different cities, the costs of doing so may vary. Larger cities require a more complex bureaucracy, have greater spatial distance between city offices and citizens, and have elected officials who represent more people, all of which may increase the difficulty of participation and the importance of individual resources (Hansen, Palfrey, and Rosenthal 1987).

These issues are further complicated when one considers that most Americans do not live in isolated towns but within larger metropolitan areas. Most studies designate large cities as "urban" or "metropolitan" and small places as rural, but Dahl's speculations suggest that municipal boundaries are important in their own right for defining community and patterns of social interaction. Thus, when considering population size, the effects of living in a municipality must be distinguished from living in a large urbanized area.

On the one hand, if city boundaries are unimportant amid a surrounding urbanized population, then the effects of metropolitan areas should both replicate and overshadow the effects of city size; it should not matter whether people are partitioned by the invisible walls of a municipal border. The important contextual element, in this instance, is the number of people in a given geographic region, not simply the number within a particular municipal jurisdiction. The major contextual influence on civic involvement will be the size of the metropolitan area and not the particular city, and the largest differences in participation will occur between small rural places and large metropolitan regions.

On the other hand, if city boundaries are important for defining the character of local political engagement or patterns of social interaction, then the metropolitan

environment should not alter the place-size effects. Differences in participation between people in large and small places should occur irrespective of the size of the surrounding metropolis because political engagement and mobilization arise primarily from the community as defined by the city boundaries. If this is true, then residents of small places in both rural and metropolitan settings should have equal levels of participation relative to their counterparts in large cities.

In sum, if larger places stimulate citizen interest and nourish a variety of subcultural social networks, then participation should increase with population size; if greater size produces alienation and social disconnection or makes involvement more costly, then participation should decline with population gains. But the effects of metropolitan contexts may hinge on the importance of city boundaries for defining social behavior and political attitudes. If the boundaries are important, then the size of the surrounding area will not alter the effects of city size; if the boundaries are less important, then metropolitan effects will supplant the relationship between city size and participation, and larger differences should exist between rural and metropolitan areas.

DATA AND ANALYSIS

Testing these assertions requires overcoming the classic difficulty of cross-level inference. Most data on citizen activity are either aggregate (e.g., statistics on precinct or county voting) or individual (e.g., surveys), which greatly restricts contextual induction (Achen and Shively 1995). An appropriate test of the effect of contemporary metropolitan social contexts requires information not only about individuals and their context but also a sample from a wide variety of places. To meet these criteria, I constructed a data set from the 1990 American Citizen Participation Study (CPS) (Verba et al. 1995) and the 1990 Census (U.S. Bureau of the Census 1991). The CPS is a national, crosssectional survey of the participatory activities of the American public; among the approximately 15,000 respondents in the screener set, 2,500 took part in in-depth follow-up interviews. For this analysis, I use data from the follow-up interview portion of the CPS. These respondents were drawn from more than 800 different places. In order to measure the effects of social context, the respondents' residence was identified, and data from the 1990 Census for the summary level of place (i.e., city) were matched for each case.³ With this individualized census information for such a large nationwide sample, the relationship between measures of social context (e.g., population size, median income) and individual behavior can be estimated while controlling for individual-level determinants, such as education, income, and age.

After constructing an appropriate cross-level data set, I then had to decide which civic behaviors to

³ Thirteen percent of the interviewees (N = 2,500) were omitted because they did not live in identifiable municipalities or under municipal jurisdictions.

examine. These range from voting to joining voluntary organizations (Putnam 1993; Verba, Schlozman, and Brady 1995), and studying the relation of all to city size is impossible in a single article. Moreover, not all civic activities are equally susceptible to contextual influences. For example, social context should influence turnout in local elections more than in national elections. To estimate the consequences of city size, civic behaviors should be local in orientation, politically directed, and foster the type of social bonds that form the basis of civil society (Putnam 1993).

I chose four variables that best meet these criteria: Contacting Locally Elected Officials, Attending Community Board Meetings, Attending Meetings of Voluntary Organizations, and Voting in Local Elections. All these activities are not influenced by social context in exactly the same way, but all represent important aspects of locally oriented participation. Contacting officials and voting are the two most direct ways people communicate their preferences about local policies to local leaders. Attending the meetings of community boards and voluntary organizations, as Putnam (1995) argues, sustains the "norms and networks of reciprocity" upon which civil society is built.4 Furthermore, participation in each of these activities should be sensitive to the incentives and opportunities discussed above. If all these acts are influenced by interest and mobilization, as Verba, Schlozman, and Brady (1995) suggest, then the effects of city size should be consistent across all.

FINDINGS

I start by comparing average rates of participation in all four civic activities across five categories of city size (less than 5,000; 5,000 to 50,000; 50,000 to 250,000; 250,000 to one million; and more than one million); residents of metropolitan and rural areas are separated in the first two categories. The participation measures are based on self-reports. Contacting officials and attending meetings are scored dichotomously (1 if the respondent had engaged in the activity in the past year, 0 otherwise); voting in local elections is measured on a five-point scale (1 = never to 5 = always). A full description of the variables is given in the Appendix.

With the exception of voting in local elections, the average rate of participation in all types of civic activity tends to decline in larger places, although this effect is primarily limited to residents of metropolitan areas. As depicted in Figure 1, 40% of residents of metropolitan places of less than 5,000 report contacting local officials, compared to 30% in places between 5,000 and 50,000 and 25% in places of more than one million. Meeting attendance is 13 percentage points lower in the largest than in the smallest places for community boards, and 12 percentage points lower for voluntary organizations. The average local voting score declines

by .2 points between metropolitan places of less than 5,000 and those between 50,000 and 250,000 but then levels off. Among residents of rural towns, contacting and voting rates decline between small and larger places, but attendance at community board or organizational meetings increases. On average, rates of participation in civic activities are lower in the smallest rural towns than in places of the same size within metropolitan areas. For example, the average rate of attending community board meetings is 9 percentage points lower in rural than metropolitan places of less than 5,000.

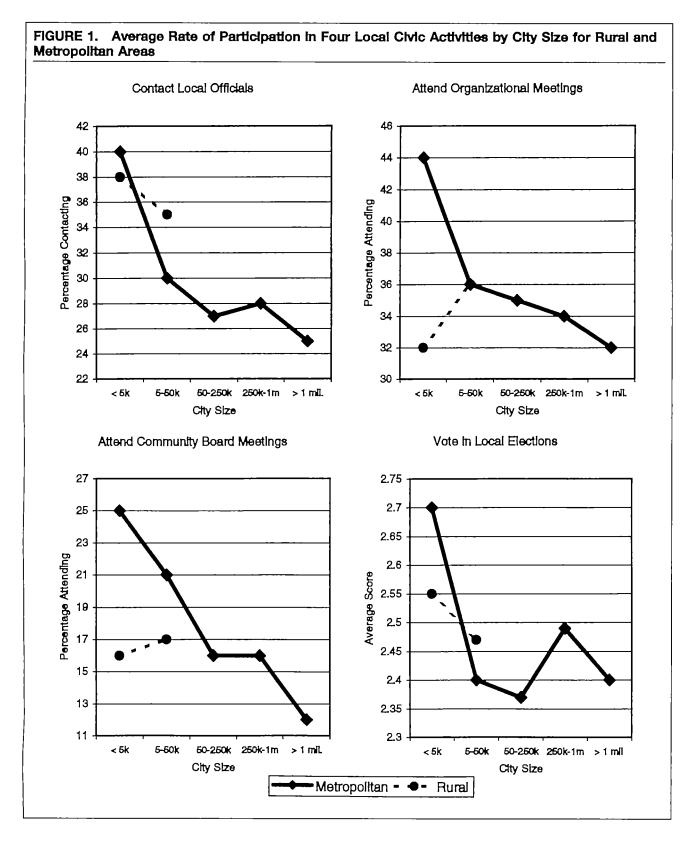
On the whole, these figures support the hypothesis that civic participation diminishes in larger places, at least in metropolitan areas. For three out of four indicators, steady decline occurs with an increase in place size. The highest participation rates are in the smallest metropolitan places. In rural areas, however, the relationship between population and civic participation differs in two respects. First, in towns of less than 5,000, participation rates are typically much lower in rural than metropolitan areas. Second, unlike the pattern in metropolitan areas, participation does not necessarily decline as rural size grows. Indeed, residents of rural places larger than 5,000 are more likely to attend board and organization meetings and are only slightly less likely to vote or contact officials than are rural dwellers in very small towns. What accounts for these rural anomalies?

Part of the answer may lie in demographic differences rather than social context (Finifter and Abramson 1975), so demographic profiles are worth exploring in further detail. Table 1 lists average levels for Years of School Completed, Age, Percent Homeowners, and Median Household Income by place size for rural and metropolitan areas in the CPS/Census sample.6 Compared to all residents of metropolitan areas, rural dwellers on average have completed almost one year less of school and live in places with a median annual household income almost \$10,000 lower. As both an individual's education and city-level income are important determinants of civic participation (Oliver 1999; Verba, Schlozman, and Brady 1995), these lower resource levels may explain lower rural participation. Yet, rural residents are older and more likely to own a home, individual factors that correlate with higher participation rates. Furthermore, despite the steady decrease in civic participation within metropolitan areas, there are few demographic variations by size: Residents of small metropolitan places have, on average, levels of education, income, and age similar to those of people in large cities. The only demographic variance is that people in smaller places in metropolitan areas are more likely to be homeowners. Given at least some confounding demographic characteristics, multivariate equations are needed to isolate the specific effects of city size on participation.

⁴ Given Putnam's (1995) speculations about the importance of face-to-face contact in voluntary organizations, I chose meeting attendance rather than membership as the measure to gauge local "social capital"

⁵ All analyses for this article were conducted with SPSS for Windows, version 8.0.

⁶ The CPS measured individual income on an eight-point scale, so I used the measure for the median household income of the city from the 1990 Census as a measure of income differences by place size and metropolitan/rural context.



Toward this end, I employed a logistic regression for the three dichotomous participation items and an ordinary least squares (OLS) regression for the voting scale. Each measure was regressed on several explanatory measures: *City Size* (measured in ten increments along a 0-1 scale); dummy variables for residence in Rural areas and Small Metropolitan Areas (less than one million), with residence in a metropolitan area of more than five million counted as the excluded category; an interaction term between city size and rural residence (city size × rural); individual characteristics associated with civic participation (Verba, Schlozman, and Brady

TABLE 1. Average Education, Age, Home Ownership, and Median Household Income by Place Size for Rural and Metropolitan Areas

	Years of School		Percentage	Med. Household	
	Completed	Age	Homeowners	Income	N
Rural place size					
Less than 5,000	12.4	44.9	72	\$18,676	243
5,000 to 50,000	13.2	43.7	67	\$19,990	292
Rural area average	12.8	44.2	69	\$19,393	535
Metropolitan place size					
Less than 5,000	13.2	40.4	81	\$ 28,527	152
5,000 to 50,000	13.6	41.1	72	\$35,565	477
50,000 to 250,000	13.8	39.7	59	\$28,719	492
250,000 to 1 million	13.9	40.6	53	\$24,831	298
More than 1 million	13.7	39.8	42	\$27,939	240
Metropolitan area average	13.7	40.3	61	\$29,283	1,659

Sources: The multi-level data in these analyses are combined from two separate sources. The variables measuring individual participation and demographic characteristics (i.e., education, age, etc.) come from the 1990 Critizen Participation Study (Verba et al. 1995). For each respondent in the CPS, the place of residence was identified and indicators of city population size, median household income, percentage black, metropolitan area size, and rural residence were appended from the 1990 Census of Population and Housing (U.S. Bureau of the Census 1991)

1995), such as Education, Age, Income, Length of Residence, Marital Status, Homeownership, race (Black), and sex (Female); and two other city-level social characteristics: affluence (Median Household Income) and racial composition (Percentage Black). To control for regional effects, I included South as a dummy variable. The coefficients from the equations are presented in Table 2, and a full description of the variables is given in the Appendix.

Controlling for other individual and city-level characteristics does not alter generally the negative relationship between civic participation and city size. Comparing the predicted rates of participation between the smallest and largest places, the likelihood of contacting local officials drops by 16 percentage points, attending organizational meetings by 8 percentage points, attending community board meetings by 18 percentage points, and voting in local elections by .14 points on a five-point frequency scale (from an average score of 2.5 to 2.36).8 The city size coefficients are statistically

⁷ A multitude of social characteristics distinguish American places (Berry 1972), but racial and economic segregation is routinely identified as the most important by-product of suburbanization

(Massey and Denton 1993; Schneider 1987). Therefore, in distin-

guishing among suburban places, I use measures of median household income and percentage black. significant in all equations except the one predicting voting.

These findings have several points worth noting. First, the relative size of the coefficients demonstrates that city size is itself a powerful predictor of local civic activity. For example, the logistic coefficients in Table 2 for the three equations predicting nonelectoral participation are larger for city size than all other individual-level variables except for education, age, and income. In other words, the differences in contacting officials or attending meetings between very small towns and the largest cities are greater than those between renters and homeowners, men and women, or married and single people.

Second, the predicted participation rate steadily declines as population increases. Alternative tests for a curvilinear relationship between city size and participation are generally negative.¹⁰ The model predicts

the value of the city size coefficient multiplied by the value of each increment along the city size scale

⁹ Of course, because of the nonlinear character of the logistic regressions, the comparative magnitudes of the independent variables cannot be estimated perfectly from the coefficients. To calculate the relative effect of one coefficient compared to another, a probabilistic function for each variable, as described in note 8, must first be estimated. For example, translating the coefficient for education (2.14) in the equation for contacting generates a predicted difference of 34 percentage points between those with only eight years of education versus those with an advanced degree. Nevertheless, comparing all the translated coefficients, the differences across city size are still larger than differences across any individual level variable except education, age, and income, just as the raw coefficlents indicate. For example, the predicted differences in the rate of contacting officials across the city size scale (16 percentage points) is greater than that between renters and homeowners (6 percentage points) or men and women (5 percentage points).

For instance, when a squared term for city size is included in the equations, no large or statistically significant coefficients are generated. The relationship between city size and civic activity also can be found by breaking the size variable into nine dummies, with residence in a city of more than one million as the excluded category, and reestimating the equation. When this was done, the coefficients for the dummy variables progressively dwindled as city size increased in all three equations in which size had a significant effect on civic

involvement.

⁸ The aggregate marginal effect of city size on the probability of participating was calculated by first using the logistic coefficients to compute a predicted probability of participation (p) for each person in the sample: $p(x) = F(B_0 + B_1 + ... + B_n X_n)$. A second probability was calculated using the same equation except that for each respondent the lowest value of the city size scale (0) was substituted for the regular city size term. After recalculating both probabilities relative to the base of the natural logarithm, the difference between the two probabilities was then estimated for each respondent and the average scores were taken for each increment of the city size scale. For a full description of this procedure see Wolfinger and Rosenstone (1980, 123). Using this procedure, the logistic coefficient for city size in the equation predicting contacting -.712) translates into a predicted average difference of 16 percentage points between residents of places with population of less than 2,500 and those in places with population of more than one million. For the OLS equation, the predicted effect of city size was derived by setting all other independent variables to their means and adding these to the constant term. This new average rate was then added to

TABLE 2. The Effects of City and Metropolitan Area Size on Local Civic Participation, with Controls for Individual and Contextual Population Characteristics

		Attend	Attend	Vote
	Contact Officials	Board Meeting	Organization Meeting	Local Elections
City-level variables				
Ćlty slze	712 ** (.256)	-1.48 ** (.036)	−.424* (.20 8)	133 (.13 4)
Med. household Income	588* (.275) [°]	– .932* (.009)	-1.0 4** (.358)	−.724 ** (.004)
Percentage black	.587 (.349)	1.37** (.414)	530 (.331)	.309* (.156)
Metropolitan area size ^b	, ,			
Small metro area	.179 (.145)	−.2 4 5 (.178)	− <i>.</i> 053 (.142)	.055 (.179)
Rural	.361 (.263)	761* (.328)	207 (.260)	.042 (.141)
Clty size × rural	440 (.679)	1.59 (.886)	261 (.683)	528 (.36 6)
Other variables				
Education	2.14** (.232)	1.97** (.291)	2.19 ** (<i>.</i> 226)	1.55** (.117)
Income	.816** (.052)	1.08** (.261)	1.12** (.201)	.429** (.109)
Age	.652** (.260)	.528 (.327)	.538* (.254)	2.23** (.135)
Homeowner	.357** (.132)	.526** (.174)	.404** (.126)	.358** (.067)
Married	.052 (.115)	.015 (.146)	.062 (.111)	.115 (.059)
Black	247 (.15 7)	456 (<i>.</i> 207)	.269 (.147)	.055 (.078)
Female	239* (.10 4)	198 (.124 <u>)</u>	−. 09 0 (.101)	.038 (.055)
Length of residence	.182 (.191)	.343 (.252)	.241 (.185)	.217 (.096)
South	201 (.12 6)	308* (.158)	127 (.121)	172 (.065)
Cox & Snell R ²	.12	.10	.13	$(r^2.28)$
N	2,032	1,914	2,038	2,022

Source: See source note for Table 1

that, ceteris paribus, residents of a city such as Woodside, California (pop. 4,300), are 8% more likely to attend a local community board meeting than those in nearby Cupertino (pop. 41,000), who are 6% more likely to do so than people in neighboring San Jose (pop. 750,000). The effect of size is continuous: The larger a city becomes, the less likely are its citizens to participate in local affairs.

Finally, the effects of city size seem largely independent of the greater rural or metropolitan context. The coefficients for the interactive term in all four equations is small and not statistically significant. People in smaller rural places and in smaller suburban places are equally more likely to participate relative to people in large cities. In other words, the model predicts that compared to those in a city like Los Angeles, in a city the size of Santa Monica, California, residents are no less likely to participate in local civic activities than are people in rural and identically sized Sioux City, Iowa, even though Santa Monica is nestled within a metropolitan area that is 20 times as large.

On the whole, there are almost no differences in civic behavior according to either the size of the metropolitan area or whether the setting is rural. By itself, city size has no effect: None of the equations predict significant differences in participation between residents of small and large metropolitan areas. Moreover, controlling for size generally has little influence on the magnitude of the city size effect, as the predicted variations in participation by city population are roughly the same in the multivariate equations as they are in the cross-tabulations illustrated in Figure 1. The only civic activity that does vary between metropolitan and rural areas is attendance at community board

meetings. Like the findings in Figure 1, people in rural places are less likely to attend community board meetings than people in metropolitan areas. But beyond this, rural dwellers living in similarly sized places are no less likely than people in metropolitan settings to undertake any other civic activity, such as contacting officials or voting in local elections. Most of the differences in participation rates between metropolitan and rural settings are the consequence of individual demographic characteristics.¹¹

The constancy of the city size effect demonstrates the importance of urban boundaries for shaping civic life. Most theories assume that people react to their city as a contained community, even if it is part of a larger metropolitan area. For example, the hypothesis that people in smaller places are more politically engaged is predicated upon the assumption that they respond to their city as their primary environment, rather than their metropolitan area. We assume that Santa Monicans are more likely to engage in community life or know their neighbors than are Los Angelenos because the boundaries of Santa Monica somehow define a

^{**}p < .01, *p < .05, standard errors are in parentheses.

^{*}Coefficients from OLS regression

Excluded category is metropolitan area of more then one million

Other multivariate analyses demonstrate that, controlling for just city size, rural residents are less likely to attend organizational meetings and vote. When the variables measuring everything but education and income are included, people in rural areas continue to have a lower participation rate, although the differences are no longer statistically significant. Adding the education and income measures greatly attenuates the rural coefficients. This suggests that the lower levels of rural participation are due largely to socioeconomic differences. Interestingly, no matter what controls are added, rural dwellers are always more likely to contact local officials, although this difference is not statistically significant, and always less likely to attend community board meetings, a difference that is statistically significant.

TABLE 3.	The Effects of City Size b	y Education on Local Civic	Participation, with Controls for
Individual	and Contextual Population	Characteristics	

	Contact Officials	Attend Board Meeting	Attend Organization Meeting	Vote Local Elections
City-level variables	· -			
Ćtty stze	871 ** (.334)	-1.12 ** (.399)	658* (.333)	110 (.191)
Med. household Income	−.572* (.267) [°]	−.810* (.444́)	-1.02** (.358) [´]	−.702** (.19́8)
Percentage black	.706 (.332)	1.34** (.405)	– .444 (<u>`</u> .324)	.418* (.192)
Education interactions	,	` ,	` ,	, ,
High school or less	-1.12** (.234)	882** (.296)	-1.38** (.234)	−.713 ** (.131)
Some college	329 (.235)	.019 (.302)	464 (<u>.</u> 256)	037 (.1 49) ^
City size × high school	.109 (.415)	371 (.541)	.648 (.394)	097 (̀.217)́
Clty stze × some college	.234 (.437)	167 (.521)	033 (.432)	223 (.249)
Metropolitan area stze ^b				` '
Small metro area	.189 (.144)	185 (.1 <i>77</i>)	045 (.140)	.065 (.077)
Rural	.263 (.196)	380 (.24 4)	253 (.19 3)	073 (.10 6)
Other variables		, ,	. ,	, ,
Income	.949** (.204)	1.20** (.258)	1.26** (.199)	.533** (.109)
Age	.617* (.260)	.573 (.328)	.404 (.253)	2.17** (.Ì37) ´
Homeowner	.405** (.131)	.607** (.174)	.450** (.125)	.398** (.067)
Married	.053 (.115)	–.031 (.146)	.048 (.110)	.114 (.060)
Black	274 (.157)	468 (<i>.</i> 207)	.248 (.147)	.046 (.079)
Female	25 4* (.103)	201 (.128 <u>)</u>	108 (.100)	.022 (.055)
Length of residence	.113 (.189)	.260 (.251)	.187 (.183)	.169 (.096)
South	245* (.123)	299* (.155)	−.161 (̀.119)́	216 (.0 6 5)
Cox & Snell R ²	.12	.10 ` ′	.13 `´	(<i>r</i> ² .28) ´
N	2,032	1,914	2,038	2,022

Source: See source note for Table 1

community with which its residents identify, even though the physical demarcation may be nothing more than a signpost. The absence of any interaction effect based on rural area validates this interpretation. City boundaries are important for defining social behavior and perceptions, even when that city is nestled within a huge metropolis.

CAUSES OF THE CITY SIZE EFFECT

According to my usage of the civic voluntarism model, the lower participation rates in larger places must result from differences in resources, political interest, and/or patterns of political mobilization. The source of the effect can be identified, therefore, by estimating how each determinant of participation changes with a city's size and then gauging how much the city size effect attenuates once these determinants are controlled.

Previous research focuses considerable attention on individual resources relative to the costs of participation (Rosenstone and Hansen 1994; Verba, Schlozman, and Brady 1995; Wilson 1972). For instance, Wolfinger and Rosenstone (1980) argue that the elderly and educated are more likely to vote because they have more resources to deal with the "costs" of electoral registration, particularly in states with difficult procedures. These individual differences in resources may be similarly relevant to city size if, as some researchers suggest, participation is more difficult in larger places

(Dahl and Tufte 1973; Hansen, Palfrey, and Rosenthal 1987; Verba and Nie 1972). Faced with larger and more complex bureaucracies, greater distances to public offices, and political organizations of larger scale, people in big cities may find voting, contacting officials, or attending meetings difficult.

If participation is more "costly" in larger places, then the effects of city size should vary according to individual resource levels. Just as the young or uneducated are less likely to vote when registration is more difficult (Wolfinger and Rosenstone 1980), so should these "resource poor" groups be more adversely affected by city size. In other words, if contacting an elected official is more difficult in Chicago than in Peoria, Illinois, then differences in contacting rates between high school dropouts and college graduates should be greater in Chicago. To test this hypothesis, I reestimated the civic participation equations with new interaction terms between two levels of individual education (High School [diploma] or Less and Some College) and city size (city size \times high schl., city size \times some col.). Those with a college degree or postgraduate education were the excluded category. The results are listed in Table 3.

In none of the four equations did the interaction terms between individual education and city size yield any large or statistically significant coefficients. The equations predict that the effects of city size are no different among differentially educated groups. Similarly negative results were also found when other measures of individual resources (e.g., age and income)

^{**}p < .01, *p < .05; standard errors are in parentheses.

^{*}Coefficients from OLS regression

^bExoluded outegory is metropolitan areas of more than one million

were substituted for the education measure.¹² These results provide no evidence that civic participation is lower in larger places because it is more "costly."¹³

Since the city size effects do not arise from variations in the cost of participation or individual resources, they must come from differences in political interest or mobilization. Previous empirical work offers inconclusive evidence that people in larger places are less engaged in community life or less likely to be mobilized. Fischer (1976, 1982) reports little evidence of any distinct psychological patterns in large places, but he does find city dwellers more "estranged from the wider community" than rural dwellers: The former are less trusting of strangers, less familiar with neighbors, and feel less efficacious with respect to community affairs. Similarly, Finifter (1970) reports that people in larger urban centers are more politically estranged. Yet, it is unclear whether these findings can explain the decline in civic participation, because none of the studies link urban alienation to lower participation. It is generally assumed, however, that political interest increases with city size because the stakes of political contests, the number of participants, and the visibility of local politicians are all higher (Verba and Nie 1972). In large places, residents have an easier time following local politics and, therefore, are thought to be more interested in local affairs. It is not less plausible that people in larger places may feel more removed from their community, so political interest may be low. Despite the greater stakes and visibility of local politics, residents may "tune out" as part of their psychological disengagement from their surroundings.

Previous research also suggests that patterns of political mobilization may vary as a function of city size (Fischer 1982). In small towns, social networks are portrayed as closely knit, in that people are more likely to know one another. In large places, social networks are typically characterized as loosely knit; there is less proximity and redundancy among acquaintances (Bott 1971; Kasarda and Janowitz 1972). Patterns of recruitment should differ, therefore, between these environments: Information about and recruitment for local activities between neighbors should be greater in small towns than in large places.

To test whether differences in psychological engagement or mobilization are the source of the city size effect, I return to the CPS/Census data set. Although the CPS does not contain any explicit indicators of political or social alienation, it does have items that

measure interest in local and national politics. These may capture some level of psychological disengagement. Respondents were also queried about whether they had been asked by someone to take part in a number of community oriented activities, including serving on a community board or contacting a local public official. From these questions, a dichotomous measure of mobilization was created (1 = asked in the past year to contact a local official or serve on a community board, 0 otherwise). An OLS regression was employed for the political interest items, and a logistic regression was done for the mobilization measure, with the same predictors used above. The coefficients for the equation are listed in Table 4.

In larger places there is significantly less interest in local politics than in smaller places. Compared to people in places smaller than 2,500, people in cities of more than one million score nearly .40 lower, on average, on the five-point local political interest scale. But this negative relationship does not appear to reflect any generalized aversion to politics among urban residents. When national political interest is regressed on the same set of predictors, the coefficient for city size is small and not statistically significant.

People in larger places are also less likely to be politically mobilized. Translating the coefficients into probabilities, residents of the largest cities are 17% less likely to be asked to serve on a community board or contact a government official than are residents of small towns. Further analysis indicates that the character of mobilization changes with place size as well. Residents of large cities are much less likely to be mobilized by friends or neighbors than are people in small places. Aside from individual education, age, and income, city size is the greatest predictor of whether a person is mobilized. As with the earlier findings, these results occur irrespective of the rural or metropolitan area. 16

Are these lower levels of local political interest and mobilization in larger places responsible for the decline in civic participation? The answer can be found by reexamining the relationship between place size and civic activity while controlling for local political interest

¹² To test the interaction between income and city size, three income categories were created: less than \$25,000, between \$25,000 and \$50,000, and above \$50,000. Interaction terms for the first two categories and city size were included in the equation. In no case were the coefficients for the interaction terms large or statistically significant. Similarly negative results were found when interactions between senior citizens (over 65 years) and city size were created.

¹³ Another test of participation cost was based on the assumption that participation in larger places is more time consuming. The CPS had several items measuring free time, although these are subject to numerous criticisms (see Verba, Schlozman, and Brady 1995). Although people in larger places did report less free time, controlling for free time had no influence on the relationship between city size and any of the civic activities.

¹⁴ Using the procedure described in note 8, I translated the logistic coefficient in the equation predicting mobilization depicted in Table 4. The logistic coefficient (-.496) translates into a difference of 17 percentage points between residents of the smallest and largest places.

¹⁵ Respondents in the CPS were also asked who had mobilized them. Selecting those who had been mobilized, I created two dichotomous measures: whether a person had been mobilized either by a friend or by a neighbor. These two items were then regressed on the standard set of predictors. In both cases, the city size coefficient was large and statistically significant. Translating the logistic regression coefficients into probabilities, mobilized respondents in small towns are 16% more likely to be mobilized by an acquaintance and 13% more likely to be mobilized by a neighbor than are people in larger places, which further demonstrates that the latter are less likely to have social contact with their neighbors.

¹⁶ Running the same equations with interaction terms between metropolitan area and city size yielded no statistically significant additional effects of city size in larger as opposed to smaller metropolitan areas.

TABLE 4. Interest in Local and National Politics and Mobilization by City Characteristics and Control Variables

	Inte	vrest In	
	Local Politics	National Politics	Mobilized*
City-level variables			
Člty slze	178 ** (.069)	026 (.346)	496* (.231)
Med. household Income	−.300** (̀.141)́	.147 (.102)	186 (.359)
Percentage black	.307* (.126)	.165 (.132)	.188 (.331)
Metropolitan area sıze ^b	• • •	,	(******)
Small metro area	047 (.0 5 5)	0 6 5 (.053)	.089 (.142)
Rural	037 (̀.076)́	.032 (.077)	206 (19 6)
Other variables	` ,	(/	(,
Education	.983** (.084)	1.22** (.096)	2.47** (.228)
Income	.305** (.078)	.497 ** (.073)	1.37** (.203)
Age	.821** (.097)	.584** (`.069)	1.05** (.257)
Homeowner	.159** (.048)	.090 (.046) ´	.508** (.127)
Married	.037 (.043)	004 (.041)	116 (.112)
Black	.095 (.055)	036 (̀.052)́	305* (.151)
Female	018 (.0 3 9)	114** (.040)	093 (.102) [°]
Length of residence	.102 (.069)	084 (.065) ´	034 (.182)
South	015 (.04 6)	035 (. 044)	244 (.121)
R ²	.17 ` ´	.19 ` ´	.19 (Cox & Sne
N	2,028	2,028	2,038

Source See source note for Table 1.

p < 0.05, p < 0.01, standard errors are in parentheses.

Coefficients from logistic regression.

becaused category is metropolitan areas of more than one million

and mobilization. If civic activity diminishes in larger places because residents are less likely to be recruited by their neighbors or are more alienated, then the relationships between place size and participation should attenuate once the interest and mobilization items are included in the equations. To test this assertion, I reestimated the regression equations for each civic activity with the four-point local political interest scale and a dummy variable for mobilization. The results are given in Table 5.

When political engagement and mobilization are taken into account, the negative relationships between place size and three of the civic activities attenuate. For example, comparing the findings in tables 2 and 5, the difference in the rate of contacting local officials between the smallest and largest places diminishes from 16 to 12 percentage points. The gap in attendance at organizational meetings also shrinks—compared to people in the largest places, residents of places smaller than 5,000 are only 3% more likely to attend organizational meetings once political interest and mobilization are considered, a difference that is no longer statistically significant. On the voting scale, the city size coefficient drops by more than half. The predicted rate of board meeting attendance is only slightly diminished by the additional controls. Nevertheless, across three measures of civic participation, the effects of place size are clearly related to differences in levels of political interest and mobilization.

Yet, even when differences in interest and mobilization are considered, the negative effects of city size on participation remain, particularly for contacting officials and attending board meetings. Does this mean that city size has other effects not considered here?

This is a difficult question to answer. The measures of citizen interest and mobilization are crude indicators for the concepts outlined in the civic voluntarism model. A better test would be measures of trust, levels of contact with neighbors, and more detailed items on mobilization for each particular civic act. One may also wonder about other contextual factors that are correlated with city size. For instance, participation may be lower in larger places because of fewer intermediary institutions to facilitate involvement, such as neighborhood groups or civic clubs (Berry, Portney, and Thomson 1993). Unfortunately, no data are available to determine whether the number of such groups varies as a function of city size. It is quite plausible that other factors underlie the city size effect, but uncovering them awaits further research with better data.

CONCLUSION

Residents of larger places are much less likely to participate in a variety of local civic activities than are people in small communities. Between the smallest and largest places, the predicted rate of contacting officials falls by 16 percentage points, attendance at community and organizational meetings declines by more than 8 percentage points, and the reported frequency of voting in local elections drops substantially. Subsequent analyses demonstrate at least two sources for the place size effect. First, people in larger communities are less likely to be mobilized for political activity, particularly by neighbors or acquaintances. The absence of mobilization stems partly from the character of social relations in larger places: As city size increases, people are less likely to know their neighbors and less likely to

TABLE 5. The Effects of City Size on Local Civic Participation, with Controls for Political Interest and Mobilization and Individual and Contextual Characteristics

	Contact	Attend	Attend	Vote
	Officials	Board Meeting	Organization Meeting	Local Elections
City-level variables				
Ćrty stze	534* (.282)	-1.34** (.338)	239 (.25 7)	026 (.12 5)
Med. household income	435 (.403)	716 (.475)	9 42* (.375)	−.567 ** (.183)
Percentage black	.443 (.370)	1.46** (.447)	731 (.3 45)	.173 (.164)
Local political Interest	.676** (.075)	.782** (.097)	.437** (.065)	.446** (.030)
Mobilized	1.47** (.122)	1.34** (.154)	.808** (.112)	.383** (.058)
Metropolitan area size ^b				
Small metro area	.204 (.159)	–.325 (.191)	0 59 (.147)	.036 (.072)
Rural	.614 (.392)	819 * (.356)	1 42 (.271)	.065 (.132)
Clty size $ imes$ rural	716 (.75 4)	2.06* (.962)	303 (. 7 12)	5 36 (.340)
Other variables				
Education	1.17** (.258)	1.10** (.318)	1.54** (.238)	.923** (.115)
Income	.309 (.228)	.614* (.280)	.835** (.210)	.175 (.102)
Age	167 (.297 <u>)</u>	– .240 (.366)	−.027 (.273)	1.78** (.128)
Homeowner	.157 (.145)	.435** (.189)	.292* (.131)	.258** (.062)
Married	.038 (.128)	−.017 (.15 8)	.047 (.111)	.107 (.055)
Black	−.227 (.172)	−.487 * (<i>.</i> 220)	.309* (.154)	.029 (.073)
Female	25 6* (.113)	202 (.138)	−.080 (. 105)	.052 (.051)
Length of residence	.121 (.206)	.281 (.265)	.189 (.184)	.162 (.089)
South	155 (.13 7)	.29 5 (.170)	−.085 (.124)	143** (.061)
Cox & Snell R ²	.25	.19	.19	$(r^2.38)$
N	2,025	1,907	2,031	2,014

Source: See source note for Table 1.

have social contacts that are geographically proximate (Fischer 1982). In this environment, local organizations and political movements find it hard to recruit members and disseminate information, which limits many opportunities for participation. Second, despite the greater visibility and stakes of local politics, people in larger places are less interested in local affairs. This may arise from a psychological response to the complex urban environment, namely, withdrawal into a more private-regarding orientation. None of these explanations is mutually exclusive, and all illustrate the general principle that as city size grows, people are less socially connected with their neighbors, less interested in local politics, and less active in local civic affairs.

These results have several implications for research on participation and urban political theory. First, social context has a large and independent effect on civic participation. Most studies of American political participation use random samples to validate models of individual behavior, but people do not live in randomized relation to one another but in specific social and political settings. These settings influence participation in systematic and predictable ways. Indeed, the variations in political participation between the smallest and largest places are often greater than differences between high school and college graduates, homeowners and renters, or single and married people. By using multilevel data that contain information on both the individual and aggregate level, a broader range of factors can be estimated. Given the magnitude of these findings, future research should pay closer attention to

differences in social and political contexts when analyzing determinants of civic participation.

Second, city boundaries are very important for creating community in an era of metropolitan expansion. One might not expect a rural town such as Muleshoe, Texas, to have the same civic culture as a similarly sized suburb of New York, such as Ho-Ho-Kus, New Jersey. After all, Ho-Ho-Kus residents have a larger surrounding social universe from which to draw. Yet, I find no interaction between metropolitan or rural area and city size in shaping participation, once other demographic characteristics are taken into account. The model predicts that residents of Ho-Ho-Kus and Muleshoe are equally more likely to participate in civic life than are New Yorkers. In the American metropolis, municipal boundaries are as important for defining the social interaction and psychological orientation of residents as they are for demarcating size. Despite the fact that boundaries in many metropolitan areas are invisible amid a continuous urban sprawl, they nevertheless influence the behavior of the residents within them.

These results suggest that Dahl's speculations about the ideal-sized democratic city are correct: People in smaller places are more civically involved. But if this is the case, then why has America's migration to smaller places coincided with a putative decline in civic involvement? Although I do not have data to estimate the effect of suburbanization over time, the results of the cross-sectional analysis and my other research with these data can provide a partial answer. On the one hand, breaking up large urban areas into collections of

^{**}p < 01, *p < .05; standard errors are in parentheses

Coefficients from OLS regression.

Excluded category is metropolitan areas of more than one mallion.

small and medium-sized municipalities should create communities with more socially, psychologically, and civically involved citizens. On the other hand, smaller places in a metropolitan context are rarely uniform in social composition. In fact, they have strong incentives to differentiate themselves in the types of services they provide and the residents they seek to attract (Peterson 1981; Tiebout 1956). Such incentives are a primary cause for the drastic social and economic segregation within many metropolitan areas (Weiher 1991). In other work, I find that the civic consequences of this racial and economic segregation generally are not positive (Oliver 1999). For example, residents of economically homogeneous municipalities are less civically engaged than people in more heterogeneous places. These tendencies occur with racial segregation as well; as was illustrated in Table 2, people in predominantly white cities are less likely to attend board meetings or vote in local elections.

In the contemporary American metropolis, subdividing populations into smaller political units alone will not be sufficient to stimulate civic involvement because the racial and economic segregation that accompanies such fragmentation will counteract the civic virtues of smaller city size. Democratic theorists who speculate about city size in the contemporary metropolis must take into account the civic consequences of social and economic segregation. The ideal democratic city may not just be of a particular size, but of a sufficient social and economic diversity as well.

APPENDIX: CODING OF THE VARIABLES

The data in this article come from two separate sources. The variables measuring participation, political interest, mobilization, and individual demographic variables come from the 1990 Citizen Participation Study (Verba et al., 1995). The contextual variables come from the 1990 Census of Population and Housing (U.S. Bureau of the Census 1991). The coding is described below.

Dependent Variables

These variables were coded with dichotomous, yes/no responses.

Contacting Local Officials. In the past 12 months...have you initiated any contacts with an elected official on the state or local level—a governor or mayor or a member of a city or town council-or someone on the staff of such an elected official?

Attending Community Board Meetings. Have you attended a meeting of (any official local governmental board or council that deals with community problems and issues, such as a town council, a school board, a zoning board, a planning board, or the like) in the past 12 months?

Attending Voluntary Organization Meetings. (Respondents identified a voluntary organization to which they belonged.) Here is a list of things that people sometimes have to do as part of the involvement with organizations. After I read each one, please tell me whether or not you have engaged in that activity as part of your involvement with this organization. Have you (gone to a meeting)?

Mobilization. (A positive response to either of the following questions was scored as positive.) In the past 12 months, have you received any request directed to you personally asking you to contact a government official—asking you to write to or talk to a government official? and serve on community board or council?

The following variables were scored as indicated.

Voting. A five-point voting-in-local-elections scale was based on responses to the following question: In the past five years, how often have you voted in elections for local or city officials? (1 = never, 2 = rarely, 3 = sometimes, 4 = often,5 = always).

Political Interest. (The political interest variables were drawn from two questions.) Thinking about your local community, how interested are you in local community politics and local community affairs? How interested in national politics and national affairs are you? (1 = not at all interested, 2 = slightly interested, 3 = somewhat interested, 4 = very interested).

Independent Variables

Education. The six categories were: less than 8 years of schooling, 8-12 years, high school diploma, some college, college degree, and advanced degree. The coding was from 0 (less than 8 years) to 1 (advanced degree).

Income. The eight categories were: less than \$7,500 annually; \$7,500 to \$15,000; \$15,000 to \$25,000; \$25,000 to \$35,000; \$35,000 to \$50,000; \$50,000 to \$75,000; \$75,000 to \$125,000; more than \$125,000. The coding was from 0 (less than \$7,500) to 1 (more than \$125,000).

Age. Recording from the original score of 18-92 years yielded a range from 0 (18 years old) to 1 (92 years old).

Length of Residence. 0 = less than 2 years, 1 = 2 or more years.

South. 0 = live outside South; 1 = live in Arkansas, Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, or Virginia.

Contextual Variables

City Size. The ten categories were: less than 2,500; 2,500 to 5,000; 5,000 to 10,000; 10,000 to 25,000; 25,000 to 50,000; 50,000 to 100,000; 100,000 to 250,000; 250,000 to 500,000; 500,000 to one million; more than one million. Size was recorded on a 0-1 scale.

Median Household Income. Rounded and originally coded on a 50-point scale (0 = <\$10,000; 1 = \$11,000; ... 49 = more than \$59,000), the recoding ranged from 0 (less than \$10,000) to 1 (more than \$59,000).

Percentage Black. Recoding from 0 to 97 yielded 0 (0% black) to 1 (97% black).

Rural. Residence in rural area = 1, in metropolitan area = 0.

Small Metropolitan Area. Metropolitan area with population of less than one million = 1, more than one million = 0.

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In Defense of Unanimous Jury Verdicts: Mistrials, Communication, and Strategic Voting

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The requirement of unanimous jury verdicts in criminal trials is widely believed to reduce the likelihood of convicting the innocent. This belief depends largely upon the assumption that jurors will vote nonstrategically based on their impression of the trial evidence. Recent literature, however, has questioned this assumption, and Feddersen and Pesendorfer propose a model in which it is never a Nash equilibrium for jurors to vote nonstrategically under unanimity rule, and equilibrium behavior produces higher probabilities of both convicting the innocent and acquitting the guilty under unanimity rule than under numerous alternatives. I extend this work by incorporating two additional features of actual jury procedure: the possibility of mistrial and communication among jurors. Under each circumstance, I demonstrate that nonstrategic voting is a Nash equilibrium under fairly general conditions and that unanimity performs better than any alternative rule in minimizing probability of trial error and maximizing expected utility.

t is a widely held belief among legal theorists that the requirement of unanimous jury verdicts in criminal trials reduces the likelihood of convicting an innocent defendant. This belief is, to a large extent, dependent upon the assumption that all jurors will vote nonstrategically, that is, will not take strategic voting issues into consideration, and therefore the jury decision will depend only upon interpretation of the evidence presented at trial. Recent literature suggests, however, that this assumption may be inconsistent with Nash equilibrium behavior and thus calls into question the supposed benefits of unanimous jury verdicts.

The use of juries in criminal trials is based, at least in part, upon the belief that, when all individuals possess a common preference for selecting the "better" of two alternatives (in this case, conviction or acquittal), a group is more likely than any single individual to select the preferred option. This is the central argument behind the extensive literature based on Condorcet's Jury Theorem (Condorcet [1785] 1976; Grofman and Feld 1988; Klevorick, Rothschild, and Winship 1984; Miller 1986; and Young 1988). Analysis and extensions of this theorem have generally been statistical and take individual probabilities of correct decisions to be exogenously determined (Berg 1993; Ladha 1992, 1993, 1995). Implicit in this approach is the assumption that individuals behave in the same manner whether acting as a dictator or participating in a group decision. In the framework of juries, this is equivalent to assuming that a juror's vote depends exclusively on her own private information (and perhaps shared public information) about the trial, and does not depend upon considerations of strategic interaction within the jury.

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Recently, however, Austen-Smith and Banks (1996) illustrate that such nonstrategic voting in group decisions may be inconsistent with Nash equilibrium behavior under fairly general conditions. In response, McLennan (1998) and Wit (1998) have attempted to rehabilitate the central notion of Condorcet's theorem by identifying reasonable conditions under which Nash equilibrium behavior, although it may be inconsistent with nonstrategic voting, still predicts that groups are more likely to make correct decisions than individuals.

Feddersen and Pesendorfer (1998) adapted the general framework of Austen-Smith and Banks to the specific case of jury procedures in criminal trials and derived some surprising results about unanimous jury verdicts. In their model it is never a Nash equilibrium for all jurors to vote nonstrategically under unanimity rule. Moreover, Nash equilibrium behavior will lead to higher probabilities of both convicting the innocent and acquitting the guilty under unanimity rule than under a wide variety of alternatives, including simple majority rule. They conclude that the societal objective of avoiding such jury errors may be better served by eliminating the requirement of unanimous verdicts in criminal cases.

I extend the Feddersen and Pesendorfer model with two minimal enhancements that bring it closer to actual jury procedure. In particular, I separately analyze the implications of (1) incorporating the possibility of mistrial and (2) allowing limited communication among jurors. For each of these, I identify general conditions under which nonstrategic voting is, in fact, a Nash equilibrium and demonstrate that the conclusion of the inferiority of unanimous jury verdicts does not persist. That is, when either mistrial or limited communication is introduced, it is no longer the case that unanimous jury verdicts generally produce equilibrium probabilities of convicting the innocent and acquitting the guilty that are higher than under alternative voting rules. Moreover, within the nonstrategic voting equilibria that exist under these model enhancements, unanimity rule maximizes ex ante expected utility for all jurors.

THE BASIC MODEL

I begin with the basic model of jury procedure analyzed by Feddersen and Pesendorfer (1998) and, more generally, by Austen-Smith and Banks (1996). It will serve as a point of departure and source of comparison for the new jury models I introduce.

Basic Theoretical Framework

There are n jurors who will vote to determine the fate of a defendant. The set of jurors is denoted by $N = \{1, 2, \ldots, n\}$, with an individual juror being represented by $j \in N$. There are two possible states of the world: The defendant is either guilty or innocent. I denote by G the state of the world in which the defendant is guilty and by I the state in which the defendant is innocent. The prior probability of G is given by the parameter r, and the prior probability of I is 1 - r.

Feddersen and Pesendorfer simplify the problem by assuming that the two states occur with equal probability (r = 0.5). This assumption does not constitute a significant theoretical restriction, but it does complicate interpretation of the assumptions and results. This is because, in actual practice, it is likely that the prior probability of guilt is greater than 0.5 and perhaps significantly so.

To see this, consider the very high frequency with which defendants are found guilty in criminal trials. Conviction rates in federal courts, for example, have been steadily increasing for decades, and in 1995, federal criminal juries found 82% of all defendants guilty (Vidmar et al. 1997). The frequency of guilty verdicts is also very high in state courts. In recent years, the conviction rate has averaged approximately 84% in Texas, 82% in California, 72% in New York, 67% in North Carolina, and 59% in Florida (Vidmar et al. 1997).

For both models of jury procedure introduced here, such high conviction rates would be impossible if the value of r, the prior probability of guilt, were equal to 0.5. In the first new model I introduce, for example, the ex ante probability of conviction always lies between 0.5 and the value of r for all parameter combinations. Thus, a value of r significantly higher than 0.5 may be more appropriate, at least for some jurisdictions. In the theoretical results of this article, I will therefore allow r to take on any value between 0 and 1, but I also will discuss the more simplified results that arise when r = 0.5. In the specific examples presented, I will examine the cases r = 0.5 and r = 0.8.

In the basic model, there are two possible outcomes of the jury vote: The defendant is convicted (C) or acquitted (A). Each juror can either vote for convictions.

tion or acquittal. All votes are done by secret ballot, and no abstentions are allowed. I will represent by |C| the total number of votes for conviction and by |A| the total number of votes for acquittal. In addition, $|C|_{-j}$ will denote the number of votes for conviction among all jurors other than j, or $N/\{j\}$, and $|A|_{-j}$ will denote the number of votes for acquittal among $N/\{j\}$.

A voting rule is described by a threshold \hat{k} , which is an integer between 0 and n. If $|C| \ge \hat{k}$, then the defendant is convicted; otherwise, the defendant is acquitted. Unanimity rule is represented by the voting rule $\hat{k} = n$, and simple majority rule is represented by the voting rule with \hat{k} equal to the smallest integer greater than n/2.

The influence of the trial evidence is represented by a private signal received by each juror. I will denote by s_i the signal received by juror j. There are two possible signals, g or i, and the signal is correlated with the true state of the world. In particular, for all j, $Prob(s_j = g|G) = Prob(s_j = i|I) = p \in (0.5, 1.0)$. Thus, p is the probability that a juror receives the "correct" signal (g in state G or g in state g, and g in state g or g in state g. I will denote by g the total number of g signals received and by g the total number of g signals received. In addition, g in denotes the number of g signals among g in g in g denotes the number of g signals among g in g in g denotes the number of g signals among g in g in

Although juror signals are drawn independently given the true state of the world, they are correlated to each other in the sense that $\operatorname{Prob}(s_j = g|s_i = g) = \operatorname{Prob}(s_j = i|s_i = i) = p^2 + (1-p)^2 > 1/2 > 2p(1-p) = \operatorname{Prob}(s_j = g|s_i = i) = \operatorname{Prob}(s_j = i|s_i = g)$. In other words, juror i's signal provides her information about juror j's signal; in particular, she believes that juror j is more likely to have a signal that matches her own than one that does not.

I will denote by $\beta(k, n)$ the posterior probability that the defendant is guilty conditional on k of n guilty signals:

$$\beta(k, n) = \frac{rp^{k}(1-p)^{n-k}}{rp^{k}(1-p)^{n-k} + (1-r)p^{n-k}(1-p)^{k}}.$$

Let $u_j(O, S)$ be juror j's utility given outcome O in state S. The utility of the "correct" trial outcomes (i.e., conviction when the defendant is guilty, acquittal when innocent) are normalized such that $u_j(C, G) = u_j(A, I) = 0$. It is further assumed that the utility of the "incorrect" trial outcomes (i.e., conviction when the defendant is innocent, acquittal when guilty) are given by $u_j(C, I) = -q_j$, and $u_j(A, G) = -(1 - q_j)$, where $q_j \in (0, 1)$. Under this construction, any juror j will prefer conviction to acquittal whenever she believes the probability that the defendant is guilty is greater than q_j . In this sense, $1 - q_j$ is a measure of what j considers to be "reasonable doubt."

We should expect any juror j to have $q_j > 0.5$. To see this, recognize that the "more probable than not" standard of proof employed in most civil trials is equivalent to $q_j = 0.5$ for all $j \in N$. In contrast, the "beyond a reasonable doubt" standard used in criminal

¹ The proof of this claim about the mistrial model is included in the Appendix. I also prove in the Appendix that the high conviction rates observed cannot be supported with r=0.5 in the communication model, because the ex ante probability of conviction is always less than or equal to 0.5 in this case. For the basic model, it is difficult to develop such clear relationships between the value of r and the ex ante probability of conviction without imposing significant restrictions on the values of other parameters.

trials is a strictly higher standard of proof and therefore requires $q_j > 0.5$ for all $j \in N$. In particular, any j with $q_j < 0.5$ would prefer to convict even in some cases in which he believes the defendant is more likely innocent than guilty. It is possible that such jurors exist, but a specific purpose of the jury selection process is to eliminate candidates with such preferences. Therefore, in the examples presented here, I will usually assume that $q_j \in (0.5, 1.0)$ for all $j \in N$.

The basic model presented by Feddersen and Pesendorfer assumes common utilities for all jurors (i.e., $q_i = q_j$ for all $i, j \in N$), although this assumption may have been made purely for technical convenience. To assure the generality of results in this article, I will use individual utilities in all the analyses.

The behavior of a given j in the basic model is described by a strategy mapping, $\sigma_j:(0, 1) \times \{g, i\} \rightarrow [0, 1]$, with $\sigma_j(q_j, s_j)$ being the probability of voting to convict given utility parameter q_j and signal s_j . Using this notation, I will define two different nonstrategic voting strategies: informative voting and sincere voting.

Informative voting is defined as voting to convict whenever a guilty signal is received and voting to acquit whenever an innocent signal is received. In other words, to vote informatively is simply to "vote your signal" and thus honestly reveal your private information. The informative voting strategy for j is given by:

$$\sigma_{J}(q_{J}, s_{J}) = \begin{cases} 1 & \text{if } s_{J} = g \\ 0 & \text{if } s_{J} = i \end{cases}$$

Informative voting is not only nonstrategic but also naive, since voting only according to one's signal may be inconsistent with expected utility maximization for some jurors. Thus, I also define sincere voting. A strategy for j is considered sincere when it consists of voting for the trial outcome that maximizes her expected utility conditional on her signal (and perhaps any other revealed signals). The general form of the sincere voting strategy for j is given by:

$$\sigma_{J}(q_{J}, s_{J}) = \begin{cases} 1 \text{ if } q_{J} < \operatorname{Prob}(G) \\ 0 \text{ if } q_{J} \ge \operatorname{Prob}(G) \end{cases}$$

I will contrast these nonstrategic voting strategies with strategic voting, which I call rational voting. It consists simply of voting according to Nash equilibrium behavior. Rational voting thus requires a juror to vote for the trial outcome that maximizes her expected utility conditional on her signal and conditional on her vote being pivotal (i.e., her vote can change the trial outcome). A rational juror must vote as if her vote is pivotal because this is the only case in which her vote will ever affect her utility. In the basic model, rational voting thus means voting as if exactly k-1 other jurors are voting to convict.

For a given voting rule, there may be conditions under which rational voting is equivalent to informative and/or sincere voting, but it may also be the case that these voting strategies do *not* coincide. One important result to recognize, however, is that whenever rational voting is equivalent to informative voting (i.e., whenever there exists a Nash equilibrium in which all

jurors "vote their signal"), sincere voting will also be rational.

Assumptions and Conclusions of the Basic Model

Feddersen and Pesendorfer make several assumptions to eliminate potential equilibria that do not satisfy certain normative criteria. In particular, they do not consider asymmetric equilibria and equilibria in which a juror's strategy is independent of the signal received. Also, certain restrictions are placed upon the relationship between the parameters p and q_j . In particular, it is assumed for all $j \in N$ that $1 - p \le q_j \le \beta(n-1,n)$. The lower bound on q_j here is not particularly restrictive, since it is generally assumed that q_j is greater than 0.5, which is greater than 1-p. The upper bound on q_j is also relatively permissive. This bound says only that, for a jury of size n > 1, n - 1 guilty signals (versus only one innocent signal) is sufficient information for all jurors to prefer conviction.

With these assumptions, Feddersen and Pesendorfer demonstrate that, under unanimity rule, there does not exist a Nash equilibrium in which all jurors vote informatively or sincerely and that there is instead a unique mixed strategy Nash equilibrium. They also identify the unique Nash equilibrium for nonunanimous voting rules and illustrate that, as the size of the jury increases toward infinity, equilibrium behavior under unanimity rule leads to higher probabilities of both convicting the innocent and acquitting the guilty than under nonunanimous voting rules. Feddersen and Pesendorfer also use the following example to demonstrate that the inferiority of unanimous jury verdicts, while primarily a limit result, can also hold for smaller juries under fairly reasonable conditions.

Example 1. Let n = 12, r = 0.5, p = 0.8, and $q_j = 0.9$ for all $j \in N$. In this scenario, the probability of each type of trial error under different voting rules is shown in Table 1. Thus, the probability of convicting an innocent defendant and the probability of acquitting a guilty defendant are both higher under unanimity rule (k = 12) than under any other majority or supermajority voting rule. In addition, the combined probability of trial error decreases as the voting rule threshold (k) is decreased, achieving a minimum under simple majority rule (k = 7).

The key to understanding these somewhat surprising results is to recognize the significant influence that being pivotal can have on juror strategies. For example, in the case of unanimity rule, conditioning on being pivotal means that each juror behaves as if all other jurors are voting to convict the defendant. It is therefore not difficult to see that, regardless of one's own signal, being pivotal provides a strong incentive to vote for conviction in this case, since all other jurors are doing the same. For nonunanimous rules, being pivotal may provide much less compelling information. Under simple majority rule (with n odd), for example, being pivotal means only that an equal number of the other

Voting Rule (k)	7	8	9	10	11	12
Probability an innocent defendant is convicted	0.0040	0.0011	0.0025	0.0045	0.0066	0.0069
Probability a gulity defendant is acquitted	0.0190	0.0660	0.1350	0.2450	0.4200	0.6540
Combined probability of trial error	0.0115	0.0336	0.0688	0.1248	0.2133	0.3305

jurors are voting in each direction. This information is not overwhelming for either guilt or innocence and therefore can be expected to have much less influence on juror voting.

To see why informative voting is not a Nash equilibrium under unanimity rule, suppose that all jurors vote informatively and consider the situation in which i receives an innocent signal $(s_i = i)$. It is easy to see that j has a positive incentive to deviate from informative voting and instead vote to convict. Since i will condition his vote on being pivotal, he will behave as if all other jurors are voting to convict. When jurors vote informatively, this means that all other n-1 jurors received guilty signals and that j received the only innocent signal. The probability of guilt perceived by jis therefore $\beta(n-1, n)$ in this case. By assumption, however, $q_i \leq \beta(n-1, n)$, and thus j prefers conviction to acquittal. Hence, j has an incentive to vote contrary to his own signal, and therefore informative voting is not a Nash equilibrium under unanimity rule in the basic model.

THE MISTRIAL MODEL

The first significant limitation of the basic model involves the delineation of trial outcomes. The model assumes only two possible outcomes of the jury process: conviction or acquittal. Under unanimity rule, for example, a defendant is convicted if and only if all jurors vote for conviction; otherwise, the defendant is acquitted. In actual practice, however, almost all jurisdictions require unanimity for either conviction or acquittal in a criminal trial (Schwartz and Schwartz 1992). Without a unanimous vote, there is a hung jury. If the hung jury situation persists through deliberations, a mistrial is declared, and a new trial can be expected to take place. If the jury process is represented by a single vote, then any nonunanimous vote will immediately result in a mistrial.

Informative and Sincere Equilibria with Exogenous Mistrial Utilities

Consider an enhancement to the basic model in which there are three possible outcomes of the jury process: conviction (C), acquittal (A), or mistrial (M). A voting rule is still described by an integer threshold \hat{k} . If $|C| \ge \hat{k}$, then the defendant is convicted; if $|A| \ge \hat{k}$, then the defendant is acquitted; otherwise, a mistrial is de-

clared. Note that \hat{k} must again be less than or equal to n but must now also be strictly greater than (n+1)/2. This lower bound on \hat{k} exists in the mistrial model because if $\hat{k} \leq (n+1)/2$, then the trial outcome may be indeterminate in some cases or a mistrial may be an impossibility (which occurs when n is odd and $\hat{k} = (n+1)/2$).

Let $u_j(M, G) = -m_j^G$ and $u_j(M, I) = -m_j^I$. I will assume that the utility of a mistrial is strictly between the utilities of acquittal and conviction. That is, $0 < m_j^I < q_j$, and $0 < m_j^G < (1 - q_j)$. In the next section, I will endogenize these mistrial utilities by equating them with the expected value of a new trial in a repeated trial process. For now, however, it is instructive to consider these mistrial utilities as exogenously determined.

It should be noted that Schwartz and Schwartz (1992) also analyzed the effect of alternative voting rules within a model of jury procedure that allows for the possibility of mistrial. This model, however, takes a very different approach: Jurors have single-peaked preferences over a range of possible charges, and the key choice variable is the prosecutorial decision about which charge (or charges) to prosecute.

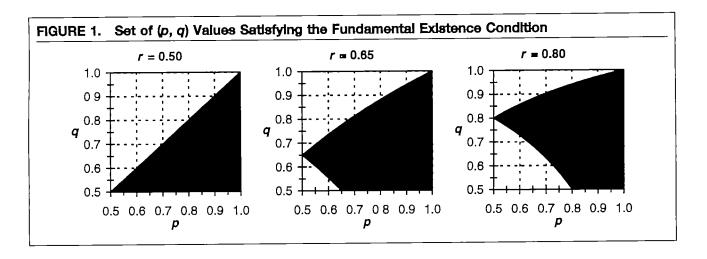
The first result in the analysis of my mistrial model presents the necessary and sufficient conditions for informative voting to be a Nash equilibrium.

PROPOSITION 1. Informative voting is a Nash equilibrium in the mistrial model if and only if, for all jurors $j \in N$, we have:

$$\begin{split} \frac{r}{1-r} \cdot \frac{1-p}{p} \\ \leq \frac{(q_j - m_j^I)(1-p)^{2\hat{k}-n-1} + m_j^I p^{2\hat{k}-n-1}}{(1-q_j - m_j^G)(1-p)^{2\hat{k}-n-1} + m_j^G p^{2\hat{k}-n-1}} \\ \leq \frac{r}{1-r} \cdot \frac{p}{1-p}. \end{split}$$

Proof. See Appendix.

It is difficult to assess immediately the generality of proposition 1 due to the complexity of the existence condition. Nonetheless, several simple observations will illustrate both the significance of the result and the generality of its scope. The first important point is that once the possibility of mistrial is added to the basic model, the conditions under which informative voting



is a Nash equilibrium become much more permissive. In particular, informative voting Nash equilibria do exist under unanimity rule in the mistrial model, which is not true for the basic model.

The complexity of the existence condition in proposition 1 is driven primarily by the generality of my formulation of the model to this point. Because I have introduced seven different variables $(k, m_l^G, m_l^I, n, p, q_l)$, and r) to characterize the jury decision process, proposition 1 must delineate the set of values for all these variables that will permit an informative voting Nash equilibrium. This approach allows proposition 1 to apply to a wide variety of trial systems and juror preferences, but it also results in a very intricate existence condition. As I will now demonstrate, once even the most simple restrictions are placed on these variables, the existence condition is greatly simplified.

For example, consider a simple restriction on the mistrial utilities, m_j^I and m_j^G . By previous assumption, $0 < m_j^I < q_j$ and $0 < m_j^G < 1 - q_j$. Suppose, then, that $m_j^I = \alpha q_j$ and $m_j^G = \alpha (1 - q_j)$ for some $\alpha \in (0, 1)$, where the precise value of α may be a function of the parameters k, n, p, q_j and r. Under this assumption, the existence condition in proposition 1 immediately reduces to:

$$\frac{r}{1-r} \cdot \frac{1-p}{p} \leq \frac{q_j}{1-q_j} \leq \frac{r}{1-r} \cdot \frac{p}{1-p}.$$

This far simpler existence condition will reappear several times in the discussion, and I therefore refer to it as the fundamental existence condition. Because of the importance of this condition, it is helpful to discuss its interpretation.

The fundamental existence condition is equivalent to the following condition: $\beta(0, 1) \leq q_j \leq \beta(1, 1)$ for all $j \in N$. This constraint can be interpreted as a "oneman jury condition," because it is the same condition that would be required for a one-man jury (or, more appropriately, a presiding judge) to render a meaningful verdict. To see this, consider a jury that consists of a single j. If $q_j < \beta(0, 1)$, then the standard for conviction is so low that conviction is preferred even when the one and only signal is innocent; thus, all defendants will be convicted, no matter which signal is

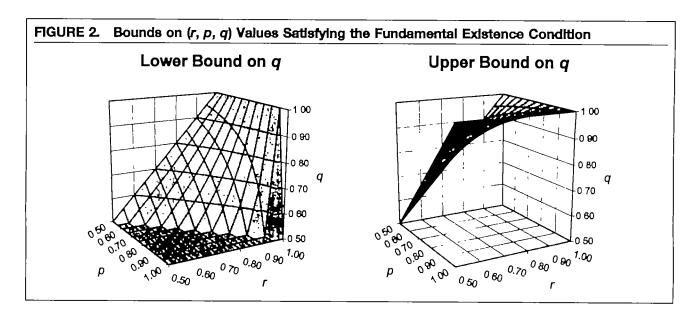
received by j. Similarly, if $q_j > \beta(1, 1)$, then the standard for conviction is so high that acquittal is preferred even when the one and only signal is guilty; thus, all defendants will be acquitted, no matter which signal is received by j. In order for this one-man jury ever to render a meaningful verdict (i.e., one that varies depending upon what actually happens at trial), it must be the case that $\beta(0, 1) \leq q_j \leq \beta(1, 1)$.

The simplifying assumption I have imposed applies only to the two mistrial utilities, but I have nonetheless reduced the number of variables in the existence condition from seven to three. With just three variables, I can now graphically illustrate the set of values that permit an informative voting Nash equilibrium in the mistrial model. The shaded areas in Figure 1 depict, for three different values of the parameter r, the set of (p, q) values that satisfy the fundamental existence condition.

Figure 1 illustrates some interesting characteristics of the fundamental existence condition. First, for a given value of p, the existence condition requires that q values not be above a certain threshold and may also require that these q values not be below another threshold. In this way, the fundamental existence condition excludes juror q values that are too extreme in either direction. As further demonstration, Figure 2 illustrates, in a three-dimensional representation, the upper and lower bounds on q imposed by the fundamental existence condition.

Second, see from figures 1 and 2 that the range of q values which satisfies the fundamental existence condition becomes larger as the value of p increases. Since p represents the probability that the true state of the world is revealed at trial, this indicates that the fundamental existence condition is more easily satisfied as the "accuracy" of trials increases.

More straightforward existence conditions are found if we instead examine conditions that are simply sufficient (but not necessary) for informative voting to be a Nash equilibrium in the mistrial model. Many different conditions will guarantee existence, but one set of sufficient conditions is the following:



$$\frac{r}{1-r} \cdot \frac{1-p}{p} \le \frac{q_j}{1-q_j} \le \frac{r}{1-r} \cdot \frac{p}{1-p},$$

$$\frac{r}{1-r} \cdot \frac{1-p}{p} \le \frac{m_j^T}{m_j^G} \le \frac{r}{1-r} \cdot \frac{p}{1-p}.$$

Note the reappearance of the fundamental existence condition as well as the presence of a similar restriction on the mistrial utilities, m_j^I and m_j^G (I no longer assume that $m_j^I = \alpha q_j$ or that $m_j^G = \alpha (1 - q_j)$). It is important to recognize, however, that many

It is important to recognize, however, that many parameter values satisfy the conditions of proposition 1 (and therefore support an informative voting Nash equilibrium) yet do not satisfy the fundamental existence condition or the easy-to-understand sufficient conditions specified above. For example, consider example 1 used by Feddersen and Pesendorfer (1998), in which n = 12, r = 0.5, p = 0.8, and $q_j = 0.9$ for all j (note that this example violates the fundamental existence condition). Under unanimity rule in this case, the existence condition of proposition 1 reduces approximately to:

$$\frac{1}{4} < \frac{m_j^I}{m_j^G} < 4.$$

This means that informative or sincere voting will be a Nash equilibrium for example 1 as long as the utility (or disutility) of one mistrial outcome is not more than four times as large as the utility (or disutility) of the other mistrial outcome. Thus, a wide range of parameter values will permit an informative voting Nash equilibrium for this example.

It is also important to recognize that any conditions sufficient for the existence of an *informative* voting Nash equilibrium, such as the condition in proposition 1 and the simpler sufficiency conditions specified above, also represent sufficient conditions for the existence of a *sincere* voting Nash equilibrium. This is true because, as discussed previously, whenever informative

voting constitutes a Nash equilibrium, sincere voting will also constitute a Nash equilibrium.

The complete proof of proposition 1 is given in the Appendix, but it is helpful to understand the basic intuition behind the result. The key element in the proof that distinguishes the predictions of the mistrial model and the basic model is the understanding of what it means to be pivotal in the two different models. To illustrate, consider the case of unanimity rule. In the basic model under unanimity rule, a juror is pivotal only when all other jurors are voting to convict. When the other jurors are voting informatively, this provides a strong incentive to vote for conviction, even for those jurors who receive an innocent signal. In the mistrial model, a juror is pivotal in two different cases: when all other jurors are voting to convict and when all other jurors are voting to acquit. Thus, the fact that a juror is pivotal in the mistrial model does not necessarily 'push" this juror toward one particular outcome (as being pivotal in the basic model pushes jurors toward conviction). Moreover, given an innocent signal in the mistrial model, a juror will believe it is more likely that all other jurors are voting to acquit than that all other jurors are voting to convict. This provides such a juror a greater incentive to vote informatively. The same is true for jurors who receive a guilty signal.

Informative and Sincere Equilibria with Endogenous Mistrial Utilities

I will endogenize the mistrial utilities, m_j^G and m_j^I , by specifying juror perceptions about the consequences of mistrial. These perceptions may incorporate many different factors, but it seems reasonable to model the utility of mistrial as simply the expected utility of an additional trial before a new jury.² In other words:

² I also could discount the expected utility of future trials or apply a fixed cost/disutility to each new trial. Mistrial utilities incorporating these factors still allow calculation of the refined necessary and

$$\begin{split} m_j^G &= (1 - q_j) \cdot \operatorname{Prob}_{\mathbf{s}}(A|G) + m_j^G \cdot \operatorname{Prob}_{\mathbf{s}}(M|G), \\ m_j^I &= q_j \cdot \operatorname{Prob}_{\mathbf{s}}(C|I) + m_j^I \cdot \operatorname{Prob}_{\mathbf{s}}(M|I), \end{split}$$

where $\operatorname{Prob}_{\bullet}(O|S)$ is the probability of outcome O in a single trial when the true state is S.

When the utility of mistrial is specified in this manner, the conditions for the existence of an informative voting Nash equilibrium are simplified significantly.

PROPOSITION 2. Suppose the utility of mistrial is equal to the expected utility of an additional trial before a new jury. Informative voting is then a Nash equilibrium in the mistrial model for any voting rule \hat{k} if and only if, for all $j \in N$, we have:

$$\frac{r}{1-r} \cdot \frac{1-p}{p} \leq \frac{q_j}{1-q_j} \leq \frac{r}{1-r} \cdot \frac{p}{1-p}.$$

Proof. See Appendix.

With the mistrial utilities determined endogenously, the condition for existence of an informative voting Nash equilibrium reduces to the now familiar fundamental existence condition. Therefore, the set of (p, q) values that permit an informative equilibrium are illustrated by figures 1 and 2.

As with proposition 1, the inequality condition of proposition 2 represents not only a necessary and sufficient condition for the existence of an informative voting Nash equilibrium but also a sufficient condition for the existence of a sincere voting Nash equilibrium. Furthermore, proposition 2 again suggests that the occurrence of informative and sincere voting among jurors may increase as the "accuracy" of trials improves. As p increases, and thus trials become more truth revealing, the conditions of propositions 1 and 2 become easier to satisfy; hence, informative and sincere voting Nash equilibria will exist for more juries and more trials. If such nonstrategic voting is a desirable outcome, then this result provides an additional argument for legal reforms that may improve the likelihood that the true state of the world, guilt or innocence, is revealed at trial.

The significance of proposition 2 clearly depends upon its generality, so it is appropriate to ask how likely it is that the conditions of the proposition will be satisfied in practice. That is an empirical question beyond the scope of this article, but the following example demonstrates that the conditions are met quite easily.

Example 2. Consider the jury selection process for a felony trial in California. This involves choosing 12 jurors from a large set of candidates interviewed by both the prosecution and defense.

The defense has 10 peremptory challenges to dismiss candidates deemed most likely to convict. In my model, this is equivalent to dismissing candidates with the

sufficient conditions for sincere voting, but analysis of such utility structures significantly increases the complexity of the presentation without providing much additional insight.

lowest q values. Similarly, the prosecution has 10 peremptory challenges to dismiss candidates deemed least likely to convict. In my model, this is equivalent to dismissing candidates with the highest q values.

There also is an unlimited number of dismissals for cause, which the judge uses to eliminate candidates whose probability of voting for conviction is deemed either unacceptably low or unacceptably high. In my model, for example, that would mean candidates with q values below 0.5^3 or too close to 1.0. Given this process, a jury of 12 can always be chosen from a jury pool, denoted P, consisting of the first 32 candidates not dismissed for cause.

Let r = 0.8 and p = 0.8.4 In this case, an informative voting Nash equilibrium exists if and only if $0.50 \le q_j \le 0.94$ for all $j \in N$. Suppose that the distribution of q values for the candidates (not dismissed for cause) is uniform between 0.5 and 1.0. The probability that any one candidate violates the inequality above is 0.12. This gives us:

Prob(informative voting equilibrium exists)

- = Prob($\forall j \in N, 0.50 \le q_1 \le 0.94$)
- $= \text{Prob}(|\{j \in P : q_j > 0.94\}| \le 10)$

$$= \sum_{k=0}^{10} b(k, 32, 0.12) = 99.9\%.$$

In this example, the existence condition of proposition 2 is almost always satisfied.

Comparison of Alternative Voting Rules

Once the existence of nonstrategic voting Nash equilibria is established, it is important to compare the performance of alternative voting rules in terms of equilibrium outcomes. One possible performance measure is the probability of a trial error, in other words, the probability of convicting the innocent or acquitting the guilty. Proposition 3 indicates that the probability of either type of trial error decreases as k, the number of votes required for a verdict, increases.

Proposition 3. Suppose that mistrial always results in a new trial and consider two voting rules, $\hat{\mathbf{k}}_1$ and $\hat{\mathbf{k}}_2$, with $\hat{\mathbf{k}}_1 < \hat{\mathbf{k}}_2$. If jurors vote informatively, then

- (1) the probability of convicting an innocent defendant is lower under voting rule k_2 than under voting rule k_1 , and
- (2) the probability of acquitting a guilty defendant is lower under voting rule \hat{k}_2 than under voting rule \hat{k}_1 .

Proof. See Appendix.

 $[\]overline{\ }$ The "beyond a reasonable doubt" standard of proof requires that any accepted j must have $q_j>0.$

⁴ These parameter values are consistent with empirical data. I have shown that a value of r around 0.8 is to be expected; when combined with a p value of 0.8, this yields an equilibrium conviction rate of 80%, which is consistent with the conviction data cited previously.

Proposition 3 implies that the probability of trial error is uniquely minimized by unanimity rule and uniquely maximized by simple majority rule. This is in stark contrast to the conclusions drawn from the basic model, in which Nash equilibrium behavior produces a higher probability of both types of trial error under unanimity rule than under any nonunanimous voting rule.

Another reasonable measure of the performance of alternative voting rules is expected utility. My final result for the mistrial model indicates that the expected utility for any juror increases as the number of votes required for a verdict increases.

Proposition 4. Suppose that the utility of mistrial is equal to the expected utility of an additional trial before a new jury and consider two voting rules, \hat{k}_1 and \hat{k}_2 , with $\hat{k}_1 < \hat{k}_2$. If jurors vote informatively, then the ex ante expected utility for any juror is higher under voting rule \hat{k}_2 than under voting rule \hat{k}_1 .

Proof. See Appendix.

This proposition indicates that unanimity rule again performs uniquely best among all voting rules, this time in terms of maximizing expected utility. Moreover, proposition 4 also implies that simple majority rule is again the uniquely worst voting rule under this performance measure.

Propositions 3 and 4 specifically apply to the version of the mistrial model in which mistrial utilities are determined endogenously, but the results (i.e., unanimity rule minimizing error and maximizing utility) also hold true when mistrial utilities are specified exogenously. The analysis in the exogenous utility case is rather simple, however, and the appropriate interpretation of the results is less clear.

THE COMMUNICATION MODEL

The basic model effectively rules out any communication among jurors. The entire jury process is assumed to be a single vote in which each juror has no information about the beliefs of other jurors. In actual practice, the jury process involves a significant amount of communication and information revelation, and several straw votes often are taken during deliberations.

Let us now consider an enhancement to the basic model that allows for minimal communication among jurors. In particular, suppose that the jury takes a single nonbinding straw vote before taking the final binding vote for conviction or acquittal. All jurors must vote either to convict (C) or acquit (A) in both cases, and the number of preliminary votes cast for each outcome is announced prior to conducting the final vote. It is assumed that no communication other than the preliminary vote scores takes place.

This enhancement to the model is not meant to represent actual deliberation procedures; it is intended to show the significance of including communication in any model of the jury process. Even the most minimal communication (a single nonbinding straw vote) can

significantly change the conclusions of the model analysis.

Informative and Sincere Equilibria

The analysis of this communication variant of my model begins by defining a nonstrategic strategy profile appropriate for the distinctive voting framework of the model. The sincere revelation strategy profile for the communication model consists of each *j* voting according to the following guidelines.

- (1) In the preliminary vote, j votes to convict if and only if $s_i = g$ (informative voting).
- (2) In the final vote, j votes to convict if and only if $\beta(k, n) \ge q_j$, where k is the number of votes to convict from the preliminary vote (sincere voting).

The first result identifies the necessary and sufficient conditions for the sincere revelation strategy profile to constitute a subgame perfect Nash equilibrium.

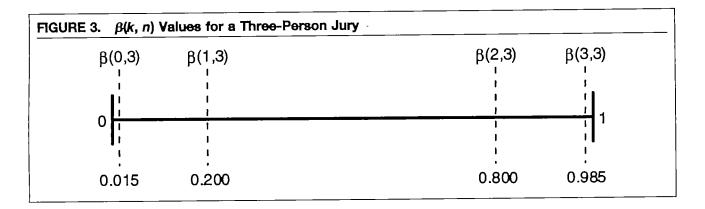
PROPOSITION 5. Let the jurors be numbered such that $q_1 \leq q_2 \leq \ldots \leq q_{n-1} \leq q_n$. The sincere revelation strategy profile is a subgame perfect Nash equilibrium for a given voting rule \hat{k} if and only if one of the following conditions is true:

- (a) $0 \le q_{\hat{k}} \le \beta(0, n)$;
- (b) $\beta(n, n) < q_k \le 1$; or
- (c) $\exists k^* \in \{1, ..., n\}$ such that $\beta(k^* 1, n) \le q_j \le \beta(k^*, n)$ for all $j \in N$.

Proof. See Appendix.

Proposition 5 states that sincere revelation is a Nash equilibrium in the communication model whenever juror utilities satisfy a certain "closeness" condition. The basic insight behind this proposition is that when juror utilities are similar enough for there to be a situation of "common interest," everyone can benefit from an honest sharing of information in the preliminary vote. Since jurors do not have competing interests, the sharing of information can only enhance the probability of achieving the outcome that all jurors prefer. In fact, the basic results of proposition 5 should hold for any game of incomplete information and common interest in which a choice must be made between two alternatives, such as between two candidates for office or two public projects.

It is important to note that the conditions in proposition 5 guarantee that the sincere revelation strategy profile will be a subgame perfect Nash equilibrium, but there will always be other equilibria in the communication model that are not informative in the preliminary vote and/or not sincere in the final vote. For example, it is always an equilibrium for jurors to vote in the preliminary vote in a way that conveys no information whatsoever about their signals and then vote according to the Nash equilibrium of the basic model in the final binding vote. There are also equilibria in which preliminary vote strategies do depend on individual signals, but jurors do not simply "vote their



signal" (they could vote directly contrary to their signal, for example).

As with the mistrial model, the significance of proposition 5 for the communication model depends to a large degree on the generality of its scope. It is therefore important to describe in more detail the different scenarios in which the existence conditions are satisfied. One simple situation that always meets the conditions of proposition 5 is the case of common utilities (i.e., when $q_i = q_j$ for all $i, j \in N$). Thus, in example 1 used by Feddersen and Pesendorfer, where $q_j = 0.9$ for all j, sincere revelation voting is a subgame perfect Nash equilibrium in the communication model under all possible voting rules.

It is possible for juror utilities to differ significantly and still satisfy the conditions of the proposition. Consider a three-person jury (n = 3), and suppose a correct signal is received 80% of the time (p = 0.8). In this case, we have $\beta(0, 3) = 0.015$, $\beta(1, 3) = 0.200$, $\beta(2, 3) = 0.800$, and $\beta(3, 3) = 0.985$, as shown in Figure 3. If all three q_j values fall between any two of the dotted lines in this figure, proposition 5 says that sincere revelation voting is a Nash equilibrium. It is thus clear that the q_j values can differ significantly yet still satisfy the condition of the proposition.

It may seem that as n increases (i.e., the size of the jury becomes larger), the difference between $\beta(k^*-1,n)$ and $\beta(k^*,n)$ will become smaller for all $k^* \in \{1,\ldots,n\}$, making the conditions of proposition 5 increasingly difficult to satisfy. This is not entirely true, however. Some of these differences remain constant (and potentially rather large) for all values of n. My next proposition uses this fact to identify sufficient conditions for the existence of a sincere voting equilibrium that are independent of jury size.

Proposition 6. For n odd, the sincere revelation strategy profile is a subgame perfect Nash equilibrium for any voting rule \hat{k} if $\forall j \in N$:

$$\frac{r}{1-r} \cdot \frac{1-p}{p} \le \frac{q_j}{1-q_j} \le \frac{r}{1-r} \cdot \frac{p}{1-p}.$$

For n even, the sincere revelation strategy profile is a subgame perfect Nash equilibrium for any voting rule \hat{k} if $\forall j \in N$:

$$\frac{r}{1-r} \cdot \left(\frac{1-p}{p}\right)^2 \le \frac{q_j}{1-q_j} \le \frac{r}{1-r} \quad or$$

$$\frac{r}{1-r} \le \frac{q_j}{1-q_j} \le \frac{r}{1-r} \cdot \left(\frac{p}{1-p}\right)^2.$$

Proof. See Appendix.

The sufficiency condition for n odd in proposition 6 is precisely the fundamental existence condition discussed previously in great detail. Therefore, the set of (p, q) values sufficient for the existence of a sincere revelation equilibrium for all odd-membered juries is again illustrated by figures 1 and 2.

For a jury with an even number of members, proposition 6 presents two different conditions, each of which is independently sufficient for the existence of a sincere revelation equilibrium. These two conditions are structured very similarly to the fundamental existence condition, but they can also be rewritten to give the following sufficient conditions:

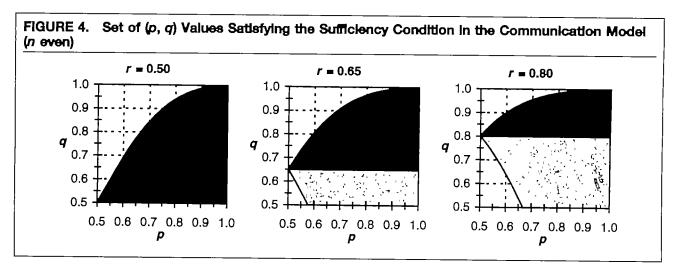
$$\frac{r(1-p)^2}{r(1-p)^2 + (1-r)p^2} \le q_j \le r \quad \text{or}$$

$$r \le q_j \le \frac{rp^2}{rp^2 + (1-r)(1-p)^2}.$$

Rewritten in this way, the conditions represent restrictions on the juror q values for given values of r and p. Figure 4 illustrates, for different values of r, the set of (p, q) values sufficient for the existence of a sincere revelation equilibrium for all even-membered juries. Note that the graphs in Figure 4 contain both a darker and a lighter shaded area, representing the two different sufficiency conditions when there is an even number of jurors.

As further illustration of these sufficiency conditions for an even-membered jury, Figure 5 provides a three-dimensional representation of the lower bound of the first sufficiency condition above (which has the straightforward upper bound $q_j \leq r$) as well as the upper bound of the second sufficiency condition above (which has the straightforward lower bound $q_i \geq r$).

To explore further the scope of the conditions in proposition 6, consider the particular case of r = 0.5



with p=0.8. Proposition 2 states that, for an odd number of jurors (whether three, eleven, or ninetynine), the sincere revelation strategy profile will be a Nash equilibrium for any voting rule whenever $0.2 \le q_j \le 0.8$ for all jurors $j \in N$. In addition, for an even number of jurors (whether four, twelve, or one hundred), the sincere revelation strategy profile will be a Nash equilibrium for any voting rule whenever all juror utilities satisfy either $0.06 \le q_j \le 0.5$ or $0.5 \le q_j \le 0.94$. This demonstrates that strategic jurors may vote sincerely in equilibrium under fairly general conditions for all juries and all voting rules.

To see the generality of proposition 6, consider the following example.

Example 3. As in example 2, consider the jury selection process for a felony trial in California. Recall that the defense has 10 peremptory challenges to dismiss candidates with the lowest q values, and the prosecution has 10 peremptory challenges to dismiss candidates with the highest q values. From a jury pool,

denoted P, consisting of the first 32 candidates (not dismissed for cause), a jury of 12 can be chosen.

Let r = 0.5 and p = 0.8. In this case, a sincere revelation Nash equilibrium exists whenever $0.50 \le q_j \le 0.94$ for all $j \in N$ (other conditions guarantee its existence as well). Again suppose that the distribution of q values for the candidates is uniform between 0.5 and 1.0. Thus, the probability that any one candidate violates the inequality above is 0.12. This gives us:

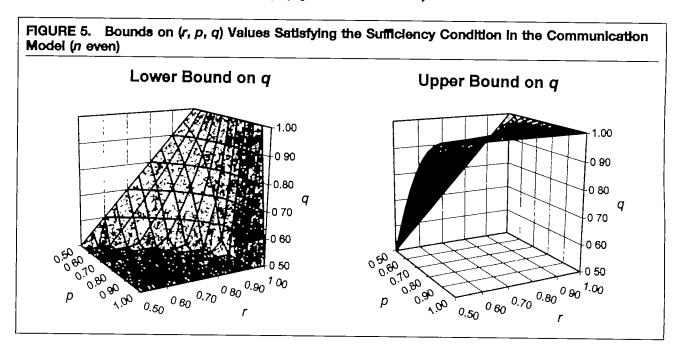
Prob(sincere revelation equilibrium exists)

$$> \text{Prob}(\forall j \in N, 0.50 \le q_j \le 0.94)$$

$$> \text{Prob}(|\{j \in P : q_j > 0.94\}| \le 10)$$

$$> \sum_{k=0}^{10} b(k, 32, 0.12) = 99.9\%.$$

In this example, the conditions of proposition 6 are almost always satisfied.



Proposition 6 suggests that the occurrence of nonstrategic voting among jurors in the communication model may increase as the "accuracy" of trials improves. The same result was observed in the analysis of the mistrial model. As p increases, and trials become more truth revealing, the conditions of proposition 6 become easier to satisfy, and sincere voting Nash equilibria will exist for more juries and more trials.

The next result for the communication model follows directly from proposition 5.

PROPOSITION 7. Suppose the juror utilities satisfy $0.5 \le q_1 \le q_2 \le ... \le q_{n-1} \le q_n$. If condition (a), (b), or (c) from proposition 5 is satisfied under voting rule \hat{k}_1 , then the same condition is satisfied under any other voting rule \hat{k}_2 satisfying $\hat{k}_2 > \hat{k}_1$.

Proof. See Appendix.

This proposition indicates that, as long as $q_j \ge 0.5$ for all j (as we would expect), sincere revelation voting is more likely to be a Nash equilibrium under unanimity rule than under any alternative voting rule.

Comparison of Alternative Voting Rules

The performance of alternative voting rules in the communication model can be evaluated by examining the probability of trial error under different rules.

Proposition 8. Suppose that the sincere revelation strategy profile is a subgame perfect Nash equilibrium for two voting rules, $\hat{\mathbf{k}}_1$ and $\hat{\mathbf{k}}_2$. If jurors behave according to this equilibrium, then

- (1) the probability of convicting an innocent defendant is the same under both voting rules, and
- (2) the probability of acquitting a guilty defendant is the same under both voting rules.

Proof. See Appendix.

Proposition 8 indicates that the sincere revelation Nash equilibrium results in the same probability of trial error under all voting rules. Thus, my conclusions once again contrast with those from the basic model, in which unanimous jury verdicts were shown to be uniquely inferior under this performance measure.

Applying the alternative criterion of expected utility maximization, my results again conflict with the negative assessment of unanimity rule from the analysis of the basic model. Instead, proposition 9 indicates that the sincere revelation Nash equilibrium in the communication model produces the same expected utility under all voting rules.

PROPOSITION 9. Suppose that the sincere revelation strategy profile is a subgame perfect Nash equilibrium for two voting rules, \hat{k}_1 and \hat{k}_2 . If jurors behave according to this Nash equilibrium, then the expected utility for any juror is the same under both voting rules.

Proof. See Appendix.

CONCLUSION

Analysis of the basic model of jury procedure developed by Feddersen and Pesendorfer produces the somewhat surprising result that sincere voting can never be a Nash equilibrium under unanimity rule. Instead, a mixed strategy equilibrium exists in which unanimous jury verdicts are uniquely inferior in terms of minimizing the probability of trial error.

The objective of this article is to evaluate the effect of certain extensions of the basic model on the existence of informative and sincere voting Nash equilibria. In particular, I examined the effects of introducing the possibility of mistrial and allowing limited communication upon the incentives for jurors to vote sincerely. In both cases, I found nontrivial conditions under which informative or sincere voting is indeed a Nash equilibrium. In addition, I compared the outcomes of these Nash equilibria under alternative voting rules and demonstrated that unanimity rule minimizes the probability of trial error and maximizes the ex ante expected utility of jurors.

An additional implication of this article is that the generality of informative and/or sincere voting equilibria is strongly dependent upon the "accuracy" of trials. In particular, as the probability that the true state of the world is revealed at trial increases, the conditions for the existence of the informative or sincere voting Nash equilibria become more general in both the mistrial model and the communication model. This provides an additional argument in support of any legal reform that can be shown to produce more accurate impressions of guilt or innocence at trial.

My focus here was the existence of pure strategy informative and sincere voting Nash equilibria, but the investigation of the effect of mistrial and communication should be extended to mixed strategy and other nonsincere Nash equilibria. In particular, it is important to determine what happens when the conditions for informative or sincere voting Nash equilibria identified here are violated. Do the equilibria that exist in such situations still produce outcomes that make unanimity rule superior in terms of minimizing error and maximizing utility? Or do the results of the basic model prevail, with unanimity being outperformed by other voting rules, such as simple majority?

A research approach for addressing these questions as well as others would be to consider an information structure in which the q values for jurors are drawn from some known distribution function and each juror otherwise knows only her own q value. The conditions for existence of nonstrategic equilibria described in this article encompass many of the parameter value combinations we might reasonably expect to observe, but this alternative approach might produce results that are even more general.

An additional important extension would be to identify the optimal jury institution by comparing alternatives that differ along several dimensions, including the number of jurors, the voting rule employed, and the presence or absence of a mistrial outcome. In order to address this issue, however, one may need to specify a

social welfare function that encompasses not only the utility of each possible trial outcome (whether right or wrong) but also the social cost of multiple trials.

APPENDIX

Proposition 1. Informative voting is a Nash equilibrium in the mistrial model if and only if, for all jurors $j \in N$, we have:

$$\begin{split} \frac{r}{1-r} \cdot \frac{1-p}{p} \leq \frac{(q_j - m_j^l)(1-p)^{2\hat{k}-n-1} + m_j^l p^{2\hat{k}-n-1}}{(1-q_j - m_j^O)(1-p)^{2\hat{k}-n-1} + m_j^O p^{2\hat{k}-n-1}} \\ \leq \frac{r}{1-r} \cdot \frac{p}{1-p} \,. \end{split}$$

Proof. Recall that a strategic voter will condition her strategy on the event that her vote is pivotal. For a given juror i, there are exactly four scenarios in which her vote is pivotal:

- (1) defendant is guilty and k-1 other jurors vote to convict $(G \cap |C|_{-,} = k - 1);$
- (2) defendant is guilty and $\hat{k} 1$ other jurors vote to acquit $(G \cap |A|_{-i} = \hat{k} - 1);$
- (3) defendant is innocent and k-1 other jurors vote to
- convict $(I \cap |C|_{-j} = k 1)$; defendant is innocent and k 1 other jurors vote to acquit $(I \cap |A|_{-j} = k 1)$.

Juror j's beliefs about the relative likelihood of each of these four scenarios will help determine her utility maximizing strategy. In particular, for any j, the expected utility of a vote to convict (ignoring the event in which the vote is not pivotal) is given by:

$$\begin{split} EU_{j}(C, s_{j}) &= \operatorname{Prob}(G \cap |C|_{-j} = \hat{k} - 1) \cdot u_{j}(C, G) \\ &+ \operatorname{Prob}(G \cap |A|_{-j} = \hat{k} - 1) \cdot u_{j}(M, G) \\ &+ \operatorname{Prob}(I \cap |C|_{-j} = \hat{k} - 1) \cdot u_{j}(C, I) \\ &+ \operatorname{Prob}(I \cap |A|_{-j} = \hat{k} - 1) \cdot u_{j}(M, I); \\ EU_{j}(C, s_{j}) &= -q_{j} \cdot \operatorname{Prob}(I \cap |C|_{-j} = \hat{k} - 1) - m_{j}^{G} \\ &\cdot \operatorname{Prob}(G \cap |A|_{-j} = \hat{k} - 1) - m_{j}^{I} \cdot \operatorname{Prob}(I \cap |A|_{-j} = \hat{k} - 1). \end{split}$$

Similarly, the expected utility of a vote to acquit is given by:

$$\begin{aligned} + \operatorname{Prob}(G \cap |\mathcal{A}|_{-j} &= \hat{k} - 1) \cdot u_{j}(A, G) \\ + \operatorname{Prob}(I \cap |\mathcal{C}|_{-j} &= \hat{k} - 1) \cdot u_{j}(M, I) \\ + \operatorname{Prob}(I \cap |\mathcal{A}|_{-j} &= \hat{k} - 1) \cdot u_{j}(A, I); \\ EU_{j}(A, s_{j}) &= -(1 - q_{j}) \cdot \operatorname{Prob}(G \cap |\mathcal{A}|_{-j} &= \hat{k} - 1) - m_{j}^{G} \end{aligned}$$

 $EU_{i}(A, s_{i}) = \operatorname{Prob}(G \cap |C|_{-i} = \hat{k} - 1) \cdot u_{i}(M, G)$

• $Prob(G \cap |C|_{-i} = \hat{k} - 1) - m_i^I \cdot Prob(I \cap |C|_{-i} = \hat{k} - 1).$ Now suppose all jurors vote informatively. That is, $\sigma_i(q_j, g) = 1$ and $\sigma_j(q_j, i) = 0$ for all j, and thus |C| = |g| and |A|

= |i|. I must show that no juror can increase his or her utility by deviating from this strategy. More specifically, for all $j \in$ N, I must show:

(1) If
$$s_j = g$$
, then $EU_j(C, g) \ge EU_j(A, g)$.
(2) If $s_j = \iota$, then $EU_j(A, i) \ge EU_j(C, i)$.

Case 1: $s_i = g$. In this case, j's belief about the probability of the first scenario in which her vote is pivotal $(G \cap |C|_{-})$ (k-1) is given by:

$$\begin{aligned} & \operatorname{Prob}(G \cap |C|_{-j} = \hat{k} - 1 | s_{j} = g) \\ & = \operatorname{Prob}(G \cap |g| = \hat{k} | s_{j} = g) \\ & = \frac{\operatorname{Prob}(G \cap |g| = \hat{k} \cap s_{j} = g)}{\operatorname{Prob}(s_{j} = g)} \\ & = \frac{\operatorname{Prob}(G) \cdot \operatorname{Prob}(|g| = \hat{k} | G) \cdot \operatorname{Prob}(s_{j} = g | |g| = \hat{k})}{\operatorname{Prob}(G) \cdot \operatorname{Prob}(s_{j} = g | G) + \operatorname{Prob}(I) \cdot \operatorname{Prob}(s_{j} = g | I)} \\ & = \frac{r \cdot \frac{n!}{k! \cdot (n - \hat{k})} \cdot p^{\hat{k}} (1 - p)^{n - \hat{k}} \cdot \frac{\hat{k}}{n}}{rp + (1 - r)(1 - p)} \\ & = \frac{(n - 1)!}{(\hat{k} - 1) \cdot (n - \hat{k})} \cdot \frac{r \cdot p^{\hat{k}} (1 - p)^{n - \hat{k}}}{rp + (1 - r)(1 - p)} \\ & = \Psi \cdot r \cdot p^{\hat{k}} (1 - p)^{n - \hat{k}}, \end{aligned}$$

where

$$\Psi = \frac{(n-1)!}{(k-1)\cdot(n-k)\cdot(rp+(1-r)(1-p))}.$$

In the same manner, I can show that:

$$Prob(G \cap |\mathcal{A}|_{-j} = \hat{k} - 1 | s_j = g) = \Psi \cdot r \cdot p^{n - \hat{k} + 1} (1 - p)^{\hat{k} - 1};$$

$$Prob(I \cap |C|_{-j} = \hat{k} - 1 | s_j = g) = \Psi \cdot (1 - r) \cdot p^{n - \hat{k}} (1 - p)^{\hat{k}};$$

$$Prob(I \cap |\mathcal{A}|_{-j} = \hat{k} - 1 | s_j = g)$$

$$= \Psi \cdot (1 - r) \cdot p^{\hat{k} - 1} (1 - p)^{n - \hat{k} + 1}.$$

Thus, the expected utility of a vote to convict is given by:

$$\begin{split} EU_{j}(C,g) &= -q_{j} \operatorname{Prob}(I \cap |C|_{-j} = \hat{k} - 1) \\ &- m_{j}^{G} \operatorname{Prob}(G \cap |A|_{-j} = \hat{k} - 1) \\ &- m_{j}^{I} \operatorname{Prob}(I \cap |A|_{-j} = \hat{k} - 1) \\ &= -q_{j} \Psi (1 - r) p^{n - \hat{k}} (1 - p)^{\hat{k}} - m_{j}^{G} \Psi r p^{n - \hat{k} + 1} (1 - p)^{\hat{k} - 1} \\ &- m_{j}^{I} \Psi (1 - r) p^{\hat{k} - 1} (1 - p)^{n - \hat{k} + 1} \\ &= -\Psi p^{n - \hat{k}} (1 - p)^{n - \hat{k}} [q_{j} (1 - r) (1 - p)^{2\hat{k} - n} \\ &+ m_{j}^{G} r p (1 - p)^{2\hat{k} - n - 1} + m_{j}^{I} (1 - r) p^{2\hat{k} - n - 1} (1 - p)], \end{split}$$

Similarly, the expected utility of a vote to acquit is given by:

$$EU_{j}(A, g) = -(1 - q_{j})\operatorname{Prob}(G \cap |A|_{-j} = \hat{k} - 1)$$

$$- m_{j}^{G} \operatorname{Prob}(G \cap |C|_{-j} = \hat{k} - 1)$$

$$- m_{j}^{I} \operatorname{Prob}(I \cap |C|_{-j} = \hat{k} - 1)$$

$$= -(1 - q_{j})\Psi r p^{n-\hat{k}+1} (1 - p)^{\hat{k}-1} - m_{j}^{G} \Psi r p^{\hat{k}} (1 - p)^{n-\hat{k}}$$

$$- m_{j}^{I} \Psi (1 - r) p^{n-\hat{k}} (1 - p)^{\hat{k}}$$

$$= -\Psi p^{n-\hat{k}} (1 - p)^{n-\hat{k}} [(1 - q_{j}) r p (1 - p)^{2\hat{k}-n-1}$$

$$+ m_{j}^{G} r p^{2\hat{k}-n} + m_{j}^{I} (1 - r) (1 - p)^{2\hat{k}-n}].$$

I now show that the second inequality in proposition 1 holds if and only if $EU_t(C, g) \ge EU_t(A, g)$:

$$\frac{(q_j - m_j^I)(1 - p)^{2\hat{k} - n - 1} + m_j^I p^{2\hat{k} - n - 1}}{(1 - q_j - m_j^O)(1 - p)^{2\hat{k} - n - 1} + m_j^O p^{2\hat{k} - n - 1}} \le \frac{r}{1 - r} \cdot \frac{p}{1 - p}$$

$$\Leftrightarrow (1 - q_{j} - m_{j}^{O})rp(1 - p)^{2\hat{k} - n - 1} + m_{j}^{O}rp^{2\hat{k} - n}$$

$$\geq (q_{j} - m_{j}^{I})(1 - r)(1 - p)^{2\hat{k} - n} + m_{j}^{I}p^{2\hat{k} - n - 1}(1 - p)$$

$$\Leftrightarrow (1 - q_{j})rp(1 - p)^{2\hat{k} - n - 1} + m_{j}^{O}rp^{2\hat{k} - n}$$

$$+ m_{j}^{I}(1 - r)(1 - p)^{2\hat{k} - n} \geq q_{j}(1 - r)(1 - p)^{2\hat{k} - n}$$

$$+ m_{j}^{O}rp(1 - p)^{2\hat{k} - n - 1} + m_{j}^{I}p^{2\hat{k} - n - 1}(1 - p)$$

$$\Leftrightarrow -\Psi p^{n - \hat{k}}(1 - p)^{n - \hat{k}}[q_{j}(1 - r)(1 - p)^{2\hat{k} - n}$$

$$+ m_{j}^{O}rp(1 - p)^{2\hat{k} - n - 1} + m_{j}^{I}p^{2\hat{k} - n - 1}(1 - p)]$$

$$\geq -\Psi p^{n - \hat{k}}(1 - p)^{n - \hat{k}}[(1 - q_{j})rp(1 - p)^{2\hat{k} - n - 1}$$

$$+ m_{j}^{O}rp^{2\hat{k} - n} + m_{j}^{I}(1 - r)(1 - p)^{2\hat{k} - n}]$$

$$\Leftrightarrow EU_{j}(C, g) \geq EU_{j}(A, g).$$

Case 2: $s_j = i$. For this case, I can calculate j's beliefs about the relative probabilities of the four scenarios in which her vote is pivotal in the same manner as above. This gives:

$$\begin{aligned} \operatorname{Prob}(G \cap |C|_{-j} = \hat{k} - 1 | \mathbf{s}_j = i) &= \Phi r p^{\hat{k} - 1} (1 - p)^{n - \hat{k} + 1}; \\ \operatorname{Prob}(G \cap |A|_{-j} = \hat{k} - 1 | \mathbf{s}_j = i) &= \Phi r p^{n - \hat{k}} (1 - p)^{\hat{k}}; \\ \operatorname{Prob}(I \cap |C|_{-j} = \hat{k} - 1 | \mathbf{s}_j = i) &= \Phi (1 - r) p^{n - \hat{k} + 1} (1 - p)^{\hat{k} - 1}; \\ \operatorname{Prob}(I \cap |A|_{-j} = \hat{k} - 1 | \mathbf{s}_j = i) &= \Phi (1 - r) p^{\hat{k}} (1 - p)^{n - \hat{k}}, \end{aligned}$$
where

$$\Phi = \frac{(n-1)!}{(k-1)\cdot(n-k)\cdot(r(1-p)+(1-r)p)}$$

Thus, the expected utility of a vote to convict is given by:

$$\begin{split} EU_{j}(C, i) &= -q_{j} \operatorname{Prob}(I \cap |C|_{\neg j} = \hat{k} - 1) \\ &- m_{j}^{G} \operatorname{Prob}(G \cap |A|_{\neg j} = \hat{k} - 1) \\ &- m_{j}^{I} \operatorname{Prob}(I \cap |A|_{\neg j} = \hat{k} - 1) \\ &= -q_{j} \Phi (1 - r) p^{n - \hat{k} + 1} (1 - p)^{\hat{k} - 1} - m_{j}^{G} \Phi r p^{n - \hat{k}} (1 - p)^{\hat{k}} \\ &- m_{j}^{I} \Phi (1 - r) p^{\hat{k}} (1 - p)^{n - \hat{k}} \\ &= -\Phi p^{n - \hat{k}} (1 - p)^{n - \hat{k}} [q_{j} (1 - r) p (1 - p)^{2\hat{k} - n - 1} \\ &+ m_{j}^{G} r (1 - p)^{2\hat{k} - n} + m_{j}^{I} (1 - r) p^{2\hat{k} - n}]. \end{split}$$

Similarly, the expected utility of a vote to acquit is given by:

$$\begin{split} EU_{j}(A, i) &= -(1 - q_{j}) \operatorname{Prob}(G \cap |A|_{-j} = \hat{k} - 1) \\ &- m_{j}^{G} \operatorname{Prob}(G \cap |C|_{-j} = \hat{k} - 1) \\ &- m_{j}^{I} \operatorname{Prob}(I \cap |C|_{-j} = \hat{k} - 1) \\ &= -(1 - q_{j}) \Phi r p^{n - \hat{k}} (1 - p)^{\hat{k}} - m_{j}^{G} \Phi r p^{\hat{k} - 1} (1 - p)^{n - \hat{k} + 1} \\ &- m_{j}^{I} \Phi (1 - r) p^{n - \hat{k} + 1} (1 - p)^{\hat{k} - 1} \\ &= - \Phi p^{n - \hat{k}} (1 - p)^{n - \hat{k}} [(1 - q_{j}) r (1 - p)^{2\hat{k} - n} \\ &+ m_{j}^{G} r p^{2\hat{k} - n - 1} (1 - p) \\ &+ m_{j}^{I} (1 - r) p (1 - p)^{2\hat{k} - n - 1}]. \end{split}$$

We now show that the first inequality in proposition 1 holds if and only if $EU_r(A, i) \ge EU_r(C, i)$:

$$\begin{split} \frac{r}{1-r} \cdot \frac{1-p}{p} &\leq \frac{(q_{J}-m_{J}^{I})(1-p)^{2\hat{k}-n-1} + m_{J}^{I}p^{2\hat{k}-n-1}}{(1-q_{J}-m_{J}^{O})(1-p)^{2\hat{k}-n-1} + m_{J}^{I}p^{2\hat{k}-n-1}} \\ \Leftrightarrow (q_{J}-m_{J}^{I})(1-r)p(1-p)^{2\hat{k}-n-1} + m_{J}^{I}p^{2\hat{k}-n} \\ &\geq (1-q_{J}-m_{J}^{O})r(1-p)^{2\hat{k}-n} + m_{J}^{O}rp^{2\hat{k}-n-1}(1-p) \\ \Leftrightarrow q_{J}(1-r)p(1-p)^{2\hat{k}-n-1} + m_{J}^{O}r(1-p)^{2\hat{k}-n} \\ &+ m_{J}^{I}(1-r)p^{2\hat{k}-n} \geq (1-q_{J})r(1-p)^{2\hat{k}-n} \\ &+ m_{J}^{O}rp^{2\hat{k}-n-1}(1-p) + m_{J}^{I}(1-r)p(1-p)^{2\hat{k}-n-1} \\ \Leftrightarrow -\Phi p^{n-\hat{k}}(1-p)^{n-\hat{k}}[(1-q_{J})r(1-p)^{2\hat{k}-n} \\ &+ m_{J}^{O}rp^{2\hat{k}-n-1}(1-p) + m_{J}^{I}(1-r)p(1-p)^{2\hat{k}-n-1}] \\ \geq -\Phi p^{n-\hat{k}}(1-p)^{n-\hat{k}}[q_{J}(1-r)p(1-p)^{2\hat{k}-n-1}] \\ \Leftrightarrow EU_{J}(A,i) \geq EU_{J}(C,i). \qquad Q.E.D. \end{split}$$

PROPOSITION 2. Suppose the utility of mistrial is equal to the expected utility of an additional trial before a new jury. Informative voting is then a Nash equilibrium in the mistrial model for any voting rule \hat{k} if and only if, for all $j \in N$, we have:

$$\frac{r}{1-r} \cdot \frac{1-p}{p} \le \frac{q_1}{1-q_1} \le \frac{r}{1-r} \cdot \frac{p}{1-p}.$$

Proof. First note that, in a single trial, we have:

$$\operatorname{Prob}_{s}(C|I) = \operatorname{Prob}_{s}(A|G) = \sum_{x=k}^{n} \binom{n}{x} p^{n-x} (1-p)^{x};$$

$$\operatorname{Prob}_{s}(M|I) = \operatorname{Prob}_{s}(M|G) = \sum_{x=-k+1}^{k-1} \binom{n}{x} p^{n-x} (1-p)^{x}.$$

This gives us:

$$\begin{split} m_j^G &= (1 - q_j) \cdot \operatorname{Prob}_s(A|G) + m_j^G \cdot \operatorname{Prob}_s(M|G); \\ m_j^G &= (1 - q_j) \sum_{x = \hat{k}}^n \binom{n}{x} p^{n-x} (1 - p)^x \\ &+ m_j^G \sum_{x = n - \hat{k} + 1}^{\hat{k} - 1} \binom{n}{x} p^{n-x} (1 - p)^x; \end{split}$$

$$m_{j}^{G} = \frac{(1 - q_{j}) \sum_{x=k}^{n} {n \choose x} p^{n-x} (1 - p)^{x}}{1 - \sum_{x=n-k+1}^{k-1} {n \choose x} p^{n-x} (1 - p)^{x}};$$

$$m_{j}^{G} = (1 - q_{j}) \cdot \Omega,$$

where
$$\Omega = \frac{\sum_{x=k}^{n} \binom{n}{x} p^{n-x} (1-p)^{x}}{1 - \sum_{x=n-k+1}^{k-1} \binom{n}{x} p^{n-x} (1-p)^{x}} \ge 0.$$

Similarly, I can show that: $m_i^I = q_i \cdot \Omega$.

Thus, the condition in proposition 1 becomes:

$$\begin{split} &\frac{r}{1-r} \cdot \frac{1-p}{p} \\ &\leq \frac{(q_j - q_j \Omega)(1-p)^{2\hat{k}-n-1} + q_j \Omega p^{2\hat{k}-n-1}}{(1-q_j - (1-q_j)\Omega)(1-p)^{2\hat{k}-n-1} + (1-q_j)\Omega p^{2\hat{k}-n-1}} \\ &\leq \frac{r}{1-r} \cdot \frac{p}{1-p} \\ &\frac{r}{1-r} \cdot \frac{1-p}{p} \\ &\leq \frac{q_j((1-\Omega)(1-p)^{2\hat{k}-n-1} + \Omega p^{2\hat{k}-n-1})}{(1-q_j)((1-\Omega)(1-p)^{2\hat{k}-n-1} + \Omega p^{2\hat{k}-n-1})} \\ &\leq \frac{r}{1-r} \cdot \frac{p}{1-p} \\ &\frac{r}{1-r} \cdot \frac{1-p}{p} \leq \frac{q_j}{1-q_j} \leq \frac{r}{1-r} \cdot \frac{p}{1-p}. \quad \textit{Q.E.D.} \end{split}$$

PROPOSITION 3. Suppose that mustrial always results in a new trial and consider two voting rules, $\hat{\mathbf{k}}_1$ and $\hat{\mathbf{k}}_2$, with $\hat{\mathbf{k}}_1 < \hat{\mathbf{k}}_2$. If jurors vote informatively, then

- the probability of convicting an innocent defendant is lower under voting rule k

 ₂ than under voting rule k

 ₁; and
- the probability of acquitting a guilty defendant is lower under voting rule k₂ than under voting rule k₁.

Proof. First note that, due to the symmetry of the mistrial model, the probability of convicting an innocent defendant is equal to the probability of acquitting a guilty defendant. Therefore, it is sufficient to prove only part (1) of the proposition.

In addition, note that it is sufficient to prove only that the probability of convicting an innocent defendant is lower under voting rule $k_1 + 1$ than under voting rule k_2 . It is then obvious by induction that, for any voting rule k_2 with $k_1 < k_2$, the probability of convicting an innocent defendant is lower under k_2 than k_1 .

The probability of convicting an innocent defendant in (possibly) repeated trials under voting rule k_1 is given by:

$$\operatorname{Prob}_{R}^{\hat{k}_{1}}(C|I) = \sum_{x=\hat{k}_{1}}^{n} \binom{n}{x} p^{n-x} (1-p)^{x} \\ + \operatorname{Prob}_{R}^{\hat{k}_{1}}(C|I) \cdot \sum_{x=n-\hat{k}_{1}+1}^{\hat{k}_{1}-1} \binom{n}{x} p^{n-x} (1-p)^{x};$$

$$\operatorname{Prob}_{R}^{\hat{k}_{1}}(C|I) = \frac{\sum_{x=\hat{k}_{1}}^{n} \binom{n}{x} p^{x-x} (1-p)^{x}}{1 - \sum_{x=n-\hat{k}_{1}+1}^{n} \binom{n}{x} p^{x-x} (1-p)^{x}};$$

$$\operatorname{Prob}_{R}^{\hat{k}_{1}}(C|I) = \frac{\sum_{x=\hat{k}_{1}}^{n} \binom{n}{x} p^{x-x} (1-p)^{x}}{\sum_{x=\hat{k}_{1}}^{n} \binom{n}{x} p^{x-x} (1-p)^{x}};$$

$$\operatorname{Prob}_{R}^{\hat{k}_{1}}(C|I) = \frac{\sum_{x=\hat{k}_{1}}^{n} \binom{n}{x} p^{x-x} (1-p)^{x}}{\sum_{x=\hat{k}_{1}}^{n} \binom{n}{x} p^{x-x} (1-p)^{x}};$$

$$\operatorname{Prob}_{R}^{k_{1}}(C|I) = \frac{\sum_{x=k_{1}}^{n} \binom{n}{x} p^{n-x} (1-p)^{x}}{\sum_{x=k_{1}}^{n} \binom{n}{x} (p^{n-x} (1-p)^{x} + p^{x} (1-p)^{n-x})}$$

Similarly, the probability of convicting an innocent defendant in possibly repeated trials under voting rule $k_1 + 1$ is given by:

$$\operatorname{Prob}_{R}^{\hat{k}_{1}+1}(C|I) = \frac{\sum_{x=\hat{k}_{1}+1}^{n} \binom{n}{x} p^{n-x} (1-p)^{x}}{\sum_{x=\hat{k}_{1}+1}^{n} \binom{n}{x} (p^{n-x} (1-p)^{x} + p^{x} (1-p)^{n-x})}.$$

I now show that $\operatorname{Prob}_{R}^{\mathbf{\ell}_{1}}(C|I) > \operatorname{Prob}_{R}^{\mathbf{\ell}_{1}}+1(C|I)$:

$$p^{\hat{k}_1-x}(1-p)^{x-\hat{k}_1} < p^{x-\hat{k}_1}(1-p)^{\hat{k}_1-x}$$

for any
$$x > k_1$$
 (recall $p < 1 - p$);

$$\binom{n}{x} p^{n+\hat{k}_1-x} (1-p)^{n+x-\hat{k}_1} < \binom{n}{x} p^{n+x-\hat{k}_1} (1-p)^{n+\hat{k}_1-x}$$

for any $x > k_1$;

$$\sum_{x=\hat{k}_1+1}^n \binom{n}{x} p^{n+\hat{k}_1-x} (1-p)^{n+x-\hat{k}_1} < \sum_{x=\hat{k}_1+1}^n \binom{n}{x} p^{n+x-\hat{k}_1} (1-p)^{n+\hat{k}_1-x};$$

$$p^{\hat{k}_1}(1-p)^{n-\hat{k}_1} \sum_{r=\hat{k}_1+1}^{n} \binom{n}{x} p^{n-r} (1-p)^r$$

$$< p^{n-\hat{k}_1} (1-p)^{\hat{k}_1} \sum_{x=\hat{k}+1}^{n} {n \choose x} p^x (1-p)^{n-x};$$

$$(p^{n-\hat{k}_1}(1-p)^{\hat{k}_1}+p^{\hat{k}_1}(1-p)^{n-\hat{k}_1})\sum_{x=\hat{k}_1+1}^{n} \binom{n}{x} p^{n-x}(1-p)^x$$

$$< p^{n-\hat{k}_1} (1-p)^{\hat{k}_1} \sum_{x=\hat{k}_1+1}^n {n \choose x} (p^{n-x} (1-p)^x + p^x (1-p)^{n-x});$$

$$\left(p^{n-\hat{k}_1}(1-p)^{\hat{k}_1}+p^{\hat{k}_1}(1-p)^{n-\hat{k}_1}+\sum_{x=\hat{k}_1+1}^n \binom{n}{x}(p^{n-x}(1-p)^x\right)$$

$$+p^{x}(1-p)^{n-x}$$
) $\sum_{x=\hat{k}_{1}+1}^{n} {n \choose x} p^{n-x}(1-p)^{x}$

$$<\left(p^{n-\hat{k}_1}(1-p)^{\hat{k}_1}+\sum_{x=\hat{k}_1+1}^n\binom{n}{x}p^{n-x}(1-p)^x\right).$$

$$\sum_{x=x_{1}+1}^{n} {n \choose x} (p^{n-x}(1-p)^{x} + p^{x}(1-p)^{n-x});$$

$$\sum_{x=1}^{n} {n \choose x} (p^{n-x}(1-p)^{x} + p^{x}(1-p)^{n-x}) \cdot \sum_{x=1}^{n} {n \choose x} p^{n-x}(1-p)^{x}$$

$$< \sum_{x=\hat{k}_{1}}^{n} {n \choose x} p^{n-x} (1-p)^{x} \cdot \sum_{x=\hat{k}_{1}+1}^{n} {n \choose x} (p^{n-x} (1-p)^{x} + p^{x} (1-p)^{n-x});$$

$$\frac{\sum_{x=\hat{k}_{1}+1}^{n} {n \choose x} p^{n-x} (1-p)^{x}}{\sum_{x=\hat{k}_{1}+1}^{n} {n \choose x} (p^{n-x} (1-p)^{x} + p^{x} (1-p)^{n-x})}$$

$$< \frac{\sum_{x=\hat{k}_{1}+1}^{n} {n \choose x} p^{n-x} (1-p)^{x}}{\sum_{x=\hat{k}_{1}}^{n} {n \choose x} (p^{n-x} (1-p)^{x} + p^{x} (1-p)^{n-x})};$$

$$Prob_{p}^{\hat{k}_{1}+1}(C|I) < Prob_{p}^{\hat{k}_{1}}(C|I).$$

By induction, we have $\operatorname{Prob}_R^{k_1}(C|I) > \operatorname{Prob}_R^{k_2}(C|I)$ where $k_1 < k_2$, and since $\operatorname{Prob}_R(A|G) = \operatorname{Prob}_R(C|I)$, we also have that $\operatorname{Prob}_R^{k_1}(A|G) > \operatorname{Prob}_R^{k_2}(A|G)$. Q.E.D.

Proposition 4. Suppose that the utility of mustrial is equal to the expected utility of an additional trial before a new jury and consider two voting rules, $\hat{\mathbf{k}}_1$ and $\hat{\mathbf{k}}_2$, with $\hat{\mathbf{k}}_1 < \hat{\mathbf{k}}_2$. If jurors vote informatively, then the ex ante expected utility for an juror is higher under voting rule $\hat{\mathbf{k}}_2$ than under voting rule $\hat{\mathbf{k}}_1$.

Proof. Note that it is sufficient to prove only that the ex ante expected utility is higher under voting rule $k_1 + 1$ than under voting rule k_1 . It is then obvious by induction that, for any voting rule k_2 with $k_1 < k_2$, the expected utility is higher under k_2 than under k_1 .

If all jurors vote sincerely, then the ex ante expected utility for j under voting rule k_1 when the defendant is guilty is given by:

$$EU_{j}(\hat{k}_{1}|G) = -(1 - q_{j})\operatorname{Prob}_{s}^{\hat{k}_{1}}(A|G) + EU_{j}(\hat{k}_{1}|G)\operatorname{Prob}_{s}^{\hat{k}_{1}}(M|G);$$

$$EU_{j}(\hat{k}_{1}|G) = \frac{-(1 - q_{j})\operatorname{Prob}_{s}^{\hat{k}_{1}}(A|G)}{1 - \operatorname{Prob}_{s}^{\hat{k}_{1}}(M|G)};$$

$$EU_{j}(\hat{k}_{1}|G) = \frac{-(1 - q_{j})\sum_{x=\hat{k}_{1}}^{n} \binom{n}{x}p^{n-x}(1 - p)^{x}}{1 - \sum_{\hat{s}=1}^{n} \binom{n}{x}p^{n-x}(1 - p)^{x}}.$$

Similarly, I can show that the ex ante expected utility for j under voting rule k_1 when the defendant is innocent is given by:

$$EU_{j}(\hat{k}_{1}|I) = \frac{-q_{j}\sum_{x=\hat{k}_{1}}^{n} \binom{n}{x}p^{n-x}(1-p)^{x}}{1-\sum_{x=-\hat{k}_{1}+1}^{\hat{k}_{1}-1} \binom{n}{x}p^{n-x}(1-p)^{x}}.$$

Thus, the overall ex ante expected utility for j under voting rule k_1 is given by:

$$EU_{j}(\hat{k}_{1}) = \operatorname{Prob}(G) \cdot \frac{-(1-q_{j}) \sum_{x=\hat{k}_{1}}^{n} \binom{n}{x} p^{n-x} (1-p)^{x}}{1 - \sum_{x=n-\hat{k}_{1}+1}^{n} \binom{n}{x} p^{n-x} (1-p)^{x}}$$

$$+ \operatorname{Prob}(I) \cdot \frac{-q_{j} \sum_{x=\hat{k}_{1}}^{n} \binom{n}{x} p^{n-x} (1-p)^{x}}{1 - \sum_{x=n-\hat{k}_{1}+1}^{\hat{k}_{1}-1} \binom{n}{x} p^{n-x} (1-p)^{x}}$$

$$= -(r(1-q_{j}) + (1-r)q_{j})$$

$$\cdot \frac{\sum_{x=\hat{k}_{1}}^{n} \binom{n}{x} p^{n-x} (1-p)^{x}}{1 - \sum_{x=n-\hat{k}_{1}+1}^{\hat{k}_{1}-1} \binom{n}{x} p^{n-x} (1-p)^{x}}.$$

$$1 - \sum_{x=n-\hat{k}_{1}+1}^{n} \binom{n}{x} p^{n-x} (1-p)^{x}$$

Similarly, the overall ex ante expected utility for j under voting rule $k_1 + 1$ is given by:

$$EU_{i}(\mathbf{k}_{1}+1)=(-r(1-q_{i})-(1-r)q_{i})$$

$$\cdot \frac{\sum_{x=\hat{k}_1+1}^{n} {n \choose x} p^{n-x} (1-p)^x}{1-\sum_{x=n-\hat{k}_1}^{\hat{k}_1} {n \choose x} p^{n-x} (1-p)^x}.$$

I now show that $EU_{i}(\hat{k}_{1}+1) > EU_{i}(\hat{k}_{1})$:

$$\sum_{x=\hat{k}_1+1}^{n} {n \choose x} p^{n+\hat{k}_1-x} (1-p)^{n+x-\hat{k}_1} < \sum_{x=\hat{k}_1+1}^{n} {n \choose x} p^{n+x-\hat{k}_1} (1-p)^{n+\hat{k}_1-x}$$

(from proof of proposition 3);

$$\sum_{x=\hat{k}_{1}+1}^{n} \binom{n}{x} (p^{2n-\hat{k}_{1}-x}(1-p)^{\hat{k}_{1}+x} + p^{n+\hat{k}_{1}-x}(1-p)^{n+x-\hat{k}_{1}})$$

$$< \sum_{x=\hat{k}_{1}+1}^{n} \binom{n}{x} (p^{2n-\hat{k}_{1}-x}(1-p)^{\hat{k}_{1}+x} + p^{n+x-\hat{k}_{1}}(1-p)^{n+\hat{k}_{1}-x});$$

$$\binom{n}{\hat{k}_{1}} (p^{n-\hat{k}_{1}}(1-p)^{\hat{k}_{1}} + p^{\hat{k}_{1}}(1-p)^{n-\hat{k}_{1}}) \sum_{x=\hat{k}_{1}+1}^{n} \binom{n}{x} p^{n-x}(1-p)^{x}$$

$$< \binom{n}{\hat{k}_{1}} p^{n-\hat{k}_{1}}(1-p)^{\hat{k}_{1}} \sum_{x=\hat{k}_{1}+1}^{n} \binom{n}{x} (p^{n-x}(1-p)^{x} + p^{x}(1-p)^{n-x});$$

$$\binom{n}{\hat{k}_{1}} \binom{n}{x} (p^{n-x}(1-p)^{x} + p^{x}(1-p)^{n-x})$$

$$\cdot \binom{n}{x=\hat{k}_{1}+1} \binom{n}{x} p^{n-x}(1-p)^{x}$$

$$\cdot \binom{n}{x=\hat{k}_{1}+1} \binom{n}{x} p^{n-x}(1-p)^{x}$$

$$< \left(\sum_{x=\hat{k}_{1}}^{n} \binom{n}{x} p^{n-x} (1-p)^{x} \right) \left(\sum_{x=\hat{k}_{1}+1}^{n} \binom{n}{x} (p^{n-x} (1-p)^{x} + p^{x} (1-p)^{n-x}) \right);$$

$$\frac{\sum_{x=\hat{k}_{1}+1}^{n} \binom{n}{x} p^{n-x} (1-p)^{x}}{\sum_{x=\hat{k}_{1}+1}^{n} \binom{n}{x} (p^{n-x} (1-p)^{x} + p^{x} (1-p)^{n-x})}$$

$$< \frac{\sum_{x=\hat{k}_{1}+1}^{n} \binom{n}{x} (p^{n-x} (1-p)^{x} + p^{x} (1-p)^{x}}{\sum_{x=\hat{k}_{1}}^{n} \binom{n}{x} (p^{n-x} (1-p)^{x} + p^{x} (1-p)^{n-x})};$$

$$-(r(1-q_j) + (1-r)q_j) \cdot \frac{\sum_{x=\hat{k}_1+1}^{n} \binom{n}{x} p^{n-x} (1-p)^x}{1 - \sum_{x=n-\hat{k}_1}^{\hat{k}_1} \binom{n}{x} p^{n-x} (1-p)^x}$$

$$> -(r(1-q_j) + (1-r)q_j) \cdot \frac{\sum_{x=\hat{k}_1}^{n} {n \choose x} p^{n-x} (1-p)^x}{1 - \sum_{x=n-\hat{k}_1+1}^{\hat{k}_1-1} {n \choose x} p^{n-x} (1-p)^x};$$

$$EU_{l}(\mathbf{k}_{1}+1)>EU_{l}(\mathbf{k}_{1}).$$

By induction, we have $EU_j(k_2) > EU_j(k_1)$ where $k_1 < k_2$. Q.E.D.

PROPOSITION 5. Let the jurors be numbered such that $q_1 \le q_2 \le \ldots \le q_{n-1} \le q_n$. Then the suncere revelation strategy profile is a subgame perfect Nash equilibrium for a given voting rule \hat{k} if and only if one of the following conditions is true:

- (a) $0 \le q_k \le \beta(0, n)$;
- (b) $\beta(n, n) < q_k \le 1$; or
- (c) $\exists k^* \in \{1, ..., n\}$, such that $\beta(k^* 1, n) \le q_j \le \beta(k^*, n)$ for all $j \in N$.

Proof. First recognize that a strategic voter will condition her strategies in both the preliminary and final votes on the event that her vote is pivotal; that is, her vote can change the trial outcome. In the event that her vote is *not* pivotal, her utility is unaffected by her vote, and therefore such situations have no implications for strategic behavior.

I will evaluate strategy in the final vote first and then work backward to examine the preliminary vote.

Final Vote Strategy: Assume that in the preliminary vote, $\sigma_j(g) = 1$ and $\sigma_j(t) = 0$ for all jurors $j \in N$. Further assume that all jurors $j \in N$ vote to convict in the final vote if and only if $\beta(k, n) \ge q_j$, where k is the number of votes to convict from the preliminary vote. I must show that no juror has an incentive to deviate from this strategy in the final vote.

Since all jurors vote sincerely in the preliminary vote, all will know the total number of guilty (g) and innocent (t) signals before taking the final vote. Thus, all jurors will have the same estimate of the probability that the defendant is guilty, namely, $\beta(k, n)$.

For any given j, we need only consider the situation in which his vote is pivotal. That is, if he votes to convict, the defendant will be convicted, and if he votes to acquit, the defendant will be acquitted. Thus, for any j, the expected utility of voting to acquit in this case is given by:

$$EU(A||g|=k) = -(1-q_j) \cdot \text{Prob}(G||g|=k)$$
$$= -(1-q_j) \cdot \beta(k, n).$$

Similarly, the expected utility of voting to convict is given by:

$$EU(C||g| = k) = -q_j \cdot \text{Prob}(I||g| = k)$$
$$= -q_j \cdot (1 - \beta(k, n)).$$

Therefore, j will want to vote to convict if and only if:

$$EU(C||g| = k) \ge EU(A||g| = k)$$

$$-q_j \cdot \operatorname{Prob}(I||g| = k) \ge -(1 - q_j) \cdot \operatorname{Prob}(G||g| = k)$$

$$-q_j \cdot (1 - \beta(k, n)) \ge -(1 - q_j) \cdot \beta(k, n)$$

$$q_j \cdot (1 - \beta(k, n)) \le (1 - q_j) \cdot \beta(k, n)$$

$$q_j - q_j \beta(k, n) \le \beta(k, n) - q_j \beta(k, n)$$

$$q_j \le \beta(k, n).$$

Therefore, sincere voting in the final vote is a Nash equilibrium for this subgame. This result is dependent only upon the assumption of informative voting in the preliminary vote and is independent of satisfaction or violation of conditions (a), (b), and (c).

Preliminary Vote Strategy: Now assume that all jurors $j \in N$ vote to convict in the final vote if and only if $\beta(k, n) \ge q_j$, where k is the number of votes to convict from the preliminary vote. Further assume that $\sigma_j(g) = 1$ and $\sigma_j(t) = 0$ for all jurors $j \in N$ in the preliminary vote. I must show that, if one of the conditions, (a), (b), or (c), is satisfied, then no juror has an incentive to deviate from this informative voting strategy in the preliminary vote. I also must show that, if all three conditions are violated, then at least one juror has an incentive to deviate in the preliminary vote.

Case 1: Condition (a) is satisfied. In this case, $0 \le q_1 \le \ldots \le q_k \le \beta(0, n)$. This means that, in the final vote, at least k jurors will always vote to convict, and the defendant will always be convicted regardless of the outcome of the preliminary vote. Therefore, no juror has a positive incentive to deviate from informative voting in the preliminary vote.

Case 2: Condition (b) is satisfied. In this case, $\beta(n, n) \leq q_k \leq \ldots \leq q_n \leq 1$ for all $j \in N$. This means that, in the final vote, at least n - k + 1 jurors will always vote to acquit, and the defendant will always be acquitted regardless of the outcome of the preliminary vote. Therefore, no juror has a positive incentive to deviate from informative voting in the preliminary vote.

Case 3: Condition (c) is satisfied. In this case, $\exists k^* \in \{1, \ldots, n\}$, such that $\beta(k^*-1, n) \leq q_j \leq \beta(k^*, n)$ for all $j \in N$. Thus, if juror j is pivotal in the preliminary vote, this means that $|g|_{-j} = k^* - 1$. In other words, if j votes C in the preliminary vote, all other jurors will vote C in the final vote;

if j votes A in the preliminary vote, all other jurors will vote A in the final vote.

This means that if j is pivotal in the preliminary vote, then i can completely dictate the final trial outcome through her preliminary vote. Even under unanimity rule, j's preliminary vote will determine the final vote of all other jurors, thus allowing j to choose the trial outcome with her final vote. Thus, we can say that a juror will prefer to vote C in the preliminary vote if and only if she prefers that the defendant be convicted in the final outcome, that is, $EU(C||g|_{-}) = k^*$ -1) $\geq EU(A||g|_{-i} = k^* - 1)$. Now suppose that $s_i = i$. In this case:

$$EU(C||g|_{-j} = k^* - 1) = EU(C||g| = k^* - 1)$$

$$= -q_j \cdot (1 - \beta(k^* - 1, n));$$

$$EU(A||g|_{-j} = k^* - 1) = EU(A||g| = k^* - 1)$$

$$= -(1 - q_j) \cdot \beta(k^* - 1, n);$$

$$\beta(k^* - 1, n) < q_j \Rightarrow \beta(k^* - 1, n) - q_j \cdot \beta(k^* - 1, n)$$

$$< q_j - q_j \cdot \beta(k^* - 1, n)$$

$$\Rightarrow -(1 - q_j) \cdot \beta(k^* - 1, n) > -q_j \cdot (1 - \beta(k^* - 1, n))$$

$$\Rightarrow EU(A||g|_{-j} = k^* - 1) > EU(C||g|_{-j} = k^* - 1).$$

Now suppose that $s_i = g$. In this case:

$$EU(C||g|_{-j} = k^* - 1) = EU(C||g| = k^*)$$

$$= -q_j \cdot (1 - \beta(k^*, n));$$

$$EU(A||g|_{-j} = k^* - 1) = EU(A||g| = k^*)$$

$$= -(1 - q_j) \cdot \beta(k^*, n);$$

$$\beta(k^*, n) \ge q_j \Rightarrow \beta(k^*, n) - q_j \cdot \beta(k^*, n)$$

$$\ge q_j - q_j \cdot \beta(k^*, n)$$

$$\Rightarrow (1 - q_j) \cdot \beta(k^*, n) \ge q_j \cdot (1 - \beta(k^*, n))$$

$$\Rightarrow -(1 - q_j) \cdot \beta(k^*, n) \le -q_j \cdot (1 - \beta(k^*, n))$$

$$\Rightarrow EU(A||g|_{-j} = k^* - 1) \le EU(C||g|_{-j} = k^* - 1).$$

Thus, j will prefer to vote to convict in the preliminary vote if and only if $s_1 = g$.

Case 4: Conditions (a), (b), and (c) are all violated. Violation of conditions (a) and (b) means that $\exists k^* \in \{1, ..., n\}$, such that $\beta(k^*-1, n) < q_k \le \beta(k^*, n)$. For a given j to be pivotal in the preliminary vote, it means that $|g|_{-j} = k^* - 1$. Violation of condition (c) means that $q_1 < \beta(k^*-1, n)$

and/or $\beta(k^*, n) < q_n$. Suppose $q_1 < \beta(k^* - 1, n)$, and consider the situation in which juror 1 is pivotal (i.e., $|g|_{-1} = k^* - 1$) and $s_1 = i$. If juror 1 votes A in the preliminary vote (i.e., votes sincerely), then the defendant will be acquitted, since $\beta(k^*-1, n) <$ q_k . If juror 1 instead deviates and votes C, then the defendant will be convicted, since $q_k \leq \beta(k^*, n)$. Since $q_1 < \beta(k^*, n)$ -1, n), juror 1 prefers that the defendant be convicted and

therefore has a positive incentive to deviate and vote C. Now suppose $\beta(k^*, n) < q_n$, and consider the situation in which juror n is pivotal (i.e., $|g|_{-n} = k^* - 1$) and $s_n = g$. If juror n votes C in the preliminary vote (i.e., votes sincerely), then the defendant will be convicted, since $q_k \leq \beta(k^*, n)$. If juror n instead deviates and votes A, then the defendant will be acquitted, since $\beta(k^* - 1, n) < q_k$. Since $\beta(k^*, n) < q_n$, juror n prefers that the defendant be acquitted and therefore has a positive incentive to deviate and vote A.

Thus, if conditions (a), (b), and (c) are all violated, then informative voting is not a Nash equilibrium in the preliminary vote.

Proposition 6. For n odd, the sincere revelation strategy profile is a subgame perfect Nash equilibrium for any voting rule k if

$$\frac{r}{1-r} \cdot \frac{1-p}{p} \le \frac{q_j}{1-q_j} \le \frac{r}{1-r} \cdot \frac{p}{1-p}.$$

For n even, the sincere revelation strategy profile is a subgame perfect Nash equilibrium for any voting rule \hat{k} if $\forall j \in N$:

$$\frac{r}{1-r} \cdot \left(\frac{1-p}{p}\right)^2 \le \frac{q_j}{1-q_j} \le \frac{r}{1-r} \quad or$$

$$\frac{r}{1-r} \le \frac{q_j}{1-q_j} \le \frac{r}{1-r} \cdot \left(\frac{p}{1-p}\right)^2.$$

Proof. Suppose that n is odd. Proposition 5 says that the sincere revelation strategy profile is a subgame perfect Nash equilibrium for any voting rule k if

$$\beta\left(\frac{n-1}{2}, n\right) \le q_j \le \beta\left(\frac{n+1}{2}, n\right), \ \forall j \in \mathbb{N}.$$

This condition is equivalent to:

$$\begin{split} \frac{rp^{(n-1)/2}(1-p)^{(n+1)/2}}{rp^{(n-1)/2}(1-p)^{(n+1)/2}+(1-r)p^{(n+1)/2}(1-p)^{(n-1)/2}} &\leq q_j \\ &\leq \frac{rp^{(n+1)/2}(1-p)^{(n-1)/2}}{rp^{(n+1)/2}(1-p)^{(n-1)/2}+(1-r)p^{(n-1)/2}(1-p)^{(n+1)/2}} \\ &\frac{r(1-p)}{r(1-p)+(1-r)p} \leq q_j \leq \frac{rp}{rp+(1-r)(1-p)} \\ &\frac{r}{1-r} \cdot \frac{1-p}{p} \leq \frac{q_j}{1-q_j} \leq \frac{r}{1-r} \cdot \frac{\dot{p}}{1-p} \,. \end{split}$$

Now suppose that n is even. Proposition 5 says that the sincere revelation strategy profile is a subgame perfect Nash equilibrium for any voting rule k if

$$\beta\left(\frac{n}{2}-1,n\right) \le q_j \le \beta\left(\frac{n}{2},n\right), \ \forall j \in N \quad \text{or}$$

$$\beta\left(\frac{n}{2},n\right) \le q_j \le \beta\left(\frac{n}{2}+1,n\right), \ \forall j \in N.$$

The first of these two conditions is equivalent to:

$$\begin{split} \frac{rp^{(\sqrt{r}/2)-1}(1-p)^{(\sqrt{r}/2)+1}}{rp^{(\sqrt{r}/2)-1}(1-p)^{(\sqrt{r}/2)+1}+(1-r)p^{(\sqrt{r}/2)+1}(1-p)^{(\sqrt{r}/2)-1}} &\leq q_j \\ &\leq \frac{rp^{\sqrt{r}/2}(1-p)^{\sqrt{r}/2}}{rp^{\sqrt{r}/2}(1-p)^{\sqrt{r}/2}+(1-r)p^{\sqrt{r}/2}(1-p)^{\sqrt{r}/2}} \\ &\frac{r(1-p)^2}{r(1-p)^2+(1-r)p^2} &\leq q_j \leq r \\ &\frac{r}{1-r} \cdot \left(\frac{1-p}{p}\right)^2 \leq \frac{q_j}{1-q_j} \leq \frac{r}{1-r}. \end{split}$$

The second of these two conditions is equivalent to:

$$\frac{rp^{n/2}(1-p)^{n/2}}{rp^{n/2}(1-p)^{n/2}+(1-r)p^{n/2}(1-p)^{n/2}} \leq q_j$$

$$\leq \frac{rp^{(w/2)+1}(1-p)^{(w/2)-1}}{rp^{(w/2)+1}(1-p)^{(w/2)-1}+(1-r)p^{(w/2)-1}(1-p)^{(w/2)+1}}$$

$$r \leq q_j \leq \frac{rp^2}{rp^2+(1-r)(1-p)^2}$$

$$\frac{r}{1-r} \leq \frac{q_j}{1-q_j} \leq \frac{r}{1-r} \cdot \left(\frac{p}{1-p}\right)^2. \qquad Q.E.D.$$

PROPOSITION 7. Suppose the juror utilities satisfy $0.5 \le q_1 \le q_2 \le \ldots \le q_{n-1} \le q_n$. If condition (a), (b), or (c) from proposition 5 is satisfied under voting rule k_1 , then the same condition is satisfied under any other voting rule k_2 satisfying $k_2 > k_1$.

Proof. Suppose condition (a) is satisfied for voting rule k_1 . This means that

$$0 \le q_{k_1} \le \beta(0, n) = \frac{(1-p)^n}{p^n + (1-p)^n} < 0.5.$$

Since $q_j \ge 0.5$ for all j, condition (a) cannot be satisfied for k_1 , and therefore the proposition is satisfied vacuously in this case.

Now suppose condition (b) is satisfied for voting rule k_1 . This means that $\beta(n, n) < q_{k_1} \le 1$. Since $k_1 < k_2$, we have that $q_{k_1} < q_{k_2}$, and thus $\beta(n, n) < q_{k_2} \le 1$. Therefore, condition (b) is also satisfied for voting rule k_2 .

Finally, suppose that condition (c) is satisfied for voting rule k_1 . In this case, the condition is completely independent of the voting rule, and condition (c) is also satisfied for voting rule k_2 .

Q.E.D.

PROPOSITION 8. Suppose that the sincere revelation strategy profile is a subgame perfect Nash equilibrium for two voting rules, $\hat{\mathbf{k}}_1$ and $\hat{\mathbf{k}}_2$. If jurors behave according to this equilibrium, then

- (1) the probability of convicting an innocent defendant is the same under both voting rules; and
- the probability of acquitting a guilty defendant is the same under both voting rules.

Proof. Without loss of generality, assume $k_1 \le k_2$. Existence of the sincere voting Nash equilibrium means that one of the three proposition 5 conditions, (a), (b), or (c), is satisfied for each of the voting rules k_1 and k_2 . It is also straightforward to show that both rules must satisfy the same condition (to see this, follow the same approach as used in the proof of proposition 7).

Suppose both rules satisfy condition (a). In this case, $0 \le q_j \le \beta(0, n)$ for $j = 1, 2, ..., k_1, ..., k_2$. Thus, at least k_2 jurors will always vote to convict in the final vote regardless of the outcome of the preliminary vote and regardless of the voting rule. Therefore, all defendants are convicted under both voting rules, and the probability of trial error under both voting rules is simply 0.5 (the prior probability that the defendant is innocent).

Now suppose both rules satisfy condition (b). In this case, $\beta(n,n) \leq q_j \leq 1$ for $j=k_1,\ldots,k_2,\ldots,n$. Thus, no more than k_1 jurors will ever vote to convict in the final vote regardless of the outcome of the preliminary vote and regardless of the voting rule. Therefore, all defendants are acquitted under both voting rules, and the probability of trial error under both voting rules is simply 0.5 (the prior probability that the defendant is guilty).

Finally, suppose both rules satisfy condition (c). In this case, $\exists k^* \in \{1, ..., n\}$, such that $\beta(k^* - 1, n) \le q_i \le$

 $\beta(k^*, n)$ for all $j \in N$. Recall that the number of votes to convict in the preliminary vote will be equal to |g| in equilibrium. Thus, if $|g| \geq k^*$, then all jurors will vote to convict in the final vote; if $|g| < k^*$, then all jurors will vote to acquit in the final vote. Since all final votes are unanimous, a defendant convicted under one voting rule also would be convicted under the other. Therefore, the probability of convicting an innocent defendant must be the same under both voting rules. Similarly, a defendant acquitted under one voting rule also would be acquitted under the other. Therefore, the probability of acquitting a guilty defendant also must be the same under both voting rules.

Q.E.D.

PROPOSITION 9. Suppose that the sincere revelation strategy profile is a subgame perfect Nash equilibrium for two voting rules, $\hat{\mathbf{k}}_1$ and $\hat{\mathbf{k}}_2$. If jurors behave according to this Nash equilibrium, then the expected utility for any juror is the same under both voting rules.

Proof. In the proof of proposition 8, I showed that the trial outcome will always be the same under both voting rules. Therefore, the expected utility (and the final realized utility) also must be the same under both voting rules. *Q.E.D.*

The following two claims relate to the discussion regarding the appropriate value for r in the section on the theoretical framework for the basic model.

CLAIM 1. With informative voting, the ex ante probability of conviction under unanimity rule in the mustrial model is always between 0.5 and the value of 1 for all parameter combinations.

Proof. The ex ante probability of conviction (in possibly repeated trials) in the mistrial model is given by:

$$\begin{aligned} \operatorname{Prob}_{R}(C) &= \operatorname{Prob}(G) \cdot \operatorname{Prob}_{R}(C|G) + \operatorname{Prob}(I) \cdot \operatorname{Prob}_{R}(C|I) \\ &= r \cdot \operatorname{Prob}_{R}(C|G) + (1-r) \cdot \operatorname{Prob}_{R}(C|I) \\ &= r \cdot \operatorname{Prob}_{R}(A|I) + (1-r) \cdot \operatorname{Prob}_{R}(C|I) \\ &= r \cdot (1 - \operatorname{Prob}_{R}(C|I)) + (1-r) \cdot \operatorname{Prob}_{R}(C|I). \end{aligned}$$

With informative voting, the proof of proposition 3 indicates that:

$$\operatorname{Prob}_{R}(C|I) = \frac{\sum_{x=k}^{n} \binom{n}{x} p^{n-x} (1-p)^{x}}{\sum_{x=k}^{n} \binom{n}{x} (p^{n-x} (1-p)^{x} + p^{x} (1-p)^{n-x})};$$
$$\operatorname{Prob}_{R}(C|I) = \frac{(1-p)^{n}}{(1-p)^{n} + p^{n}}.$$

This gives us:

$$\operatorname{Prob}_{R}(C) = r \frac{p^{n}}{(1-p)^{n} + p^{n}} + (1-r) \frac{(1-p)^{n}}{(1-p)^{n} + p^{n}}$$
$$= \frac{rp^{n} + (1-r)(1-p)^{n}}{p^{n} + (1-p)^{n}}.$$

Now, if $r \ge 0.5$, we have:

$$\frac{rp^n+(1-r)(1-p)^n}{p^n+(1-p)^n}\leq \frac{rp^n+r(1-p)^n}{p^n+(1-p)^n}=r\Rightarrow \operatorname{Prob}_R(C)\leq r.$$

We also have:

$$(2r-1)p^n \ge (2r-1)(1-p)^n$$

$$\Rightarrow 2rp^{n} + 2(1-r)(1-p)^{n} \ge p^{n} + (1-p)^{n}$$

$$\Rightarrow \frac{rp^{n} + (1-r)(1-p)^{n}}{p^{n} + (1-p)^{n}} \ge \frac{1}{2}$$

$$\Rightarrow \operatorname{Prob}_{R}(C) \ge \frac{1}{2}.$$

Thus, for $r \ge 0.5$, we have $\frac{1}{2} \le \operatorname{Prob}_R(C) \le r$. For $r \le 0.5$, I can similarly show that $r \le \operatorname{Prob}_R(C) \le \frac{1}{2}$. Q.E.D.

CLAIM 2. Suppose r = 0.5, $q_j \ge 0.5$ for all $j \in N$, and that the sincere revelation strategy profile is a Nash equilibrium in the communication model. Under this equilibrium, the ex ante probability of conviction will then be less than 0.5 for all voting rules and parameter combinations.

Proof. Suppose that $q_j > \beta(n, n) \ \forall j \in \mathbb{N}$. In this case, all jurors will always vote for acquittal in the sincere revelation equilibrium, so the ex ante probability of conviction is zero, and the claim is proven.

Now suppose it is not true that $q_j > \beta(n, n) \ \forall j \in N$. With $q_j \geq 0.5 \ \forall j \in N$, existence of the sincere revelation equilibrium then requires that $\exists k^* \in \{1, \ldots, n\}$, satisfying $\beta(k^* - 1, n) \leq q_j \leq \beta(k^*, n) \ \forall j \in N$. Note that with r = 0.5, $\beta(k^*, n) < 0.5$ for any $k^* < n/2$. Since $q_j \geq 0.5 \ \forall j \in N$, the constraint $\beta(k^* - 1, n) \leq q_j \leq \beta(k^*, n) \ \forall j \in N$ therefore requires $k^* \geq n/2$.

The ex ante probability of conviction under the sincere revelation equilibrium in then given by:

$$Prob(C) = Prob(G) \cdot Prob(C|G) + Prob(I) \cdot Prob(C|I)$$

$$= r \cdot Prob(C|G) + (1 - r) \cdot Prob(C|I)$$

$$= r \cdot Prob(|g| \ge k^*|G) Prob(C|G)$$

$$+ (1 - r) \cdot Prob(|g| \ge k^*|I)$$

$$= r \sum_{x=k^*}^{n} {n \choose x} p^x (1 - p)^{n-x}$$

$$+ (1 - r) \sum_{x=k^*}^{n} {n \choose x} p^{n-x} (1 - p)^x.$$

Since r = 0.5, we have:

$$Prob(C) = \frac{1}{2} \left[\sum_{x=k^*}^{n} \binom{n}{x} p^x (1-p)^{n-x} + \sum_{x=k^*}^{n} \binom{n}{x} p^{n-x} (1-p)^x \right]$$

$$= \frac{1}{2} \left[\sum_{x=k^*}^{n} \binom{n}{x} p^x (1-p)^{n-x} + \sum_{x=0}^{n-k^*} \binom{n}{x} p^x (1-p)^{n-x} \right]$$

$$\leq \frac{1}{2} \sum_{x=0}^{n} \binom{n}{x} p^x (1-p)^{n-x} = \frac{1}{2}. \qquad Q.E.D.$$

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Jury Verdicts and Preference Diversity

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develop a model of decision making in juries when there is uncertainty about jurors' preferences. I provide a characterization of the equilibrium strategy under any voting rule and show that nonunanimous rules are asymptotically efficient. Specifically, large juries make the correct decision with probability close to one. My analysis also demonstrates that under the unanimous rule, large juries almost never convict the defendant. The last result contrasts markedly with the literature and suggests that the unanimity rule can protect the innocent only at the price of acquitting the guilty.

urors typically have to decide the fate of defendants without being perfectly informed whether they are innocent or guilty. The jurors can make one of the following mistakes: acquit the guilty or convict the innocent. If the jurors' voting behavior does not vary across different voting rules, then the unanimity rule minimizes the probability of convicting both innocent and guilty defendants. This argument leads to the common belief that the unanimity rule is the appropriate voting rule if we want to protect the innocent.

Feddersen and Pesendorfer (1998) assume that jurors behave strategically and show that voting behavior is indeed sensitive to the voting rule. They derive the surprising result that, in large juries, the probability of convicting the innocent is larger under the unanimity rule than under nonunanimous rules.

This article demonstrates that when there is uncertainty about jurors' preferences, the unanimity rule can still protect innocent defendants against wrongful convictions. I develop a model in which each juror votes strategically, has some private information about the defendant's innocence or guilt, and is uncertain about the other jurors' preferences.

The literature on collective decision making has a long tradition. At the end of the eighteenth century, Condorcet ([1785] 1976) asserted that, under the majority rule, groups of people make better decisions than single individuals and large electorates adopt the correct decision with very high probability. Implicit in Condorcet's jury theorem and subsequent formalizations and extensions (Berg 1993; Grofman and Feld 1988; Klevorick, Rothschild, and Winship 1984; Ladha 1992; Young 1988) is the assumption that individuals, even if members of a committee, behave as if their vote alone determines the final outcome. In particular, a juror votes to convict (acquit) if the probability that the defendant is guilty, given *only* her private information, is greater (smaller) than her threshold of reasonable

Starting with Austen-Smith and Banks (1996), how-

ever, several articles have demonstrated that it is not optimal for jurors to vote according to only their private information (see, among others, Feddersen and Pesendorfer 1997; McLennan 1998; Myerson 1998; Wit 1998). Instead, a strategic juror should condition her vote on the event that it is pivotal, that is, the event in which her decision determines the final outcome. Evidence from experimental studies (Ladha, Miller, and Oppenheimer 1996; McKelvey and Palfrey 1998) also suggests that voters act strategically. This article follows the game-theoretic approach to collective decision making and assumes strategic voting behavior.

Jurors do not know the state of the world, which in our context regards whether the defendant is innocent or guilty. As do Feddersen and Pesendorfer (1998), I assume that each juror receives one of two possible costless signals. Subsequent work relaxes these assumptions by considering the case in which the signals are drawn from a continuous distribution (Duggan and Martinelli 1999) and the case of costly information gathering (Persico 1999).

Finally, I assume uncertainty about jurors' preferences. All jurors prefer the correct verdict (conviction of the guilty and acquittal of the innocent), but they may have different thresholds of reasonable doubt. In other words, they may have different concerns about acquitting the guilty and convicting the innocent. A juror, of course, knows her own threshold of reasonable doubt but is uncertain as to how the other jurors weigh the two possible errors. In an election context, Feddersen and Pesendorfer (1997) also assume uncertainty about preferences and show that, under any nonunanimous voting rule, large electorates fully aggregate the information available to the voters. In Duggan and Martinelli (1999) and Feddersen and Pesendorfer (1998) all jurors share the same preferences. Coughlan (2000) allows for preferences to vary across jurors, but he assumes that each juror knows exactly the preferences of all the others. My article represents a first attempt to compare unanimous and nonunanimous voting rules in environments with uncertainty about preferences and strategic behavior.

I demonstrate the existence of a symmetric equilibrium under any voting rule. The equilibrium strategies assume a very simple form: A juror votes to convict if her threshold of reasonable doubt is smaller than a specified cutpoint, and she votes to acquit if her threshold of reasonable doubt exceeds the cutoff. I show that nonunanimous rules are asymptotically effi-

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cient, similar to the case in which jurors share the same preferences. Under any nonunanimous rule, large juries make the correct decision with probability close to one.

The asymptotic properties of the unanimity rule change substantially, however, when uncertainty about juror preferences is introduced. In contrast to Feddersen and Pesendorfer (1998), who assume that preferences are the same across jurors, I show that under the unanimity rule, large juries almost never convict the defendant. Hence, the price paid for the protection of the innocent is the acquittal of the guilty. The model suggests that a juror is less likely to vote to convict when she is uncertain about the other jurors' thresholds of reasonable doubt. Intuitively, a voter's assessment that the defendant is guilty, given that all the other jurors are voting to convict, is stronger when preferences do not vary across individuals than when preferences are private information. In the latter case, a juror could be pivotal because fellow jurors may have low thresholds of reasonable doubt and be predisposed to vote to convict regardless of the signals they observe.

The remainder of the article is organized as follows. In the next section I present the model. Then I discuss some preliminary results and turn to the analysis of unanimous and nonunanimous voting rules. I also compare different voting rules in a jury of fixed size. I conclude by suggesting possible extensions of the model. Some technical proofs are relegated to the Appendix.

THE MODEL

A jury composed of n > 1 individuals has to decide whether to acquit (A) or convict (C) a defendant who may be either innocent (I) or guilty (G). I assume that the probability of innocence is equal to the probability of guilt (and therefore equal to 1/2).

Although a juror is uncertain about the state of the world, she observes one of two possible signals, innocent (i) and guilty (g), before the vote takes place. The signal that a juror receives is correlated with the state of the world,

$$\Pr(i|I) = \Pr(g|G) = p,$$

where p belongs in the interval (½, 1) and $\Pr(s|\omega)$ denotes the probability that a juror observes signal s = i, g when the state of the world is $\omega = I$, G. A juror is more likely to observe signal i when the defendant is innocent and signal g when the defendant is guilty. Conditional on the state of the world, the signals are independent across jurors.

A juror does not know the signals observed by the other jurors. It can be argued that jurors have opportunities to reveal their information during delibera-

tions,³ but it cannot simply be assumed that each juror reports her signal truthfully. For example, a juror who is very concerned about acquitting the guilty (because of a low threshold of reasonable doubt) and behaves strategically may have an incentive not to reveal that she observed an innocent signal in order to decrease the probability of acquittal. Coughlan (2000) shows that there exists an equilibrium in which all jurors reveal their information if and only if their preferences are close enough. Since deliberations are not formally modeled here, I assume that the signal each juror receives constitutes private information.

In addition, I assume that jurors have private information about their own preferences. All prefer adopting the correct decision, but they may assign different weights to the two errors a jury can make. Kalven and Zeisel (1966) provide some evidence that jurors have different concerns about the two possible mistaken judgments. Moreover, jurors often do not know much about the other members of the jury, and it is very difficult, if not impossible, for them to find out the exact concerns of one another during the trial. In other words, when a juror votes, she is uncertain not only about the type of the defendant but also about fellow jurors' preferences (Davis 1973).

Formally, each juror has a type $q \in (0, 1)$, which is private information. I assume that Nature selects the jury by choosing n juror types independently, according to a probability distribution F with support on the interval (0, 1), where F(q) denotes the probability that the type belongs to the set (0, q]. We require the distribution function F to be continuous, strictly increasing, and to admit a density f, such that $\lim_{x\to 0} f(x) > 0$ and $\lim_{x\to 1} f(x) > 0$.

Each juror's utility depends on her type q, the state of the world, and the final decision taken by the jury. Specifically,

$$u(A, I, q) = u(C, G, q) = 0,$$

 $u(C, I, q) = -q, \quad u(A, G, q) = q - 1.$

The highest payoff (normalized to zero) is obtained when the right decision is made. If an innocent defendant is convicted, the payoff is -q; if the jury acquits a guilty person, the juror gets a payoff equal to q-1. The parameter q hence reflects the relative weights a juror gives to the two possible errors. A juror with type q close to one is much more concerned about convicting the innocent than acquitting the guilty, whereas a juror with type q close to zero reverses the relative importance of the two possible errors.

The parameter q can also be viewed as a threshold of reasonable doubt. Suppose a juror believes the defendant is guilty with probability π . If the defendant is acquitted the juror's expected payoff is $-\pi(1-q)$; if the defendant is convicted, the juror receives -q(1-q)

¹ This assumption is made only for notational simplicity. The results hold even if the two states are not equally likely.

² The assumption that the probability of receiving signal ι in state I is equal to the probability of observing signal g in state G is made only to simplify the notation.

³ The literature in social psychology and law examines various social processes (such as influence, conformity, coalition alliances) that operate during the phase of deliberations and discusses their effect on the final verdict (see, e.g., Davis 1973, Gelfand and Solomon 1977, Nagel and Neef 1975).

 π). The juror would like the defendant to be convicted if and only if π is greater than q.

The vote is simultaneous, and each juror must vote either to convict or to acquit, but she cannot abstain. The defendant is convicted if at least $r = 1, \ldots, n$ jurors vote to convict and is acquitted otherwise. The parameter r characterizes the voting rule. For example, when r is equal to n we have the unanimity rule; the simple majority rule is defined by r = n/2 + 1 if n is even and r = (n + 1)/2 if n is odd.

A strategy for juror j can be characterized by two measurable functions, $i_j:(0,1)\to[0,1]$ and $g_j:(0,1)\to[0,1]$. The function $i_j(q)$ denotes the probability that the juror votes to convict given a threshold of reasonable doubt q and signal i. Similarly, the probability that the juror votes to convict when her type is q and she observes signal g is denoted by $g_j(q)$.

We say that juror j votes informatively if she votes to acquit when she observes an innocent signal and votes to convict when a guilty signal is realized. In my notation, such behavior is described by the strategy $i_j(q) = 0$ and $g_j(q) = 1$ for all q in the interval (0, 1). Clearly, if all jurors vote informatively, the unanimity rule minimizes (among all voting rules) the likelihood of convicting the innocent, even though it maximizes the probability of acquitting the guilty. Yet, as I show in the next section, when jurors behave strategically, there is no voting rule under which informative voting can be adopted by all members of the jury in equilibrium.

The following class of strategies plays a very important role in my analysis. I say that juror j adopts a cutoff strategy if there exist two numbers, q_i and q_g , in the unit interval such that

$$i_j(q) = \begin{cases} 1 & \text{for } q < q_i \\ 0 & \text{for } q > q_i \end{cases}$$

and

$$g_{J}(q) = \begin{cases} 1 & \text{for } q < q_{g} \\ 0 & \text{for } q > q_{g} \end{cases}$$

A cutoff strategy is characterized by two thresholds, one for each signal the juror can receive. After observing signal s=i,g the juror votes to convict if her type is smaller than the cutoff q_s and votes to acquit if her type is greater than q_s . A juror who uses a cutoff strategy conditions her vote on her signal only if her type is between the two cutpoints. Given a pair of cutoffs (q_i, q_g) , I do not distinguish between cutoff strategies that prescribe a different voting behavior only to the types q_i and q_g , since, under my assumption on the distribution function F, the two strategies differ only on a set of types of measure zero.

I restrict attention to symmetric Bayesian Nash equilibria in which players do not use weakly dominated strategies. Because all players adopt the same behavior, I drop the subscripts from the equilibrium strategies and let i(q) and g(q) denote the probability

that a juror with type q votes to convict when her signal is i and g, respectively.

ANALYSIS

I begin by establishing some general results that hold under any voting rule. I rule out symmetric equilibria in which players use weakly dominated strategies and derive a condition that any strategy which is not weakly dominated must satisfy. Moreover, I show that in equilibrium, jurors adopt cutoff strategies. In the remainder of this section I analyze unanimous and nonunanimous voting rules.

Consider any voting rule that requires at least two votes in order to convict the defendant. The symmetric strategy profile in which each juror votes to acquit, independently of both her threshold of reasonable doubt and her signal (i(q) = g(q) = 0 for all q), constitutes a Bayesian Nash equilibrium. Suppose that all jurors other than juror j vote to acquit. Then juror j's vote is irrelevant, since the defendant will be acquitted in any case. But if her vote is never pivotal, then any decision is optimal for juror j.

Similarly, under a nonunanimous rule a juror is never pivotal if all other members of the jury always vote to convict. Thus, a symmetric strategy profile in which i(q) and g(q) differ from one only on a set of types of measure zero constitutes a Bayesian Nash equilibrium with the probability of zero that a single juror can influence the final verdict.

Yet, all equilibria in which a juror is never pivotal involve weakly dominated strategies. The following lemma states that a juror who does not adopt a weakly dominated strategy must vote to acquit if she is very concerned about convicting the innocent (i.e., if her type is close to one) and must vote to convict if she is very concerned about acquitting the guilty (if her type is close to zero).

Lemma 1. Consider an arbitrary voting rule r = 1, ..., n. If a strategy $(i(\cdot), g(\cdot))$ is not weakly dominated, then

$$i(q) = \begin{cases} 1 & \text{if } q \in \left[0, \frac{1}{1 + \left(\frac{p}{1-p}\right)^n}\right] \\ 0 & \text{if } q \in \left[\frac{1}{1 + \left(\frac{1-p}{p}\right)^{n-2}}, 1\right] \end{cases}$$

$$g(q) = \begin{cases} 1 & \text{if } q \in \left[0, \frac{1}{1 + \left(\frac{p}{1-p}\right)^{n-2}}\right] \\ 0 & \text{if } q \in \left[\frac{1}{1 + \left(\frac{1-p}{p}\right)^n}, 1\right] \end{cases}$$

⁴ In this model a final verdict is always achieved. Coughlan (2000) considers the possibility of a mistrial.

⁵ Other symmetric equilibria in which a juror's vote is never pivotal can be easily constructed by assuming that i(q) and g(q) are different from zero only on a set of types of measure zero. Again, the probability that a juror's decision is relevant to determine the fate of the defendant is zero, and therefore any voting behavior is optimal for a single juror.

Proof. Take an arbitrary strategy profile and consider juror j. Suppose her type is q, and she has received signal s = i, g. Let $\psi(z, \omega)$ denote the probability that exactly z out of her n-1 fellow jurors vote to convict when the state of the world is $\omega = I$, G. The expected utility of a juror who votes to convict is given by

$$\Pr(G|s) \left[1 - \sum_{x=r-1}^{n-1} \psi(z, G) \right] (q-1)$$
$$- \Pr(I|s) \sum_{x=r-1}^{n-1} \psi(z, I) q.$$

The expected utility if she votes to acquit is

$$\Pr(G|s) \left[1 - \sum_{z=r}^{n-1} \psi(z, G) \right] (q-1)$$
$$-\Pr(I|s) \sum_{z=r}^{n-1} \psi(z, I) q.$$

The difference in utility between voting to convict and voting to acquit is

$$\Pr(G|s)\psi(r-1, G)(1-q) - \Pr(I|s)\psi(r-1, I)q.$$

There are two possibilities. Either the juror is never pivotal, that is, $\psi(r-1, G) = \psi(r-1, I) = 0$, or there is a positive probability that the juror is pivotal, that is, both $\psi(r-1, G)$ and $\psi(r-1, I)$ are strictly positive. In the former case any decision is optimal. In the latter, the juror strictly prefers to vote to convict if

$$q < \frac{\Pr(G|s)\psi(r-1, G)}{\Pr(G|s)\psi(r-1, G) + \Pr(I|s)\psi(r-1, I)}$$

$$= \frac{\Pr(s|G)\psi(r-1, G)}{\Pr(s|G)\psi(r-1, G) + \Pr(s|I)\psi(r-1, I)}$$

$$= P(G|s, piv), \qquad (1)$$

where P(G|s, piv) denotes the probability that the defendant is guilty, given that juror j receives signal s and is pivotal. The juror strictly prefers to vote to acquit when the sign of the above inequality is reversed.

Let N_{-j} denote the set of all jurors other than juror j. Given any strategy profile $(g_{\eta}(q), i_{\eta}(q))_{\eta \in N_{-j}}$ such that juror j is pivotal with positive probability, P(G|s, piv) can be rewritten as

$$P(G|s, piv) = \left[1 + \frac{\Pr(s|I)}{\Pr(s|G)} \frac{\sum_{\substack{M \subset N_{\neg r}|M|=r-1}} \rho_{M}}{\sum_{\substack{M \subset N_{\neg r}|M|=r-1}} \tau_{M}}\right]^{-1},$$

where

$$\rho_{M} = \prod_{\eta \in M} \left(p \int i_{\eta}(q) \ dF(q) \right)$$

$$+ (1-p) \int g_{\eta}(q) dF(q) dF(q)$$

$$\prod_{\eta \notin M} \left(p \int (1-i_{\eta}(q)) dF(q) dF(q) \right) dF(q)$$

$$+ (1-p) \int (1-g_{\eta}(q)) dF(q) dF(q) dF(q),$$

and

$$\begin{split} \tau_{\mathit{M}} &= \prod_{\eta \in \mathit{M}} \left((1-p) \int i_{\eta}(q) \; dF(q) \right. \\ &+ p \int g_{\eta}(q) \; dF(q) \right) \\ &\prod_{\eta \notin \mathit{M}} \left((1-p) \int \left(1 - i_{\eta}(q) \right) \; dF(q) \right. \\ &+ p \int \left(1 - g_{\eta}(q) \right) \; dF(q) \right). \end{split}$$

It is easy to show that $((1-p)/p)^{n-1} \le \rho_M/\tau_M \le (p/(1-p))^{n-1}$, for any subset $M \subset N_{-j}$, such that τ_M , and therefore ρ_M , are different from zero. This, in turn, implies

$$\left(\frac{1-p}{p}\right)^{n-1} \leq \frac{\sum\limits_{\boldsymbol{M}\subset N_{-p}|\boldsymbol{M}|=r-1}^{p}\rho_{\boldsymbol{M}}}{\sum\limits_{\boldsymbol{M}\subset N_{-p}|\boldsymbol{M}|=r-1}^{p}\tau_{\boldsymbol{M}}} \leq \left(\frac{p}{1-p}\right)^{n-1},$$

and therefore I obtain

$$0 < \frac{1}{1 + \frac{\Pr(s|I)}{\Pr(s|G)} \left(\frac{p}{1-p}\right)^{n-1}} \le P(G|s, piv)$$

$$\le \frac{1}{1 + \frac{\Pr(s|I)}{\Pr(s|G)} \left(\frac{1-p}{p}\right)^{n-1}} < 1.$$

Consider a juror who observes signal s and whose type belongs to the interval $(0, [1 + (\Pr(s|I)/\Pr(s|G))(p/(1-p))^{n-1}]^{-1})$. Equation 1 implies that whenever her vote matters, voting to convict is strictly better than voting to acquit. Similarly, consider a juror whose type belongs to the interval $([1 + (\Pr(s|I)/\Pr(s|G))((1-p)/p)^{n-1}]^{-1}, 1)$. Again, equation 1 implies that when the juror is pivotal, she strictly prefers voting to acquit. It follows that any strategy $(g(\cdot), i(\cdot))$ which is not weakly dominated must satisfy the conditions given by lemma 1. Q.E.D.

The intuition behind lemma 1 is simple. The expression $[1 + (\Pr(s|I)/\Pr(s|G))((1-p)/p)^{n-1}]^{-1}$ denotes the probability that the defendant is guilty given that

n-1 jurors observe guilty signals and the last juror receives signal s. Consider a juror who observes signal s and whose threshold of reasonable doubt is greater than $[1 + (\Pr(s|I)/\Pr(s|G))((1-p)/p)^{n-1}]^{-1}$ (i.e., the juror is very concerned about convicting an innocent defendant). Clearly, the juror prefers the defendant to be acquitted even if she knows that all her fellow jurors received guilty signals. Therefore, any time there is a positive probability that her vote makes a difference, the juror strictly prefers to vote to acquit. She is indifferent between voting to acquit and voting to convict when the final verdict does not depend on her decision. I conclude that voting to acquit weakly dominates voting to convict.

Analogously, the expression $[1 + (\Pr(s|I)/\Pr(s|G))(p/(1-p))^{n-1}]^{-1}$ represents the probability that the defendant is guilty, given that a juror observes signal s and all the other n-1 jurors receive innocent signals. Voting to acquit is a weakly dominated strategy for a juror who receives signal s and whose threshold of reasonable doubt is smaller than $[1 + (\Pr(s|I)/\Pr(s|G))(p/(1-p))^{n-1}]^{-1}$. The juror is so concerned about acquitting the guilty that she would like the defendant to be convicted even if all the other members of the jury observe innocent signals. Thus, either the juror's vote is decisive and she strictly prefers to vote to convict or her vote cannot change the final verdict and she is indifferent between the two alternatives.

An important consequence of lemma 1 is that if a juror does not adopt a weakly dominated strategy, the ex ante probability that she votes to convict is strictly between zero and one in any state of the world (by ex ante I mean that the probability is computed before knowing the juror's type and the realization of her signal). This in turn implies that, in all equilibria in which players do not use weakly dominated strategies, the probability that a juror's vote is pivotal is strictly positive.

I now describe equilibrium behavior. The following lemma shows that in equilibrium jurors use cutoff strategies.

Lemma 2. Under any voting rule, the symmetric equilibrium strategy has a cutoff structure with thresholds (q_i, q_g) that satisfy $0 < q_i < q_g < 1$.

Proof. Fix a voting rule $r=1,\ldots,n$. Consider a juror whose type is q and who observes signal $s=\iota,g$. Since, by assumption, her opponents do not use weakly dominated strategies, the probability that the juror's vote is pivotal is strictly positive. From the proof of lemma 1 (equation 1) it follows that the juror prefers to vote to acquit (convict) if her type is greater (smaller) than P(G|s,piv). If her type q is exactly equal to P(G|s,piv), then the juror is indifferent between the two alternatives, and she will adopt a cutoff strategy with cutpoint $q_1 = P(G|i,piv)$ and $q_3 = P(G|g,piv)$. The expressions of P(G|i,piv) and P(G|g,piv) are, respectively:

$$P(G|i, piv) = \frac{1}{1 + \left(\frac{p}{1-p}\right) \frac{\psi(r-1, I)}{\psi(r-1, G)}},$$

$$P(G|g, piv) = \frac{1}{1 + \left(\frac{1-p}{p}\right) \frac{\psi(r-1, I)}{\psi(r-1, G)}},$$

where $\psi(r-1, \omega)$ is the probability that r-1 out of the other n-1 jurors will vote to convict in state $\omega = I$, G. Since $\frac{1}{2} , I conclude that <math>0 < P(G|i, piv) < P(G|g, piv) < 1.6$ Q.E.D.

To provide some intuition for lemma 2, I assume that the jury uses the unanimity rule (a similar argument can be made for any other voting rule). In order to decide how to vote, a juror should compare her threshold of reasonable doubt q with the probability that the defendant is guilty, given all the information she has. Even though the signal observed by each juror is private information and the vote is simultaneous, in equilibrium the juror knows something more than her own signal. In fact, the strategies adopted by all players are commonly known in equilibrium. Moreover, the juror knows that her vote matters only if all the other jurors vote to convict, and therefore she should condition her decision on this event. The juror should use her opponents' strategies to compute P(G|s, piv), the probability that the defendant is guilty, given her signal s = i, g and the fact that her vote is pivotal. Then the juror should vote to convict if P(G|s, piv) is greater than her threshold of reasonable doubt and should vote to acquit otherwise.

I conclude this section by showing why the strategy profile that prescribes informative voting by all players cannot constitute an equilibrium under any voting rule. The probability that the defendant is guilty, given that a juror who observes an innocent signal is pivotal (when all the other players vote informatively), is given by

$$P(G|i, piv) = \frac{1}{1 + \left(\frac{1-p}{p}\right)^{r-1} \left(\frac{p}{1-p}\right)^{n-r+1}} > 0.$$

A juror who receives signal i and has a threshold of reasonable doubt smaller than P(G|i,piv) is not willing to vote to acquit. Similarly, after observing signal g, a juror whose threshold of reasonable doubt is larger than

$$P(G|g, piv) = \frac{1}{1 + \left(\frac{1-p}{p}\right)^r \left(\frac{p}{1-p}\right)^{n-r}} < 1$$

prefers to vote to acquit if all her fellow jurors vote informatively.

⁶ For notational simplicity, in lemma 2 I consider only symmetric equilibria. This proof can be used to show, however, that jurors use cutoff strategies in *any* Bayesian Nash equilibrium in weakly undominated strategies.

THE UNANIMITY RULE

In this section I analyze collective decision making under the unanimity rule. I first show the existence of a symmetric Bayesian Nash equilibrium and then investigate its asymptotic properties.

From the previous section we know that a symmetric Bayesian Nash equilibrium is completely characterized by a pair of numbers (q_i, q_g) , the cutoffs associated with signals i and g. A juror who observes signal s = i, g is indifferent between voting to acquit and voting to convict only when her type is equal to the probability that the defendant is guilty, given that her signal is s and her vote is decisive. To find the equilibrium values of the cutoffs, I need to express P(G|s, piv) in terms of q_i and q_g and then equate P(G|s, piv) to q_s , for s = i, g.

Formally, let γ_{ω} denote the ex ante probability that a given juror votes to convict when the state of the world is $\omega = I$, G. Given a cutoff strategy with cutpoints q_i and q_g , the expressions of γ_I and γ_G are given by

$$\gamma_I = pF(q_i) + (1-p)F(q_i), \qquad (2)$$

and

$$\gamma_G = (1 - p)F(q_t) + pF(q_s). \tag{3}$$

The equilibrium cutoffs are therefore the solution to the following system of equations:

$$q_{I} = \frac{1}{1 + \left(\frac{p}{1 - p}\right) \left(\frac{\gamma_{I}}{\gamma_{G}}\right)^{n - 1}},$$
 (4)

$$q_{g} = \frac{1}{1 + \left(\frac{1-p}{p}\right)\left(\frac{\gamma_{I}}{\gamma_{G}}\right)^{n-1}}.$$
 (5)

The existence of a symmetric Bayesian Nash equilibrium is guaranteed by proposition 1. In the proof I use a fixed point argument to show that the system of equations 4 and 5 admits at least a solution (q_i, q_g) that satisfies $0 < q_i < q_g < 1.7$

Proposition 1. Under the unanimity rule, a symmetric Bayesian Nash equilibrium exists for any jury size.

Proof. From equations 4 and 5 the cutpoint q_i can be expressed as a function of q_g as follows:

$$q_{t} = k(q_{g}) := \frac{q_{g}(1-p)^{2}}{q_{g}(1-p)^{2} + (1-q_{g})p^{2}}.$$
 (6)

The function k is differentiable and strictly increasing on [0, 1]. Moreover, $0 < k(q_g) < q_g$ for all q_g in (0, 1) and k(1) = 1.

Consider the function $v_n:(0,1]\to(0,1)$ defined by

$$\nu_{n}(q_{g}) := \frac{1}{1 + \left(\frac{1-p}{p}\right) \left[\frac{(1-p)F(q_{g}) + pF(k(q_{g}))}{pF(q_{g}) + (1-p)F(k(q_{g}))}\right]^{n-1}}.$$
(7)

Clearly, if \bar{q}_g is a fixed point of the function v_n , then $(k(\bar{q}_g), \bar{q}_g)$ is a solution to the system of equations 4 and 5. The function v_n is continuous, $v_n(1) = p$ for every n and

$$\lim_{q_{p} \to 0} v_{n}(q_{g}) = \frac{1}{1 + \left(\frac{1-p}{p}\right) \left[\frac{(1-p) + p\left(\frac{1-p}{p}\right)^{2}}{p + (1-p)\left(\frac{1-p}{p}\right)^{2}}\right]^{n-1}} > 0,$$

where I make use of L'Hospital's rule (the above limit converges to one as the jury size grows large). For any given n, the function v_n has at least one fixed point in the interval (0, 1). Therefore, a symmetric Bayesian Nash equilibrium exists.

Q.E.D.

I now turn to the asymptotic properties of the unanimity rule. I index the probabilities of mistakes by n and let $P_n(C|I)$ denote the probability that an innocent defendant is convicted by a jury of size n. Analogously, $P_n(A|G)$ represents the probability of acquitting a guilty person when there are n jurors.

When all jurors have the same preferences, Feddersen and Pesendorfer (1998) show that $P_n(C|I)$ is bounded away from zero even in large juries and that this probability can be increasing in the number of jurors. The next proposition demonstrates that these results do not extend to an environment in which there is uncertainty about jurors' preferences. When jurors are uncertain about their fellow jurors' thresholds of reasonable doubt, large juries almost never convict the defendant under the unanimity rule. Hence, the price paid for the protection of the innocent is the acquittal of the guilty.

PROPOSITION 2. Under the unanimity rule,

$$\lim_{n\to\infty}P_n(C|I)=0$$

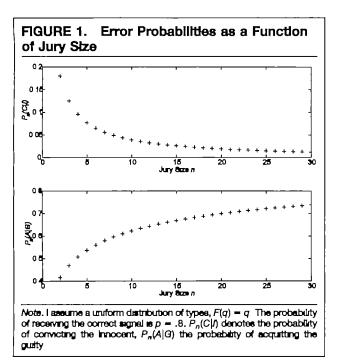
and

$$\lim_{n\to\infty}P_n(A|G)=1.$$

Proof. See Appendix.

An increase in the jury size has two opposite effects on the probability of convicting the defendant. First, given a particular strategy, the probability of conviction decreases as the number of jurors increases. Second, when the size of the jury is large and the unanimity rule is adopted, the fact that a juror is pivotal constitutes strong evidence that the defendant is guilty. This makes a juror more willing to vote to convict. In fact, in the proof of proposition 2 I show that the equilibrium

⁷ Proposition 1 does not guarantee uniqueness of the symmetric Bayesian Nash equilibrium. It is possible to construct both examples with multiple symmetric equilibria and examples that admit a unique symmetric Bayesian Nash equilibrium.



cutpoints q_i and q_g converge to one when the number of jurors goes to infinity. This means that the ex ante probability that a juror votes to convict is close to one, and the fraction of players who condition their vote on their signal converges to zero.

In the proof I demonstrate that the first effect is dominant, and large juries almost never convict the defendant. The asymptotic properties of the unanimity rule in a world with uncertainty about jurors' preferences thus differ substantially from the properties of the unanimity rule when all jurors share the same preferences. A juror is less likely to vote to convict when she is uncertain about her opponents' thresholds of reasonable doubt. Intuitively, the evidence that the defendant is guilty when a juror is pivotal is stronger when preferences do not vary across individuals than when preferences are private information. In the latter case, a juror's vote could be decisive because the other members of the jury may have low thresholds of reasonable doubt and be predisposed to vote to convict regardless of the signals they receive.

The speed of convergence of the error probabilities to the limit values given in proposition 2 depends, of course, on the distribution of juror types and on p, the probability of receiving the correct signal. Here I provide an example to see whether the asymptotic results can represent a good approximation of the error probabilities in small juries. I assume that juror types are drawn from a uniform distribution and let the probability of observing the right signal be p=.8. Figure 1 illustrates the error probabilities as a function of the jury size.

The convergence of $P_n(C|I)$ to zero is fast. In a 12-person jury the probability of convicting an innocent

defendant is about 3% (the probability decreases to 1% when the jury size is n=30). The convergence of $P_n(A|G)$ to one is much slower (this is a general result, see proposition 3 below). When there are 30 jurors, the probability that a guilty defendant will be convicted is still 25% (the probability is 35% in a 12-person jury). This example suggests that in small juries the unanimity rule can protect an innocent defendant from a mistaken conviction. Moreover, protection of the innocent does not necessarily imply acquittal of the guilty.

As shown in Figure 1, the probability of convicting the innocent decreases as the number of jurors increases, whereas the probability of acquitting the guilty increases with jury size. In this example it is possible to make the conviction of the innocent less likely by increasing the number of jurors, even though this reduces the likelihood of convicting the guilty.

The next result concerns the probability that a convicted defendant is innocent, which I denote by $P_n(I|C)$. Even though in large juries a defendant is convicted with small probability, proposition 3 establishes that a convicted defendant is very likely to be guilty. For large juries $P_n(I|C)$ cannot be immediately derived by applying Bayes's rule, since both $P_n(C|I)$ and $P_n(C|G)$ are zero in the limit.

Proposition 3. Under the unanimity rule,

$$\lim_{n\to\infty}P_n(I|C)=0.$$

Proof. See Appendix.

The probability of conviction when the defendant is innocent converges to zero faster than the probability of conviction when the defendant is guilty. As the jury size grows large, the ratio $P_n(C|I)/P_n(C|G)$ goes to zero. Thus, conditional on being convicted by a large jury, the defendant is innocent with probability close to zero.

NONUNANIMOUS VOTING RULES

In this section I consider nonunanimous voting rules. Again, I show the existence of symmetric Bayesian Nash equilibria and then investigate the asymptotic properties. Since the jury size will be allowed to vary, it is convenient to define a nonunanimous rule by the fraction α of votes needed to convict. For notational simplicity I restrict attention to combinations of α and n, such that αn is an integer.

As was shown in lemma 2 the equilibrium strategy has a cutoff structure. Similar to the unanimity rule case, a juror will be indifferent between voting to convict and voting to acquit only when her threshold of reasonable doubt coincides with the probability that the defendant is guilty, given her signal and the fact that her vote is decisive. Thus the equilibrium cutoffs corresponding to the voting rule α are the solutions to the following system of equations:

$$q_{i} = \frac{1}{1 + \left(\frac{p}{1-p}\right) \left(\frac{\gamma_{I}}{\gamma_{G}}\right)^{\alpha n-1} \left(\frac{1-\gamma_{I}}{1-\gamma_{G}}\right)^{n(1-\alpha)}}, \quad (8)$$

$$q_{s} = \frac{1}{1 + \left(\frac{1-p}{p}\right) \left(\frac{\gamma_{I}}{\gamma_{G}}\right)^{\alpha n-1} \left(\frac{1-\gamma_{I}}{1-\gamma_{G}}\right)^{n(1-\alpha)}}, \quad (9)$$

where γ_I and γ_G (defined in equations 2 and 3) represent the ex ante probability that a juror votes to convict in state I and G, respectively. Proposition 4 below is the analog of proposition 1 for nonunanimous voting rules. It shows that under any nonunanimous voting rule it is possible to find an optimal cutoff strategy for a single juror given that the same strategy is adopted by all the other members of the jury.

Proposition 4. Under any nonunanimous rule, a symmetric Bayesian Nash equilibrium exists for any jury size.

Proof. I prove that the system of equations 8 and 9 admits a solution (q_i, q_g) that satisfies $0 < q_i < q_g < 1$. By inspecting equations 8 and 9 we notice that $q_i = k(q_g)$, where the function k is defined in equation 6. Consider the function $d_n: (0, 1) \to (0, 1)$, defined by:

$$d_{\pi}(q_{g}) := \left\{ 1 + \left(\frac{1-p}{p} \right) \left[\frac{(1-p)F(q_{g}) + pF(k(q_{g}))}{pF(q_{g}) + (1-p)F(k(q_{g}))} \right]^{\alpha \pi - 1} \cdot \left[\frac{1-(1-p)F(q_{g}) - pF(k(q_{g}))}{1-pF(q_{g}) - (1-p)F(k(q_{g}))} \right]^{\pi(1-\alpha)} \right\}^{-1}. \quad (10)$$

If \bar{q}_g is a fixed point of the function d_n , then $(k(\bar{q}_g), \bar{q}_g)$ solves the system of equations 8 and 9. The function d_n is continuous on (0, 1). By applying L'Hospital's rule I obtain

 $\lim_{q_{\mathtt{m}} \to 0} d_{\mathtt{m}}(q_{\mathtt{g}})$

$$= \frac{1}{1 + \left(\frac{1-p}{p}\right) \left[\frac{(1-p) + p\left(\frac{1-p}{p}\right)^2}{p + (1-p)\left(\frac{1-p}{p}\right)^2}\right]^{\cos - 1}} > 0$$

(the limit converges to one as n goes to infinity) and

$$\lim_{q_{\mathbf{g}}\to 1}\,d_{\mathbf{g}}(q_{\mathbf{g}})$$

$$= \frac{1}{1 + \left(\frac{1-p}{p}\right) \left[\frac{(1-p) + p\left(\frac{p}{1-p}\right)^{2}}{p + (1-p)\left(\frac{p}{1-p}\right)^{2}}\right]^{n(1-\alpha)}} < 1$$

(the limit converges to zero as n goes to infinity). I conclude that for all n the function g_n has a fixed point in the interval (0, 1).

Q.E.D.

I now analyze the probability of a mistaken judgment when the jury size is large. The following proposition shows that nonunanimous rules are asymptotically efficient. Under any nonunanimous rule, large juries convict the guilty and acquit the innocent with probability close to one.

Proposition 5. Under any nonunanimous rule,

$$\lim_{n\to\infty} P_n(C|I) = \lim_{n\to\infty} P_n(A|G) = 0.$$

Proof. See Appendix.

In the proof I show that for large juries $\gamma_I < \alpha <$ γ_G , that is, the fraction of votes needed to convict is strictly between the ex ante probability that a juror votes to convict when the defendant is innocent and the ex ante probability that a juror votes to convict when the defendant is guilty. In order to provide some intuition for this result, I consider what could happen if instead $\alpha < \gamma_I < \gamma_G$ and the jury size is large. In this case the probability that the defendant is guilty, given that the juror is pivotal, will be very small.8 Since α is closer to γ_I than to γ_G , when a juror is pivotal, that is, when the actual share of votes to convict is almost α , it is much more likely that the state of the world is I. Thus, the juror will not be willing to convict, that is, her optimal cutoffs should be close to zero. But then the ex ante probability that a juror votes to convict cannot be greater than α .

Similarly, if $\gamma_I < \gamma_G < \alpha$ and the jury size is large, then it is very likely that the defendant is guilty when a single juror is pivotal. The juror should vote to convict for almost all values of q. But this implies that both γ_I and γ_G should be close to one and greater than α .

The remaining possibility is that $\gamma_I < \alpha < \gamma_G.^9$ When the defendant is innocent and the jury is very large, the actual share of votes to convict will be close to γ_I (by the law of large numbers), and the defendant will be acquitted with probability close to one. Similarly, if the defendant is guilty, then the actual fraction of votes to convict will be close to γ_G for large values of n, and with probability close to one the defendant will be convicted. ¹⁰

⁸ Consider the expressions of q_i and q_g in equations 8 and 9. When $\alpha < \gamma_I < \gamma_G$, we have $[(\gamma_I)^n (1 - \gamma_I)^{(1-\alpha)}]/[(\gamma_G)^{\alpha} (1 - \gamma_G)^{(1-\alpha)}] > 1$, which implies that for large values of n the cutoffs q_i and q_g are close to zero

⁹ Recall that in any equilibrium q_i is strictly smaller than q_g , and therefore γ_G is strictly greater than γ_I

¹⁰ In my example with a uniform distribution of juror types and p = .8, the convergence to the limit probabilities given in proposition 5 seems to be reasonably fast. For example, under the simple majority rule, both the probability of convicting the mnocent and the probability of acquitting the guilty are close to 2.1% when the jury size is n = 29. Under the supermajority rule $\alpha = 2/3$, a 30-person jury convicts the innocent with probability of 2% and acquits the guilty with probability of 3.6%.

Voting Rule r	1	.08697	.15313	.25687	5 .39618	6 .55357
Equilibrium cutoff for signal / (q,)	.04798					
Equilibrium cutoff for signal g (q_g)	.44643	.60382	.74313	.84687	.91303	.95202
Probability of convicting the innocent	.55936	.32091	.20422	.14630	.10895	.06448
Probability of acquitting the guilty	.06448	.10895	.14630	.20422	.32091	.55936

COMPARISON OF THE VOTING RULES

The previous analysis has shown that, for sufficiently large juries, any nonunanimous rule outperforms the unanimous rule in terms of minimizing the probability of a mistaken verdict. In particular, in the case of infinitely large juries, the unanimous rule is the only voting rule that never leads to a conviction of a guilty defendant. Hence, any nonunanimous rule aggregates the available information efficiently, but this is not the case for the unanimity rule.

Yet, the analytical results pertain to the limiting case of infinitely large juries, so this section is devoted to a comparison of different voting rules in a jury of fixed size. I investigate whether there is a relationship between the probabilities of mistaken judgments and the number of votes needed to convict.

As my examples show, we cannot draw any conclusion of general validity. There are cases in which requiring more votes to convict makes conviction of both the innocent and the guilty less likely, similar to what happens when jurors vote informatively. Yet, it is also possible to construct examples in which the probability of convicting the innocent is greater under the unanimity rule than under majority rules.

I consider a six-person jury and assume that the probability of receiving the right signal is p = .8 and that juror types are drawn from a uniform distribution. Table 1 reports for any voting rule r = 1, ..., 6, the equilibrium cutpoints¹¹ and the probabilities of mistaken judgments.

Equilibrium voting behavior is quite sensitive to the voting rule. The equilibrium cutoff associated with signal i ranges from .04 when only one vote is needed to convict to .55 under the unanimity rule. Similarly, conditional on receiving a guilty signal, the probability that a juror votes to convict is .44 when r = 1 and .95 when r = 6. Such a variation in individual behavior may seem surprising, but bear in mind that the event on which each juror conditions her vote varies with the voting rule. When r = 1, a juror is not willing to convict, since her vote is decisive only when no other juror is voting to convict, an event which strongly supports the conjecture that the defendant is innocent. Under the unanimity rule, being pivotal is strong evidence that the defendant is guilty, since all the other jurors are voting to convict. A juror is more inclined in this case to vote to convict.

This example also illustrates a trade-off between voting rules. As the number of votes required to convict increases, the jury is less likely to convict the innocent, but there is a higher probability of acquitting the guilty.¹²

Requiring more votes to convict, however, does not necessarily mean greater protection of the innocent. Moreover, it is not always the case that juries face a trade-off when choosing among different voting rules. I provide a simple example in which the majority rule performs better than the unanimity rule in terms of minimizing the probabilities of both errors.

I assume that there are three jurors who receive the correct signal with probability p = .8. The distribution function F satisfies

$$F(q_i) = .1$$
, $F(q_i') = .5$, $F(q_g) = .6$, $F(q_g') = .8$, where $q_i \sim .2809$, $q_i' \sim .3039$, $q_g \sim .8621$, and $q_g' \sim .8748$.

It is easy to check that (q_i, q_g) are the cutpoints of a symmetric Bayesian Nash equilibrium for the majority rule (r=2), and that (q_i', q_g') are the cutoffs of a symmetric Bayesian Nash equilibrium for the unanimity rule. Under the majority rule, the probability of convicting the innocent is .104 and the probability of acquitting the guilty is .5. Under the unanimity rule, the innocent are convicted with probability .1756 and the guilty are acquitted with probability .5948. Clearly, both errors are more likely when unanimous agreement is needed to convict. 13

CONCLUSION

This article develops a model of collective decision making in an environment of uncertainty about juror preferences. Nonunanimous rules are asymptotically efficient, whereas the unanimity rule fails to aggregate information efficiently. In large juries that adopt the unanimity rule, the probability of convicting the innocent is close to zero, but the probability of acquitting the guilty is almost one. The last result contrasts markedly with the literature and suggests that the

When juror types are drawn from a uniform distribution, the symmetric Bayesian Nash equilibrium (in weakly undominated strategy) is unique.

¹² Note that in this example the probability of convicting the innocent under voting rule r coincides with the probability of acquitting the guilty under voting rule n+1-r. This is a special property of the uniform distribution and does not extend to generic distribution functions.

¹³ It is possible to construct examples, which I do not report for brevity, in which the probability of any error decreases as the number of votes to convict increases.

unanimity rule can protect the innocent only at the price of acquitting the guilty.

I have compared different voting rules in a jury of fixed size. There are cases in which requiring more votes to convict decreases the probability of conviction, and there are also cases in which the probability of a conviction under the unanimity rule exceeds the likelihood under majority rules. Since real-world juries tend to be small, the properties of voting rules in juries of finite size deserve further investigation.

Moreover, jurors are exposed to the same evidence during the trial, and a natural extension of my model would be to introduce correlation among jurors' signals. Another interesting extension would be to allow jurors to communicate before reaching a verdict. Because deliberations present the jury with an opportunity to communicate private information, they may play a decisive role in shaping the final verdict.

APPENDIX

Proof of Proposition 2

Since the equilibrium need not be unique, I show that the results stated in proposition 2 hold for any sequence of symmetric Bayesian Nash equilibria. I write q_1^n and q_3^n for the cutoffs corresponding to a symmetric Bayesian Nash equilibrium when the jury size is n. Similarly, I use γ_n^n to denote the ex ante probability that a juror votes to convict in state $\omega = I$, G when the number of players is n. I need to establish a preliminary lemma.

LEMMA 3. Let $\{q_i^n, q_g^n\}_{n=2}^\infty$ be a sequence of equilibrium cutoffs under the unanimity rule. Then

$$\lim_{n\to\infty}q_i^n=\lim_{n\to\infty}q_i^n=1.$$

Proof. Consider the function $h:[0,1] \to (0,1]$ defined by

$$h(x) := \begin{cases} \frac{(1-p)F(x) + pF(k(x))}{pF(x) + (1-p)F(k(x))} & \text{for } x \in (0, 1] \\ \frac{(1-p) + p\left(\frac{1-p}{p}\right)^2}{p + (1-p)\left(\frac{1-p}{p}\right)^2} & \text{for } x = 0 \end{cases}$$
(11)

where the function k is defined in equation 6. The function h is continuous on [0, 1], and h(x) < 1 for all x < 1.

We can express the function v_n defined in equation 7 as

$$\nu_{\mathbf{a}}(x) = \frac{1}{1 + \left(\frac{1-p}{p}\right)(h(x))^{\mathbf{a}-1}}.$$
 (12)

Fix $\varepsilon > 0$. Let h_{ε} be the maximum of h on the interval $[0, 1-\varepsilon]$ (the maximum exists since h is continuous). Since $h_{\varepsilon} \in (0, 1)$ there exists $\bar{n} > 0$ such that $1/[1 + ((1-p)/p)(h_{\varepsilon})^{n-1}] > 1 - \varepsilon$ for all $n > \bar{n}$. This, in turn, implies $v_n(x) > 1 - \varepsilon > x$ for all x in the interval $[0, 1-\varepsilon]$ and all $n > \bar{n}$. Therefore, the fixed point of the function v_n is greater than $1 - \varepsilon$ for every $n > \bar{n}$. I conclude $\lim_{n \to \infty} q_{\varepsilon}^n = 1$. Finally, $\lim_{n \to \infty} q_1^n = \lim_{n \to \infty} k(q_{\varepsilon}^n) = 1$, where the last equality holds since k is a continuous function and k(1) = 1.

I am now ready to show that an innocent defendant will almost never be convicted by large juries. From equation 5 we can express $(\gamma_I^n)^{n-1}$ as a function of q_*^n :

$$(\gamma_I^n)^{n-1} = \frac{p(1-q_g^n)(\gamma_O^n)^{n-1}}{(1-p)q_g^n}.$$

As q_g^n converges to one, the numerator $p(1-q_g^n)(\gamma_G^n)^{n-1}$ converges to zero—it is majorized by $p(1-q_g^n)$ —while the denominator $(1-p)q_g^n$ converges to (1-p). Thus, $\lim_{n\to\infty} (\gamma_I^n)^{n-1} = 0$. Since for any $n < \gamma_I^n < 1$, I conclude that $\lim_{n\to\infty} P_n(C|I) = \lim_{n\to\infty} P_n(C|G) = 0$ is more involved. I use a contradiction argument. If $P_n(C|G)$ does not converge to zero, then for some g in the interval (0,n) it is possible to

The proof that $\lim_{n\to\infty} P_n(C|G) = 0$ is more involved. I use a contradiction argument. If $P_n(C|G)$ does not converge to zero, then for some ε in the interval (0, p) it is possible to find a subsequence $\{(\gamma_G^n)^n\}$ such that $(\gamma_G^n)^n \ge \varepsilon$ for all n. The subsequence $\{(\gamma_G^n)^n\}$ is bounded, so it contains a convergent subsequence. Without loss of generality I suppose that this is true of $\{(\gamma_G^n)^n\}$ itself. I let $a \ge \varepsilon$ denote the limit and show that $a \le p$. From equation 2 we have

$$(\gamma_I^n)^n = [pF(q_I^n) + (1-p)F(q_I^n)]^n > (F(q_I^n))^n$$

which implies that $\lim_{n\to\infty} (F(q_i^n))^n = 0$. Also from equation 3 we obtain

$$(\gamma_G^n)^n = [(1-p)F(q_i^n) + pF(q_i^n)]^n < [(1-p)F(q_i^n) + p]^n$$

$$< (1-p)(F(q_i^n))^n + p,$$

where the last inequality follows from the convexity of x^n for x > 0. Thus,

$$a = \lim_{n \to \infty} (\gamma_G^n)^n \leq (1 - p) \lim_{n \to \infty} (F(q_i^n))^n + p = p.$$

Let $\{\delta_n\}$ and $\{\beta_n\}$ be two sequences of numbers belonging to the unit interval and satisfying

$$\gamma_I^n = pF(q_I^n) + (1-p)F(q_I^n) = 1 - \delta_n - \beta_n$$
 (13)

and

$$\gamma_G^n = (1 - p)F(q_I^n) + pF(q_I^n) = 1 - \delta_n \tag{14}$$

for all n. Clearly, $\lim_{n\to\infty} \delta_n = \lim_{n\to\infty} \beta_n = 0$. From equations 13 and 14 we obtain

$$F(q_i^n) = 1 - \delta_n - \frac{p}{2p-1} \beta_n,$$

$$F(q_s^n) = 1 - \delta_n + \frac{1-p}{2p-1} \beta_s$$

Note that $\beta_n < (2p-1)/(1-p) \ \delta_n$, since $F(q_g^n) < 1$. Therefore, for every n,

$$\gamma_I^n > 1 - \delta_n - \frac{2p-1}{1-p} \delta_n = 1 - \frac{p}{1-p} \delta_n$$

which implies $\lim_{n\to\infty} (1-(p/(1-p))\delta_n)^n = 0$. Yet,

$$\lim_{n\to\infty} (\gamma_G^n)^n = \lim_{n\to\infty} \left(1 - \frac{n\delta_n}{n}\right)^n = e^{\ln a}. \tag{15}$$

The sequence of numbers $\{n\delta_n\}$ must be bounded. If not, we could select a subsequence $\{n'\delta_{n'}\}$ such that $n'\delta_{n'} > -2$ ln a for all n'. But this would imply

$$e^{\ln a} = \lim_{n' \to \infty} \left(1 - \frac{n' \delta_{n'}}{n'}\right)^{n'} \le \lim_{n' \to \infty} \left(1 + \frac{2 \ln a}{n'}\right)^{n'} = e^{2 \ln a},$$

which is impossible, since a < 1.

Since the sequence $\{n\delta_n\}$ is bounded, it contains a convergent subsequence, let us say $\{n'\delta_{n'}\}$. It follows from equation 15 that $\lim_{n\to\infty} n'\delta_{n'} = -\ln a$. But this implies

$$\lim_{n'\to\infty}\left(1-\frac{p}{1-p}\frac{n'\delta_{n'}}{n'}\right)^{n'}=e^{(p/(1-p))\ln a}>0,$$

a contradiction. This concludes the proof.

Q.E.D.

Proof of Proposition 3

Since the equilibrium cutpoint associated with signal g is a fixed point of the function v_n , I obtain from equation 12

$$(h(q_{\mathbf{g}}^{\mathbf{n}}))^{n-1} = \left(\frac{p}{1-p}\right) \left(\frac{1}{q_{\mathbf{g}}^{\mathbf{n}}} - 1\right).$$

As q_s^n converges to one, $(h(q_s^n))^{n-1}$ goes to zero. Since h(1) = 1 and h is a continuous function, we have $\lim_{n \to \infty} (h(q_s^n))^n = 0$. I conclude

$$\lim_{n\to\infty} P_n(I|C) = \lim_{n\to\infty} \frac{(h(q_g^n))^n}{1+(h(q_g^n))^n} = 0.$$

Q.E.D.

Proof of Proposition 5

Fix a voting rule $\alpha \in (0, 1)$. I index the equilibrium cutpoints and the ex ante probabilities that a juror votes to convict by the jury size n.

Consider the function $l:[0,1] \to \Re_+$ defined by

$$l(x) := \begin{cases} \left[\frac{(1-p) + p\left(\frac{1-p}{p}\right)^{2}}{p + (1-p)\left(\frac{1-p}{p}\right)^{2}} \right]^{\alpha} & \text{for } x = 0 \\ \frac{[(1-p)F(x) + pF(k(x))]}{pF(x) + (1-p)F(k(x))} \right]^{\alpha} & \text{for } x = 0 \end{cases}$$

$$\cdot \left[\frac{1 - (1-p)F(x) - pF(k(x))}{1 - pF(x) - (1-p)F(k(x))} \right]^{1-\alpha} & \text{for } x \in (0, 1)$$

$$\left[\frac{(1-p) + p\left(\frac{p}{1-p}\right)^{2}}{p + (1-p)\left(\frac{p}{1-p}\right)^{2}} \right]^{1-\alpha} & \text{for } x = 1$$

where the function k is defined in equation 6. I now show that there exists a number \tilde{q} in (0, 1) that satisfies

$$(1-p)F(\bar{q}) + pF(k(\bar{q})) < \alpha < pF(\bar{q}) + (1-p)F(k(\bar{q}))$$

$$(16)$$

and such that

$$l(x) > 1 \quad \text{for } x > \tilde{q}, \tag{17}$$

$$l(x) < 1 \quad \text{for } x < \tilde{q}. \tag{18}$$

The function l is continuous on [0, 1], l(0) < 1 and l(1) > 1. Thus, there exists a value \mathbf{q} in (0, 1) such that $l(\mathbf{q}) = 1$. Note that $x^{\alpha}(1-x)^{1-\alpha}$ is a single-peaked function for $x \in [0, 1]$ with a maximum at $x = \alpha$. Since for any x in (0, 1)

$$(1-p)F(x) + pF(k(x)) < pF(x) + (1-p)F(k(x)),$$
 (19)

it follows that $l(\tilde{q}) = 1$ only if inequality 16 is satisfied. Finally, inequalities 17 and 18 hold since both sides of the inequality 19 are strictly increasing in x.

I now show that any sequence $\{q_g^n\}_{n=2}^{\infty}$ of equilibrium cutoffs associated with signal g converges to \tilde{q} . Note that the function d_n defined by equation 10 can be expressed as

$$d_n(x) = \frac{1}{1 + \left(\frac{1-p}{p}\right) \frac{l(x)^n}{h(x)}},$$

where the function h is defined by equation 11.

Fix $\varepsilon > 0$. Let h_{ε} be the maximum of h on the interval $[\bar{q} + \varepsilon, 1]$. Similarly, let l_{ε} denote the minimum of the function l on the interval $[\bar{q} + \varepsilon, 1]$ (note that both h and l are continuous functions, thus h_{ε} and l_{ε} are well defined). Since $l_{\varepsilon} > 1$ it is possible to find an integer \bar{n} such that $1/[1 + ((1 - p)/p)(l_{\varepsilon}^n/h_{\varepsilon})] < \bar{q} + \varepsilon$ for all $n > \bar{n}$. This implies $d_n(x) < \bar{q} + \varepsilon < x$ for all x in $[\bar{q} + \varepsilon, 1]$ and all $n > \bar{n}$. In a similar way it is possible to prove that given any $\varepsilon > 0$ the fixed point of d_n is larger than $\bar{q} - \varepsilon$ for large values of n (I omit the details). I conclude that $\lim_{n \to \infty} q_n^n = \bar{q}$.

omit the details). I conclude that $\lim_{n\to\infty} q_s^n = q$. For both states of the world the ex ante probability that a juror votes to convict is a continuous function of the cutoff.

$$\tilde{\gamma}_I := \lim_{q \to 0} \gamma_I^q = (1-p)F(\bar{q}) + pF(k(\bar{q})) < \alpha,$$

and

$$\tilde{\gamma}_G := \lim_{n \to \infty} \gamma_G^n = pF(\tilde{q}) + (1-p)F(k(\tilde{q})) > \alpha.$$

By the law of large numbers the share of votes to convict if the defendant is innocent converges in probability to $\tilde{\gamma}_I < \alpha$, so the defendant is acquitted with probability close to one for large values of n. Similarly, if the defendant is guilty the share of votes to convict converges in probability to $\tilde{\gamma}_G > \alpha$, and the defendant is convicted with probability close to one for n sufficiently large.

Q.E.D.

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An Experimental Study of Jury Decision Rules

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e present experimental results on groups facing a decision problem analogous to that faced by a jury. We consider three treatment variables: group size (three or six), number of votes needed for conviction (majority or unanimity), and pre-vote deliberation. We find evidence of strategic voting under the unanimity rule: A large fraction of our subjects vote for a decision analogous to conviction even when their private information indicates a state analogous to innocence. This is roughly consistent with the game theoretic predictions of Feddersen and Pesendorfer. Although individual behavior is explained well by the game theoretic model, there are discrepancies at the level of the group decision. Contrary to Feddersen and Pesendorfer, in our experiments there are fewer outcomes analogous to incorrect convictions under unanimity rule than under majority rule. In the case of no deliberation, we simultaneously account for the individual and group data using quantal response equilibrium.

Recent research in political science addresses from a theoretical point of view the question of how individuals behave in juries. The classic Condorcet jury theorem deals with a model in which all jurors have identical preferences (they want to convict the guilty and acquit the innocent) but differ in their probability of making a correct decision. The theorem states that if the jury decision is made by majority rule, then the probability of a correct decision by the group is higher than that of any individual, and the probability of a correct decision goes to one as the size of the jury becomes very large.

Austen-Smith and Banks (1996) challenge the foundations of the Condorcet jury theorem by questioning whether any game theoretic basis can be given for the type of behavior assumed by the theorem. They demonstrate that if individuals start from a common prior about guilt of the defendant and then obtain private information, it is generally not a Nash equilibrium to vote sincerely (i.e., based only on one's private information). Subsequent work by Wit (1996) and McLennan (1996) reestablished that the conclusions of the theorem still hold if individuals vote strategically according to a symmetric mixed strategy equilibrium of the game.

Feddersen and Pesendorfer (1998) analyze the Nash equilibrium of the Condorcet jury theorem, with particular attention to the effect of the decision rule. They compare the unanimous decision rule with rules that require only a majority or supermajority to convict. They conclude that the unanimous rule results in probabilities of convicting an innocent defendant that

are higher than those for majority rule and that do not go to zero as the number of jurors goes to infinity.

The Feddersen and Pesendorfer model assumes that the jury decision is a simultaneous move game, in which all jurors vote without any communication beforehand. As Coughlan (2000) shows, if it is possible to have a straw poll before the vote, then there exist equilibria in which voters reveal their information in the straw poll and vote optimally in the actual vote based on the pooled information from the straw vote. This type of behavior would eliminate the unattractive aspects of unanimity, because decisions under majority rule should be identical to those under unanimity.

There has been relatively little experimental work to investigate the implications of these theories. The one exception is work by Ladha, Miller, and Oppenheimer (1996), who run experiments in which a jury makes a sequence of decisions under majority rule. They find some evidence of strategic behavior and that asymmetric Nash equilibria are sometimes played.

We report here on experiments run to test the Feddersen-Pesendorfer and Coughlan predictions. Our experiments thus consider groups faced with a decision problem isomorphic to the one modeled in those articles. We address the following questions: whether players vote strategically, whether unanimity rule leads to more incorrect convictions than majority rule, how this varies with group size, and whether the effects of a straw poll are the same as those predicted by Coughlan (2000).

As described in detail below, our experiments were conducted using an abstract group decision situation that avoided any direct reference to legal terms like "guilt" or "innocence." Specifically, the experimental implementation of the Feddersen-Pesendorfer jury game was done using colored jars and balls. For each group, the true state of the world (which we analogize here to either "guilty" or "innocent") was determined by randomly assigning the group a red jar (guilty) or a blue jar (innocent) with equal probability. The red jar contained predominantly red balls (guilty signals) and the blue jar contained predominantly blue balls (innocent signals). Before any voting occurred, each subject blindly selected one ball (with replacement) from her

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group's (randomly assigned) jar. The subject then observed the color of her selected ball, but did not observe the predominant color of the jar from which it was drawn, or the color of the balls selected from the same jar by the other members of the group. Then each group member privately voted for either a red jar (analogous to voting to "convict") or a blue jar (analogous to voting to "acquit"). The votes were then aggregated to form a group decision of red (convict) or blue (acquit) according to the specified voting rule, majority or unanimity. If the group decision matched the color of the group's assigned jar (analogous to convict if guilty, or acquit if innocent), then everyone in the group received a small token payment plus a large prize. If the group decision was incorrect, everyone in the group received only the small token payment. All of this was carefully explained in the instructions, which were read aloud (see Appendix A). We consider the effects of three treatment variables: voting rule (majority or unanimity), group size, and effect of a straw poll. In each treatment we consider individual and group behavior.

In the data, we find clear evidence of strategic behavior that is roughly consistent with the Feddersen-Pesendorfer game theoretic model. That is, a large percentage (between 30% and 50%) of subjects vote red (convict) even when they draw a blue ball (innocent signal). The percentage increases with the group size, as is predicted. Although individual behavior is explained well by the game theoretic model, there are some discrepancies between data and theory at the group level. For example, in our experiments there were fewer decisions analogous to incorrect convictions under unanimity rule than under majority rule, while Nash theory predicts the opposite. In the case of no straw poll, we are able to explain most of these discrepancies, using the logit version of quantal response equilibrium (QRE).

THE CONDORCET JURY MODEL

The Condorcet jury model applies to a general class of group decision problems in which members have a common interest but hold different beliefs about the true state of the world. By a common interest is meant that if the state of the world were common knowledge, then all group members would agree about which decision to make. The differences in beliefs create an information aggregation problem, which creates potential obstacles for the group to reach a consensus and make the "correct" decision. This class of decision problems has application to many settings, including juries in criminal and civil trials, corporate strategy decisions by boards of directors or partners, hiring and tenure decisions by faculty committees, examinations judged subjectively by committees, and so forth. The first of these, trial juries, was the subject of an article by Feddersen and Pesendorfer (1998), which is the main motivation for our work.

Several caveats are in order. First, the model we study applies to a broad class of settings, and it does not capture all the interesting institutional details of a single setting, such as a trial jury. Second, the model is an approximation, and it leaves out some contextual components that may affect behavior in specific applications. Third, it is meant to be simple, so that we can isolate and study certain phenomena of theoretical interest. Fourth, it is flexible, in the sense that one can analyze the model using a wide range of different assumptions about the degree of sophistication or rationality of group members. In this sense, although it is unquestionably a formal theoretical model, it allows one to explore the implications of bounded rationality as well as the implications of rational choice within the confines of the same model. It thus provides a very nice framework for conducting experiments to compare rational choice and bounded rationality.

Model Structure and Notation

We consider a game with a set $N = \{1, 2, ..., n\}$ of n players (e.g., jurors), and let $1 \le k \le n$ represent the number of votes needed for a positive decision (e.g., conviction). The game begins by nature choosing a state of the world in $\Omega = \{G, I\}^1$, with probability s and 1 - s, respectively. The players do not observe the state that is selected, but each obtains some private information about the state. If the true state is G, then each player observes an independent Bernoulli random variable, which is g with probability p and i with probability 1 - p. If the true state is I, then each juror observes an independent Bernoulli random variable, which is i with probability p and g with probability 1 - 1p. After observing their private information, players vote for one of two actions in $X = \{C, A\}^2$ If k or more players vote for C, then the group decision is C; otherwise the decision is A. The utility $u: X \times \Omega \mapsto \Re$ of each player is defined by u(A, I) = u(C, G) = 0, U(C, I) = -q, and U(A, G) = q - 1, where 0 < q < q

In all our experiments, we used the values s = .5, q = .5, and p = .7. We will be concerned with two different voting rules. For the majority rule case, k is the least integer greater than n/2; for the unanimity case, k = n.

We will distinguish between two kinds of behavior, which we call naive and strategic. Naive voters ignore the group strategy aspect of the decision problem and simply vote as if they were the only voter. In the jury setting, that means if they receive a guilty signal they vote to convict, and if they receive an innocent signal they vote to acquit. This is the kind of behavior assumed by Condorcet.

The second kind of behavior is strategic. Contemporary game theorists, including some political scientists (e.g., Austen-Smith and Banks 1996) argue that we should assume individuals behave strategically rather than naively. They also prove formally that naive and strategic behavior can have dramatically different logical implications in the Condorcet jury model. In particular, strategic behavior by jurors is modeled using

¹ Read "G" = Guilty, "T" = Innocent.

² Read "C" = Convict, "A" = Acquit.

	Majority				UnanImIty			
п	$\sigma(g)$	σ(I)	<i>Pr</i> [C /]	Pr[A G]	<u>σ(g)</u>	σ(I)	<i>Pr</i> [C /]	Pr[A G]
3	1.0000	0.0000	.2160	.2160	1.0000	0.3143	.1404	.4989
6	1.0000	0.0000	.0705	.2557	1.0000	0.6511	.1863	.4849
12	1.0000	0.0000	.0386	.1178	1.0000	0.8244	.2073	.4776
24	1.0000	0.0000	.0115	.0314	1.0000	0.9120	.2173	.4740

game theory, which predicts that under certain specific circumstances it is optimal for jurors to vote against their signal. Our predictions about strategic behavior in our experiments are given by the choice probabilities at symmetric Nash equilibria.

We also consider statistical versions of both types of behavior. Since we observe all kinds of choices in our experiment, it is necessary to introduce an error component to individual choices. For the case of naive behavior, we do this by assuming that naive subjects vote with their signal with some fixed probability, γ , and make an error with probability $1 - \gamma$. The probability of correct choice is assumed to be independent of the signal received and the same across all treatments. This becomes a free parameter of the naive behavior model, which allows us to fit the data to the model by standard estimation methods. For the case of strategic behavior, we incorporate the error structure into the equilibrium concept by using QRE. The QRE model (explained in more detail below) assumes that players may deviate with some probability from best responses, and the probability of deviation depends on the expected payoff difference between the best response and the deviation. We use a logit parameterization of QRE that includes a free parameter, \(\lambda \), which determines payoff responsiveness. Higher values of λ in the strategic model correspond approximately to higher values of γ in the naive model.

Strategic Behavior: Nash Equilibrium

To characterize equilibria, a strategy for a voter is a function $\sigma:\{g,i\}\mapsto [0,1]$, taking signals into account in the probability of voting for conviction. There are trivial equilibria to the above game in which voters ignore their information. Of special interest, however, are symmetric "informative" equilibria to the game. Symmetric equilibria require that players with the same signal adopt the same (mixed) strategy. Informative equilibria are those in which the players do not ignore their information.

As shown by Feddersen and Pesendorfer (1998), for the case of unanimity, the unique symmetric informative equilibrium requires that $\sigma(g) = 1$ and that the posterior probability of the defendant actually being guilty conditional on player i receiving an innocent signal and all other players voting guilty must be equal to q. That is,

$$q = \frac{(1-p)g_G^{n-1}}{(1-p)g_G^{n-1} + pg_I^{n-1}},$$

where

$$g_G = p\sigma(g) + (1 - p)\sigma(i) \tag{1}$$

and

$$g_I = (1 - p)\sigma(g) + p\sigma(i)$$
 (2)

are the probabilities that an individual votes to convict when the defendant is guilty or innocent, respectively. Using $\sigma(g) = 1$, this implies that

$$\sigma(i) = \frac{D_n p - (1-p)}{p - D_n (1-p)},$$

where

$$D_{n} = \left(\frac{(1-q)(1-p)}{qp}\right)^{\frac{1}{n-1}}.$$

Also, the probability of an incorrect jury decision to convict the innocent $(Pr[C|I] = (g_I)^n)$ or acquit the guilty $(Pr[A|G] = 1 - (g_G)^n)$ is determined from the above formula. Table 1 gives the equilibrium values of $\sigma(i)$, Pr[A|G], and Pr[C|I], respectively, for unanimity rule for certain values of n.

For majority rule, the formulas are slightly more complicated, but the symmetric equilibrium is simpler, namely, $\sigma(g) = 1.0$ and $\sigma(i) = 0.0$, regardless of the size of n. We can then compute the corresponding probabilities of convicting an innocent defendant: $(Pr[C|I] = \sum_{k>n/2} \binom{n}{k} (1-p)^k p^{n-k})$ and of acquitting a guilty defendant $(Pr[A|G] = \sum_{k \le n/2} \binom{n}{k} p^k (1-p)^{n-k})$. These numbers also are reported in Table 1 for the parameter values used in our experiment.

In the case of a straw poll, the game expands so that the players have two votes. A strategy is now a specification of how to vote in the straw poll, as a function of the player's signal, and then a specification of how to vote in the final vote, as a function of the signal and the observed outcome of the straw poll. Let $\sigma_0: \{g, i\} \mapsto [0, 1]$ be the probability of voting for conviction on the straw poll. Define $N_0 = N \cup \{0\}$ to be the possible outcomes of the poll. Then let $\sigma_1: \{g, i\} \times N_0$. In the case of a straw poll, Coughlan (2000) shows that, for the parameters used here, there is a fully informative equilibrium in which all players reveal their signals on the straw vote, that is, $\sigma_0(i) = 0$, and $\sigma_0(g) = 1$. They then vote based on the majority outcome of the straw

³ This is true for both the case of odd and even n, although it is somewhat more difficult to prove for even n, since the rule is no longer symmetric (n/2 votes to acquit suffice to acquit, but more than n/2 votes to convict are necessary to convict).

poll in the final vote, that is, $\sigma_1(s, M(\sigma_0)) = M(\sigma_0)$ for all signals $s \in \{i, g\}$, where $M(\sigma_0)$ is zero, one, or some appropriate mixing probability according to whether the majority outcome of the straw poll was to convict, acquit, or a tie.

Strategic Behavior: Quantal Response Equilibrium

The above solutions all assume no error. As a general way to incorporate decision error, McKelvey and Palfrey (1995, 1998) propose QRE, which is a statistical version of Nash equilibrium. 4 The basic idea is that it is unreasonable to expect individuals always to behave in perfect accord with rationality and always choose best responses to the other players. Instead, they choose better responses more often than worse responses. QRE is not a deterministic model but instead specifies a probability distribution, $\sigma^*(\cdot)$, over actions. The probabilities are ordered by the expected payoffs of the actions, $EU(\cdot)$, according to some specific function, called a quantal response function, which is just the statistical version of a best response function.⁵ Actions with higher expected payoff will be played more frequently, and actions with lower expected payoffs will be played less frequently. So, for any individual i and any pair of actions available to i, let us say a and b, $\sigma'(a) >$ $\sigma_i^*(b)$ if and only if $EU_i(a) > EU_i(b)$. The "E" of QRE stands for equilibrium, in the sense that the expected payoffs, $EU(\cdot)$, are themselves derived from the equilibrium probabilities, $\sigma^*(\cdot)$. One can think of an iterative process in which a given profile of choice probabilities for all the players' results determines a profile of expected payoffs for every action, which in turn (via the quantal response function) generates a new profile of choice probabilities. A QRE is just a fixed point of this iterative process. QRE retains the rational expectations flavor of a Nash equilibrium but relaxes the assumption that players choose optimal responses.

There are several ways to justify a formal model with the above properties. The idea that individuals choose stochastically rather than deterministically has been proposed for a long time, for example, to motivate reinforcement learning and discrete choice econometrics. Alternatively, one can "rationalize" stochastic choice if players have stochastic utility functions. Harsanyi (1973) proposes a model in this vein: The game payoff matrix is viewed as just an approximation of the utilities of the player over outcomes in the game, and each player's actual utility varies around these means according to some statistical distribution. In a QRE, for every action an individual might choose, there is a privately observed payoff disturbance for that action,

and one then looks at the Bayesian equilibrium to the corresponding game of private information. This is equivalent to smoothing the best response curves of the players and then looking at a fixed point of these smoothed response functions, which is exactly what QRE does.

We focus on a particularly tractable form of QRE, called logit QRE, in which the quantal response functions are logit curves. That is, for any pair of actions a and b, we let $\ln[\sigma(a)/\sigma(b)] = \lambda \cdot [EU(a) - EU(b)]$, where λ is a response parameter.

In our game players have different information and in some cases make a sequence of decisions. Therefore, we turn to the extensive form of the game; we represent profiles of action probabilities as behavior strategies and apply QRE to the "agent" form of this game. Formally, let $p = (p_1, \ldots, p_n)$ be a completely mixed profile of behavior strategies, where $p_i = \{p_{ijk}\}$ and p_{ijk} is the probability that player i, with signal $j \in \{i, g\}$, votes for $k \in \{c, a\}$. Let $\bar{u}_{ijk}(p)$ denote the expected utility to player i from taking action k with signal j, given p. Then p^* is a logit equilibrium if and only if, for all i, j, k,

$$p_{\psi k}^{*} = \frac{e^{-\lambda \mathcal{Q}_{\psi k}(p^{*})}}{\sum_{l} e^{-\lambda \mathcal{Q}_{\psi}(p^{*})}},$$

where again $\lambda > 0$ is a free parameter that determines the slope of players' logit response curves.

As we vary λ from 0 to ∞ , we can map out a family of QREs that correspond to different levels of rationality (or, more precisely, payoff responsiveness). When $\lambda =$ 0, response curves are completely flat, so all strategies are used with equal probability (pure error, or zero rationality). When λ approaches ∞ , logit response curves converge to standard best response curves, so players use only optimal strategies (no error, or perfect rationality). This family of QREs has several interesting properties, which are described by McKelvey and Palfrey (1995, 1998). For example, if we consider a convergent sequence of logit equilibria for a sequence of λ values converging to ∞ , the limit point must be a Nash equilibrium (or sequential equilibrium for extensive form games) of the underlying game. In this sense, Nash equilibrium is just a very special boundary case of QRE, which corresponds to perfect rationality.

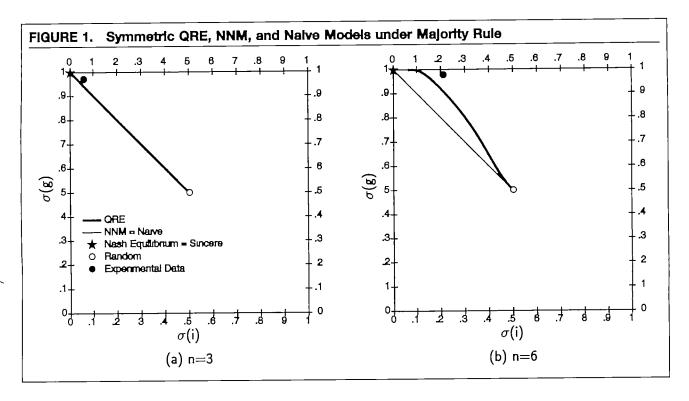
We also consider an alternate model of errors combined with strategic behavior, called the noisy Nash model (NNM), which also looks at statistical variation around the Nash equilibrium but differs from QRE in two ways. First, it does not incorporate the rational expectations assumption of QRE. The NNM model assumes that individuals follow Nash behavior with some fixed probability γ (to be estimated) and choose randomly with probability $1 - \gamma$. Like QRE, in the limiting case when γ approaches 1, the prediction approaches Nash equilibrium. Second, for intermedi-

⁴ See Rosenthal (1989), Chen, Friedman, and Thisse (1977) and Zauner (1999) for alternative formulations of similar ideas. The QRE approach has been used in various applied areas, such as the travelers' dilemma (Capra et al, 1999), all-pay auctions and public goods games (Anderson, Goeree, and Holt 1998a, 1998b), and mternational conflict (Signorino 1999).

⁵ We use a logit specification for the quantal response function, which is explained below

⁶ See McKelvey and Palfrey 1998 for details, where this version is referred to as the AQRE (for agent QRE). We use the notation QRE to refer to this version as well.

⁷ That is, they vote to convict or to acquit with equal probability.



ate values of the error term, the NNM will often differ from QRE. The difference is twofold. (1) NNM assigns the same probability of deviating from Nash equilibrium $(1 - \gamma)$, regardless of the expected utility loss from such a deviation. (2) NNM is not an equilibrium model. Recall that QRE is defined as a fixed point in terms of choice probabilities and logit responses. That is, each player's errors (deviations from Nash play) affect the expected payoffs of all the other players and hence will indirectly affect all other players' logit responses. In contrast, under NNM, there is no such feedback, so that one player's deviation from Nash play has no indirect effect on any other player's deviation from Nash play. See Fey, McKelvey, and Palfrey (1996), McKelvey and Palfrey (1998), McKelvey, Palfrey, and Weber (n.d.), for further discussion of the differences between QRE and NNM. Similar to QRE, we can map out a family of NNM predictions by varying the free parameter γ from 0 to 1.8 When $\gamma = 0$, all strategies are used with equal probability (pure error, or zero rationality). When γ approaches 1, predictions of NNM converge to Nash equilibrium.

In figures 1 and 2, the symmetric portion of the logit QRE correspondence for jury sizes n = 3 and n = 6 and both the majority and unanimity voting rules are displayed as the thick solid curves. Each graph is on the unit square of mixed behavior strategies of a representative player. The horizontal and vertical axes of each graph correspond to the probability of voting to convict, given innocent and guilty signals, respectively. At the center of each unit square is the "pure error"

logit equilibrium that corresponds to $\lambda=0$. As λ increases, the equilibrium curves converge to the symmetric Nash equilibrium, which is on the upper boundary of the unit square (the upper left corner, in the case of majority rule).

In a similar fashion, one constructs the correspondences defined by the NNM model and the naive (nonstrategic) model by varying γ between 0 and 1. Referring to figures 1 and 2, the NNM correspondence is simply the line segment that connects the center of the probability square (pure error) to the symmetric Nash equilibrium. The naive model correspondence is the dashed line segment that connects the center to the upper left corner of the strategy space. This vertex corresponds to "honest" voting.

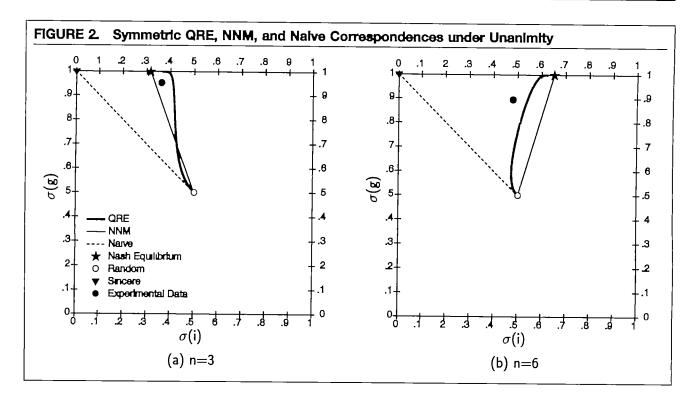
In Appendix B we fully characterize and compute the symmetric quantal response equilibrium correspondence for the three-person unanimous jury game with a straw vote. The characterization of the majority rule QRE correspondence is similar and is not included in the appendix. Our efforts to compute the symmetric quantal response equilibrium correspondence for majority rule juries with a straw vote were unsuccessful. Also, we found that the six-person jury game with a straw vote is too complex to compute the QRE correspondence, using our numerical methods.

EXPERIMENTAL DESIGN

We conducted four experiments with undergraduate and graduate students at the California Institute of Technology. Each experiment had twelve subjects (plus one subject who was used as a monitor). Each experiment had four sessions. Between sessions, two treatment variables were altered: the decision rule (majority or unanimity) and the straw poll (taken or not). The

As with QRE, we limit attention to the NNM corresponding to the symmetric mixed strategy Nash equilibrium.

⁵ Later we present and discuss the asymmetric components of this correspondence. The large dots in the figures are explained in the data analysis section.



treatment variables were manipulated according to the design in Table 2, which gives the particulars of each experiment. In each session, subjects participated in a sequence of fifteen "matches." In each match the subjects were randomly divided into groups of size n, where n was one of the treatment variables, and a game similar to that described in the previous section was conducted. In Table 2, the values of each treatment variable are given per session. For example, U/N (15) means 15 matches with the unanimity decision rule and no straw poll. All matches in the same experiment were run with the same number of subjects.

Subjects were not told that the experiment was intended to represent a jury decision. The states of the world were called the red jar and blue jar instead of guilty and innocent, and rather than a choice between convict or acquit, the subjects were instructed to guess whether the true jar was red or blue. The complete instructions are given in Appendix A.

Briefly, each match proceeded as follows. Subjects were told that there were two jars, a red one and a blue one. The red jar contained seven red balls and three blue; the blue jar contained seven blue balls and three red. One of the jars was selected for each group, as determined by the roll of a die by the monitor (a subject chosen at random from the group at the beginning of the experiment). ¹¹ The subjects were not told which jar had been selected, but they were each allowed to choose one ball at random from the jar. ¹²

They then voted for either the red jar or the blue jar. Two decision rules were investigated: majority rule and unanimity. The decision rule, which was explained to them before the session, was used to determine the group decision, and payoffs were based on whether the group decision was correct or incorrect. Each subject received fifty cents if the group decision was correct, five cents if incorrect. Subjects were paid in cash at the end of the experiment, plus a "show up fee" of \$5.

RESULTS

Jury Behavior without Deliberation

The first two columns of Table 3 show the realizations of the voter strategies in the case of no straw poll. As discussed in the previous section, our experiment was run with colored jars and balls to represent states of the world and evidence. We frequently interpret the results of the experiment, however, in terms of the underlying jury problem that the experiment represents. Thus, a red jar represents guilt, and a blue jar represents innocence. A red ball is evidence for guilt and a blue ball is evidence for innocence. A vote for the red jar represents a vote to convict and a vote for the blue jar is a vote to acquit. The columns of Table 3 and of subsequent tables are labeled to reflect this correspondence.

The data support our predictions for both the ma-

this procedure was conducted honestly, before the experiment we generated the order of the samples for each match, each group, each possible state, and each subject. The samples in the experiment were generated according to this list. Subjects recorded which ball they selected in each match, and they were free to peruse the list after the match to verify that the correct number of balls of each color were present, and that the ball they selected was of the correct color.

¹⁰ The last three sessions of experiment CJ1 were truncated to ten matches each, due to one particularly slow subject

¹¹ The die was rolled once for each group in each match, so in each match, different groups could have different states.

¹² This was accomplished by placing the balls in random order on their computer screen, with the colors hidden. Subjects then used the mouse to select one ball and reveal its color. To convince them that

TABLE 2.	First and Second T	reatments by	/ Experimental	Session		
Experiment	Number of	Group				
Number	Subjects	Sıze	1	2	3	4
CJ1	12	3	U/N (15)	M/N (10)	M/Y (10)	U/Y (10)
CJ2	12	3	M/N (15)	U/N (15)	U/Y (15)	M/Y (15)
CJ3	12	6	U/N (15)	M/N (15)	M/Y (15)	U/Y (15)
CJ4	12	6	M/N (15)	U/N (15)	U/Y (15)	M/Y (15)

jority rule treatment and the unanimity treatment. Under majority rule, the subjects voted the same direction as their signals more than 94% of the time; the only exception corresponds to the analogous case of innocent signals in a six-member jury, in which case the subjects erred 21% of the time. This seems like a surprising result but can be explained by the fact that the Nash equilibrium in the six-person majority rule experiments is weak. In the symmetric equilibrium of that game, jurors who receive innocent (blue) signals are indifferent between voting to acquit and voting to convict. In the QRE, this indifference leads to a prediction that players with innocent (blue) signals will vote against their signal more frequently than players with guilty (red) signals, for every positive value of λ .

In the case of unanimity, jurors with a red (guilty) signal tend to vote with the signal—strongly so for groups of three, less strongly for groups of six. However, when they got a blue (innocent) signal, 36% of subjects voted red (to convict) in the three-person groups, 48% did so in the six-person groups. For the three-person groups, this is very close to the Nash predicted value of .314. For the six-person groups, the rate is significantly below the predicted value of .651.

Since the Nash equilibrium of the game requires that in some cases a pure strategy is adopted, any observations in which subjects do not follow that strategy are enough to reject statistically the Nash equilibrium as a model of behavior. Thus, any game theoretic framework that explains the data must incorporate a model of the error source. Earlier, we proposed three alternatives that incorporate error: naive (nonstrategic), logit QRE, and NNM.

Table 4 gives the results of estimating the free parameters in these three models. Because of the symmetry of the game, all three make identical predictions for the three-person majority rule case. Also, the aggregate choice frequencies from all the nondeliberation data are superimposed (large dots) in figures 1 and 2. The first (and perhaps most important) thing to

observe is that the naive model does very poorly. In other words, voters are behaving as if they understand the strategic subtleties of the decision problem. For the NNM model we estimate γ to be in the 0.90 to 0.93 range for three-person groups. Since $(1 - \gamma)$ is the probability of choosing randomly, this corresponds to an error rate of less than 5%. For six-person groups, we estimate γ to be in the 0.75 to 0.78 range, which corresponds to an error rate slightly greater than 10%. For this size group, the QRE fits significantly better than the NNM under both majority and unanimity rules. In the case of three-person groups under unanimity, the fits of QRE and NNM are almost identical, with no significant difference between the two. The naive model is rejected in favor of the QRE for all treatments in which the two models make distinct predictions.

Group Behavior with Deliberation

As is evident from Table 3, in the case of a straw poll, the final vote cannot be predicted very well using the equilibrium of the Feddersen and Pesendorfer model. On average, subjects voted against a red (guilty) signal about 15% of the time, independent of group size or voting rule. With a blue (innocent) signal, between 16% and 37% vote to convict against their signal depending on the treatment. This is a higher rate of voting against signal than for those who obtain a red (guilty) signal, but we do not get differences as large as those between majority rule and unanimity as in the case of no straw poll.

Table 5 presents the results of the straw poll sessions based on the Coughlan equilibrium, which predicts that subjects will reveal their signal in the straw poll and then base their vote in the final round on the majority outcome of the straw poll. For the most part, the subjects do use the straw poll to reveal their signal. More than 90% in every cell (except 89.7% in one cell) revealed their signal in the straw poll. Of some interest

		Without S	Without Straw Poll		raw Poll
Rule	п	Blue (Innocent)	Red (Gulity)	Blue (Innocent)	Red (Gullty)_
Majority	3	.057 (157)	.972 (143)	.205 (146)	.838 (154)
	6	.209 (172)	.979 (188)	.248 (202)	.835 (158) .834 (151)
Unanimity	3 6	.360 (186) .478 (186)	.954 (174) .897 (174)	.161 (149) .372 (183)	.864 (177)

TABLE 4. QRE, NNM, and Naive Model Parameter Estimates of Three- and Six-Person Groups Three-Person Slx-Person Nash **QRE NNM** Naive f, Nash **ORE** NNM Nalve Majority .972 1.000 0.964 0.964 σ(**g**) 0.964 .979 1.000 0.960 0.889 0.889 $\sigma(l)$.057 0.000 0.036 0.036 0.036 209 0.000 0.158 0.111 0.111 $\lambda | \gamma$ 19.016 0.928 0.928 23.204 0.778 0.778 14.536 0.856 0.856 $\lambda_{\mathcal{B}}|\gamma_{\mathcal{B}}$ 19.016 0.708 0.708 21.642 0.953 0.953 29.757 0.837 0.837 53.738 53.738 53.738 110,201 125.580 125.580 Unanimity $\sigma(g)$.954 1.000 0.958 0.952 0.792 .897 1.000 0.887 0.878 0.704.360 0.314 0.407 0.3320.209 .478 0.651 0.535 0.614 0.297 λγ 9.197 0.903 0.583 12.041 0.755 0.407 7.689 0.827 γ_{lo} 0.496 11.921 0.643 0.311 10.893 0.954 0.662 12.530 0.845 0.498 154.897 154.350 184.227 188.016 194.057 219.409 Note: See Table 3 for number of observations (λ_{lo} , λ_{lo}) is a 95% confidence interval for λ . (γ_{lo} , γ_{lo}) is a 95% confidence interval for γ

is the additional finding that voting against innocent signals occurs with about twice the frequency of voting against guilty signals in all four treatments.

In the final vote, when the outcome of the straw poll is not a tie, under all treatments players vote with the public signal at least 83% of the time. One might expect the proportion to be higher. In an equilibrium of the Coughlan type, individuals in the final vote should ignore their own signal and only pay attention to the public signal. Table 6 gives the result of a probit analysis of the final vote by the individual's private signal and the publicly available information. For public information, we use the number of other individuals who voted red (to convict) in the straw poll plus the information of the individual (1 for a red [guilty] signal, 0 for a blue [innocent] signal). If voters are following the Coughlan equilibrium, then they should base their vote on the public information and ignore their private signal. The probit analysis indicates that this is not the case. The individual's private information has a significant effect on the final vote for all combinations of treatment variables.

We believe there are several reasons the straw poll does not work exactly as predicted by Coughlan (2000). The simplest explanation is based on the results of the treatment without a straw poll. Even in the simplest group—three-person majority rule—some players fail to vote sincerely. This source of error causes a small effect if there is no straw poll, and the QRE discussion in the last section showed how these small effects can be accounted for using an equilibrium model with

errors. With a straw poll, the relatively small effects of errors in the initial voting stage become compounded in the second stage, since the noisy behavior in the straw poll means that individuals are not sure how to interpret the poll results. If they believe there is some likelihood that others will not perfectly follow the first-stage equilibrium, then they *should* give some weight to their own private information in the second stage. This has a snowball effect on straw poll behavior, because they know that others will be uncertain how to interpret the straw vote. Since QRE is an equilibrium model, it can capture this effect of compounding errors.

Appendix B presents a QRE analysis of the straw vote game for the three-person unanimity treatment, including a table that presents the maximum likelihood estimates of λ and γ . Our computational algorithm failed to converge in its computation of the QRE correspondence for the majority rule game or the six-person games. The main findings are summarized as follows, and a more detailed account is contained in Appendix B.

There are several important features of the QRE correspondence. First, for higher values of λ , there are multiple symmetric equilibria that correspond to the various Nash equilibria. Second, the graph is not well behaved, as it contains two points of bifurcation. Third, one of the branches corresponds to the informative equilibrium studied by Coughlan (2000), and it is the component of the QRE correspondence that most closely matches the data. Fourth, this component has the feature that voters condition their final vote on

TABLE 5. Voting with a Straw Poli: Proportion Voting to Convict by Prior Information (Straw Vote by Signal, and Final Vote by Straw Poli Result)

		Straw Vote		Final Vote			
Rule	п	Blue (Innocent)	Red (Gullty)	Blue (Innocent)	Tle	Red (Gullty)	
Majority	3	.103 (146)	.968 (154)	.111 (144)		.917 (156)	
-	6	.089 (202)	.994 (158)	.135 (156)	.606 (66)	.877 (138)	
Unanimity	3	.067 (149)	.954 (151)	.160 (156)	_	.868 (144)	
	6	.049 (183)	.989 (1 <i>77</i>)	.167 (120)	.657 (102)	.971 (138)	

	Decision Rule						
	Ma	ortty	Unanlmous				
Varlab le	n = 3	, n = 6	n = 3	n = 6			
Constant	-1.891	-2.213	-1.799	-2.039			
O State	(-9.468)	(-10.080)	(-9.363)	(-9.285			
Public Information	1.016	` 0.618 [′]	0.810	0.723			
straw poli)	(7.355)	(8.540)	(6.407)	(9.298			
Private Signal	0.874	`1.154 [´]	1.136	0.751			
Tivato oigina	(4.068)	(6.487)	(5.276)	(4.037			
Number of observations	300	360 ´	300	360			
Log likelihood	-109.82	-138.46	-110.26	-130.13			
% correctly predicted	89.0	82.2	83.7	84.17			

their own signal as well as the vote outcome in the final round. In particular, in the jury setting, for any fixed number of votes to convict in the straw vote, the probability of voting to convict in the final stage is higher if one observes a guilty signal than if one observes an innocent signal. Fifth, the probability of voting to convict in the final vote is monotonic in the number of straw votes to convict.

These features of the QRE are consistent with the data and with the simple probit analysis of Table 6. Yet, although the main qualitative predictions of QRE are found in the data, the quantitative fit is less successful. In fact, the maximum likelihood fit of the NNM is better than the fit of the QRE model. Further details and discussion are in Appendix B.

Group Decision Accuracy

Table 7 summarizes the accuracy of the final decision as a function of the experimental treatment variables. We compare the actual data with the Nash equilibrium predictions of error rates, given in Table 1. First, in the experimental data with no straw poll, under unanimity the probability of voting incorrectly when the true state is blue (to convict an innocent defendant) decreases from .190 to .029 in our data as the group becomes larger (this difference is significant at the .05 level using a difference of proportion test, with t=2.223). The Nash theory predicts the opposite (from .14 to .19). Furthermore, in the blue (innocent) state, the error rate for six-person unanimous groups is lower than the error rate for majority groups (.03 vs. .30). This differ-

ence (significant at the .01 level, t=2.924) is also counter to the Nash theory. As for majority rule groups, error rates decline with larger size in the true red (guilty) state and increase with larger size in the blue (innocent) state. This is exactly the opposite of what is predicted in the Nash equilibrium.

We view the contradictions with the aggregate theoretical predictions as surprising, especially because the individual choice frequencies are not very different from the theoretical levels. We interpret this to mean that the accuracy of group decisions is not a robust phenomenon. That is, small changes in individual choice behavior can result in large changes in the probability of an erroneous group judgment. This is especially true for unanimity rule, where our data suggest that a small amount of individual decision error can produce a much larger number of "acquittals" than is predicted by the Nash equilibrium.

As evidence in support of this claim, we also computed expected group accuracy under the QRE model. To compute these values, we used the maximum likelihood QRE estimates of $\sigma(i)$ and $\sigma(g)$ from Table 4 and substituted into equations 1 and 2 to get values of g_G and g_I . Then the probabilities P(C|I) and P(A|G) were computed by the corresponding binomial formulas used in Table 1. We see that the accuracy implications of the QRE estimates match the data better than the Nash predictions. All the above discrepancies between the data and the Nash predictions except one are resolved by the QRE predicts that, under unanimity, the probability of convicting an inno-

			Without Straw Poll True State					With Straw Poll True State			
	п	Innocent (Blue)		Gullty (Red)		Innocent (Blue)		Gulity (Red)			
Rule		Data	Nash	QRE	Data	Nash	QRE	Data	Nash	Data	Nash
Majority	3	.175 (40)	.216	.234	.300 (60)	.216	.234	.226 (53)	.216	.191 (47)	.216
Majority	6	.296 (27)	.071	.177	.212 (33)	.256	.221	.176 (34)	.071	.115 (26)	.256
Unanimity	3	.190 (63)	.140	.187	.526 (57)	.499	.502	.140 (50)	.216	.360 (50)	.216
Oracilling	6	.029 (34)	.186	.069	.731 (26)	.485	.772	.107 (28)	.071	437 (32)	.256

cent defendant should decrease (from .19 to .07) as the size of the jury increases. This is consistent with the data.

The experiments with a straw poll suggest that the poll increases the accuracy of judgments in the guilty state but has essentially no effect in the innocent state. In a jury setting, it appears that the probability of convicting the innocent does not increase with deliberation, but the probability of acquitting the guilty declines substantially. In this case we can compute the Nash predictions. With a straw vote, the fully informed Nash equilibrium predicts that both majority and unanimous juries of the same size should have identical accuracy. This is true since the equilibrium strategy is to reveal individual signals in the straw poll, and then vote on the final ballot based on whether the number of reported guilty signals exceeds $\frac{\pi}{2}$.

Group Accuracy In the QRE

As pointed out earlier, Feddersen and Pesendorfer (1998) imply that large unanimous juries will convict innocent defendants with fairly high probability, whereas the likelihood of such errors in large majority juries will vanish. Even in juries of six, the probability of convicting the innocent is predicted to be higher with unanimity rule than with majority rule. This did not happen in our experiment. We found in both the three- and six-person groups that errors of this kind are more prevalent with majority than with unanimity rules.

We now show that our empirical findings are consistent with the logit equilibrium. Specifically, for any logit parameter of $\lambda < \infty$, the probability of conviction goes to zero in large unanimous juries. In contrast, for any $\lambda > 0$, the probability of a majority-rule error in the symmetric informative logit QRE goes to 0 as jury size increases, regardless of the state of the world. We first show that the probability of an individual innocent vote in a logit QRE is bounded below by an expression that is independent of n.

Theorem 1. Fix $\lambda < \infty$. For every $\delta > 0$, there exists N (δ , λ), such that for all n > N (δ , λ) the probability of acquittal in any logit QRE is greater than $1 - \delta$, regardless of whether the defendant is innocent or guilty.

Proof. Writing p_{iga}^* and p_{igc}^* for the probability that an individual votes to acquit or convict, respectively, with a guilty signal, it follows that in any logit QRE,

$$p^*_{\frac{1}{2^{n}}} = \frac{1}{1 + e^{\lambda [\frac{n}{2^{n}}(p^*) - \frac{n}{2^{n}}(p^*)]}} > \frac{1}{1 + e^{\lambda}},$$

since $\bar{u}_{ige}(p^*) - \bar{u}_{ige}(p^*) < 1$. A similar argument establishes that $p_{ua}^* > 1/1 + e^{\lambda}$.

The theorem now follows immediately, because the lower bound on both p_{iga} and p_{iia} is independent of n. Q.E.D.

THEOREM 2. Fix $\lambda < \infty$, and consider the logit QRE of the unanimous jury game with a straw poll. For every $\delta > 0$, there exists N (δ, λ) , such that for all $n > N(\delta, \lambda)$ the

probability of acquittal is greater than $1 - \delta$, regardless of whether the defendant is innocent or guilty.

Proof. The argument from the previous theorem applies to the final stage of the straw poll in exactly the same way as it applies to the case with no straw poll. Therefore, the lower bound on p_{iga} and p_{iia} identified in the previous theorem is the same and is independent of n. The result follows immediately.

Q.E.D.

We see that (logit) QRE behavior is entirely consistent with the traditional jurisprudential theory that argues for unanimous juries as a protection against conviction of the innocent. The reason is that the Nash equilibrium conviction/acquittal probabilities are not robust to decision errors. In order for the probability of convicting the innocent to increase, the probability of voting to acquit must go to zero extremely fast in the number of jurors (on the order of 1/n), since the probability of conviction is equal to $(1 - p_{10}^*)^n$. When the probability of voting to acquit goes to 0 any slower than this, defendants will always be acquitted by large juries. That is, unanimous juries will become completely uninformative.

In our unanimity rule data, we found two interesting results that are relevant to the jury setting. First, both with and without straw polls, the innocent are wrongly convicted less frequently by large juries than small juries. Second, both with and without straw polls, the probability of acquitting the guilty increases with n. Both of these findings are the opposite of what Nash theory predicts, but they are consistent with the logit QRE, which predicts that unanimous juries will become more heavily biased toward acquittal as their size increases. Under majority rule this acquittal bias does not occur, but at the nontrivial cost of roughly 50% higher wrongful conviction rates of innocent defendants.

Individual Behavior, Heterogeneity, and Asymmetric Equilibria

The theoretical work underlying this experiment focuses entirely on symmetric informative equilibria. As was noted in passing, there are many equilibria in these voting games, all of which satisfy standard refinement criteria, such as perfection, properness, and stability. We now investigate whether our data can be interpreted as evidence either for uninformative equilibria or for asymmetric equilibria.

There is clear evidence against uninformative equilibria. The unique uninformative equilibrium for the unanimity game has all players always voting to acquit, independent of their actual signal. In contrast, Table 7 clearly showed that voting is very informative in the unanimity games, with and without straw polls. We conclude that our data do not provide evidence of uninformative equilibria.

The issue of asymmetric equilibria is both more subtle and more problematic. A necessary condition for asymmetric equilibria is some evidence of heterogeneity in the observed decision rules of different players within the same treatment. We find strong evidence for heterogene-

TABLE 8. Breakdown of Strategy Types in Unanimous Juries without Deliberation (Innocent Signals Only) Session 4 Total 2 3 1 Strategy Type 0 (0/0) 11 (0/93) 3 (0/27) 4 (0/31) 4 (0/35) 1: Sincere 10 (32/75) 28 (95/213) 6 (19/41) 7 (24/57) 5 (20/40) 2: Mlxer 3 (23/23) 9 (61/61) 2 (15/15) 3(18/18)1(5/5)Convicter Note: (Guilty Votes/Innocent Signals) is in parentheses

ity in our data. As a simple way to categorize behavior, we divide players into three strategy types, based on how analogous jurors would vote when they receive an innocent signal. Strategy type 1 always votes sincerely, that is, $\sigma(i) = 0$. We call these honest voters "sincere." Strategy type 2 mixes when an innocent signal is received, that is, $\sigma(i) > 0$. We call these voters "mixers." Strategy type 3 always votes to convict, independent of the signal, that is, $\sigma(i) = 1$. We call these voters "convicters." To implement this classification, we use frequency of choices.

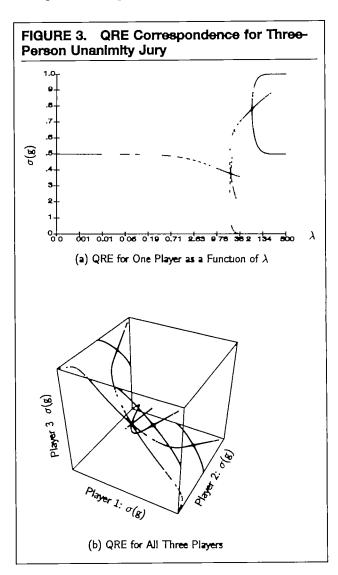
Table 8 presents the breakdown of strategy types by session. Overall, 23% of the voters are always sincere, 58% mix, and 19% always vote to convict.

The numbers in parentheses indicate the number of guilty votes and the total number of innocent signals observed by all voters classified in that cell. For example, in session 1, in the unanimity/no-deliberation treatment, four subjects always voted sincerely when they received an innocent signal. They received a total of 31 innocent signals and voted to acquit in every instance. In that same session, seven subjects mixed, that is, they neither always voted to acquit nor always voted to convict when they received an innocent signal. They received a total of 57 innocent signals and voted to convict 24 times.

To illustrate the range of possible asymmetric equilibria in jury games, consider the simplest case of three-person juror unanimity juries with no deliberation. In this case there is an equilibrium with two convicter types and one sincere voter. To see that this is an equilibrium, first look at the convicters. Either of them is pivotal if and only if the sincere player votes to convict, which happens only if she receives a guilty signal. Thus the best response is to vote to convict with a guilty signal and to vote either to convict or to acquit (indifferent) with an innocent signal. For the sincere voter, since the other two players are always voting to convict, conditioning on being pivotal is uninformative, so sincere behavior is strictly optimal for either signal. Using similar reasoning, one can easily show that for any probability $p \in [0, 1]$, there is an equilibrium in which one sincere voter, one punitive, and one mixer will vote to convict with a innocent signal with probability p (and always vote to convict with a guilty signal). An even wider range of asymmetric equilibria exist with six-person juries. Ladha (1998) has shown that the pure strategy asymmetric Nash equilibria maximize the accuracy of information aggregation in jury games.

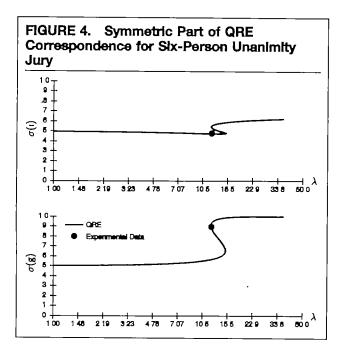
We have not fully characterized the asymmetric equilibria, but the point is clear. There are lots of equilibria, so the empirical restrictions of equilibrium behavior are limited. But we point out that in all these equilibria for three-person juries, the aggregate probability of a vote to convict, given an innocent signal, is above one in three.

The issue of asymmetric equilibria is further complicated if one takes into account the possibility that subjects may make errors. In Figure 3 we plot the full QRE correspondence for the three-person unanimity game under the assumption that iteratively dominated strategies are not played (i.e., a player who receives a



¹³ Since nearly all jurors always vote to convict with a guilty signal, we do not break down the individual strategy types based on that.

¹⁴ There are no voters who always vote to acquit.



guilty signal always votes guilty). Even under this assumption, it is evident from the figure that the full QRE correspondence is quite complicated. First, it bifurcates in several places. This is not a generic feature of the QRE correspondence, and it happens here because the game is symmetric and hence is not generic. In generic games, as shown by McKelvey and Palfrey (1995, 1998), the principal branch of the ORE correspondence selects a unique equilibrium (i.e., there is no bifurcation). For nongeneric games, such as these jury games, such a selection is no longer possible. In this case, the forks or bifurcations in the QRE correspondence are places where the equilibrium correspondence branches to connect to the asymmetric equilibria. Thus, if we drop the focus on asymmetric equilibria, the QRE gives no guidance as to which asymmetric equilibrium should be selected. Second, if subjects make errors, then in addition to the limiting points of the correspondence, one predicts that other points on the correspondence could occur. If there is a failure to coordinate on any equilibrium, then matters are further complicated.

Figure 3 deals only with the three-person jury. For the case of a six-person jury, matters become even more complicated and there are too many parameters to solve. Figure 4 plots the symmetric part of the QRE correspondence for a six-person jury using the unanimity rule. This graph is the same as in Figure 2 but is viewed from a different projection into the coordinate axes. In this view, we see that even the principal branch of the symmetric part of the QRE correspondence is not monotonic in λ. Thus, if one follows the principal branch of the QRE starting at $\lambda = 0$, one reaches a point (at about $\lambda = 15$) where the curve bends backward for a while. In this region, the symmetric branch of the QRE is multiple valued. For the six-person case, we have not been able to compute the full QRE correspondence that would include the asymmetric QRE. But since there is no unique selection of a

symmetric QRE for some values of λ , this may even increase the probability of a lack of coordination as well as the tendency toward asymmetric behavior.

LEARNING

The game we implemented in the laboratory is complicated. We asked subjects to make choices based on limited partial information in an uncertain environment with asymmetric information. They were not guided by any clues that would make it easy to connect the group decision problem they were solving to reallife situations that they have experienced. This was intentional, since we did not wish to bias their decisions or distort their induced preferences in ways that are difficult to predict or measure. Furthermore, in order to make the most of this information, they needed to anticipate how other subjects in the room would make decisions based on their (different) partial information.

Given the complexity of the task, we expected to find trends in the data. For example, the concept of conditioning a decision on the event that one's vote is pivotal is a very subtle notion, yet that is the main idea underlying the equilibrium of these games. It is conceivable that in early rounds subjects are less aware of this equilibrium effect and experience is required before it affects their decision making. The difficulty of the task was one factor that motivated the repeated trials feature of our design.

For the majority rule experiments, the aggregate data are a close match to the equilibrium prediction that voters will be sincere. This is not surprising, since the simple intuitive rule of voting sincerely corresponds to the optimal decision rule when one conditions on being pivotal. In six-person groups with majority rule, we found significantly more deviation from sincere voting, but this can be explained by the fact that voting sincerely is only a weak equilibrium in a group that size. That is, with a blue (innocent) signal, a voter is indifferent between voting red (guilty) and voting blue (innocent).

To test whether the observed phenomenon of strategic voting was partly the result of learning, we focus on the decisions by voters with blue (innocent) signals in the unanimity treatments with no straw poll. This was the only case in our experiment in which players are predicted to vote against their signal in the Nash equilibrium. For these voters, we compared the frequency of voting against their blue signal (to convict) for the first five rounds of data and the last five rounds of data. In all four sessions, the frequency of voting red (guilty) with a blue (innocent) signal in the last five rounds was higher than in the first five rounds and was closer to the Nash equilibrium prediction.

As expected, the time trend data from other voter types in other treatments do not show much of a pattern. Breaking the data down by session, treatment, and signal (a total of 28 cases), we found that in 61% of the cases the decision frequencies moved closer to the Nash equilibrium or remained constant in the later periods, ¹⁵ whereas in 54% of the cases the decision

¹⁵ In all but one of the cases in which there was no change between

frequencies were farther from the Nash equilibrium in the later periods.

CONCLUSION

Our experiments reveal a definite tendency to vote strategically in the case of a unanimous rule and no straw vote. In three-person groups more than one-third of the time players with blue (innocent) signals voted against this signal, which is very close to the Nash equilibrium prediction. In six-person groups, this frequency increased significantly, also as predicted by the theory, but the increase was not as great as Nash equilibrium predicts.

Individual behavior is explained well by the game theoretic model, but at the level of group decisions, there are numerous discrepancies. In particular, contrary to Feddersen and Pesendorfer (1998), in our experiments there were fewer decisions analogous to incorrect conviction under unanimity rule than under majority rule, and larger juries may convict fewer innocent defendants than smaller juries under unanimity. We simultaneously accounted for the individual and group data by using quantal response equilibrium to model the error.

Our study suggests that during deliberation, which we implemented experimentally with nonbinding straw polls, voters tend to reveal their signals but not to the full extent predicted by the theory. Most of them then use the public information to make their final vote. Thus, the introduction of straw polls largely (but not completely) eliminates strategic voting by juries in the case of the unanimity rule. Surprisingly, straw polls may lead to more strategic voting under the majority rule. The fact that there is a residual of strategic revelation even with straw polls results in some departure from the "sincere" equilibrium path of the final stage. As expected, it appears that straw polls increase the frequency with which juries make correct decisions in the case of a guilty defendant.

APPENDIX A: EXPERIMENT INSTRUCTIONS

Decision-Making Experiment (Condorcet Jury)

This is an experiment in group decision making, and you will be paid for your participation in cash, at the end of the experiment. Different subjects may earn different amounts. What you earn depends partly on your decisions, partly on the decisions of others, and partly on chance.

The entire experiment will take place through computer terminals, and all interaction between you will take place through the computers. It is important that you not talk or in any way try to communicate with other subjects during the

We will start with a brief instruction period. During the instruction period, you will be given a complete description of the experiment and will be shown how to use the computers. If you have any questions during the instruction period, raise

the first and last periods, all the early data are exactly at the Nash equilibrium.

your hand, and your question will be answered so everyone can hear. If any difficulties arise after the experiment has begun, raise your hand, and an experimenter will come and assist you.

In this experiment, one subject will act as a monitor. The monitor will be paid a fixed amount (\$20.00) for the experiment. The monitor will assist in running the experiment by generating random numbers for use in the experiment, and serving as someone who can check that the experiment is run correctly. We will select the monitor by having you each select an envelope. Open your envelope, and read the card inside to determine if you are the monitor.

[PASS OUT ENVELOPES]

Will the monitor please go to the master computer, and we will now pass out record sheets to the subjects in the experiments.

PASS OUT RECORD SHEETS

Please put your name at the top of the record sheet. The experiment you are participating in is broken down into 4 separate sessions. Each session will consist of 15 matches. At the end of the last match of the last session, you will be paid the total amount you have accumulated during the course of the 4 sessions. Everyone will be paid in private, and you are under no obligation to tell others how much you earned. Your earnings are denominated in francs. 16 Your dollar earnings are determined by multiplying your earning in francs by a conversion rate. In this experiment, the conversion rate is .005, meaning that 100 francs is worth 50 cents.

We will now begin the computer instructions for the first session. We will teach you about the experiment and how to use the computer by going through a short practice match. During the instruction session, do not hit any keys until you are told to do so, and when you are told to enter information, type exactly what you are told to type. You are not paid for the practice match.

START JURY PROGRAM

When the computer prompts you for your name, type your full name. Then hit the ENTER key, and confirm by typing Y.

First Screen

You now see the first screen of the experiment. At the top center of the screen, you see your subject number. Please record that on your record sheet now. This session consists of 15 matches. In each match, you will be matched with another group of subjects. Your group number and the number of subjects you are matched with is on the top right side of your screen. In this session, each of you is matched with two other subjects, in groups of size 3.

There are two jars, which we call the red jar and the blue jar. The red jar contains 7 red balls and 3 blue balls. The blue jar contains 7 blue balls and 3 red balls. At the beginning of the match, we will randomly choose one of the two jars for your group. This will be done by the monitor, who will roll a die to determine the color. If an odd number is rolled, the jar will be blue; if an even number is rolled, the jar will be red. Different groups may have different colors. You will not be told which jar has been chosen. You may now press a key to advance to the next screen.

¹⁶ The use of francs (or some other form of artificial currency) is common in experimental work, and is done primarily to allow for calibration of total payout via adjusting the exchange rate rather than adjusting the payments on the payoff tables viewed by the subject. This allows numbers in the payoff tables to be in whole numbers and avoids the use of decimals.

Second Screen

To help you determine which jar has been selected for your group, each member of your group will be allowed to select one ball, at random, from the jar. This is done on the computer by randomly ordering the balls in the jar for each subject, but leaving the colors hidden. You can then use the mouse to click on one of ten balls. When you select a ball, its color will be revealed to you and displayed on your screen, and you must record this information on your record sheet. This is called your "Sample." You are not told the colors of the sample balls drawn by the other members of your group. Note that the balls are numbered. This is just so that you can remember and record which ball you selected.

Each subject in your group selects a ball on their own, and only sees their own sample ball. The balls are ordered differently for each subject. However, all the members of your group have the same number of red and blue balls. That is, if the true color of the jar for your group were red, then all members of your group are drawing their sample balls from a list containing 7 red and 3 blue balls. If the true color of your group's jar is blue, then all members of your group would be drawing their sample balls from a list containing 3 red and 7 blue balls. Please select a ball now, and record its color and number on your record sheet. When you have finished recording, press a key to continue.

Third Screen

After everyone has drawn the sample, you will be asked to vote for either a red jar or a blue jar.

[READ EITHER A OR B]

- A Unanimity instructions: If (and only if) everyone in your group votes red, your group decision is red. Otherwise, your group decision is blue. That is, if anyone in your group votes blue, the group decision is blue.
- B Majority rule instructions: If a majority (two or more) of your group votes red, then the group decision is red. Otherwise, your group decision is blue. That is, your group's decision will be whichever color receives more votes.

The table on this screen tells you your payoffs. If your group's decision is the same as the true color, then the decision is correct, and each member of your group earns 100 francs. If your group decision is different from the true color of the jar, then the group decision is incorrect, and each member of the group earns 10 francs. You may vote by selecting, with your mouse, either the red or the blue jar and then confirming. In the actual experiment, you may vote however you like. In the practice match, please vote red if you are on the left side of the room, and blue if you are on the right side of the room. Please vote now.

Fourth Screen

After everyone has voted, the vote is tallied, and you and the others in your group will be told what the result of the vote was, and what the true color of the jar for your group was. Please hit a key to proceed.

Fifth Screen

After all decisions have been made, you are given a summary of the decisions up to now. Please record this information on your record sheet. This marks the end of the match. After you have recorded all of the information, please hit a key to continue.

Sixth Screen—Display Overhead

We will now illustrate in more detail how the sample is drawn. Your computer screen now shows the balls you drew your sample from with all of the colors revealed. The ball that you drew is outlined with a white box.

Prior to the experiment, all of the lists of sample balls for each subject, and each match, and each possible true jar color have been generated by a random number generator. We have a printout of this information that will be available to the monitor. You now see on the overhead projector screen a copy of the first page of this data, which is for the practice match. Find your subject number and the true color of your group's jar. Compare the display on your computer screen to that on the overhead projector. The color of the balls should be identical. Please look at the overhead projector screen. Note also that the ordering of the balls for all subjects is different, even if they are drawing from the same jar.

During the actual experiment, you will not be shown this screen. However, your samples will be done in exactly the same way. We have printed out the similar sheet for all of the matches to follow. If you record the number of the ball that you drew each match, then at the end of the experiment, if you wish, you may ask the monitor to check your sample against this sheet to verify that the ball you selected was the correct color.

After a match is over, and everyone has recorded the information about their match, you will be randomly rematched into another group consisting of different subjects. A new jar will be randomly drawn for your group, you will get a new sample from a jar of the group color, you will vote, and a group decision will be determined as described above. After we have finished 15 matches in this manner, the first session of the experiment will be over, and we will give you instructions for the next session.

Are there any questions before we begin with the actual experiment?

[EXPERIMENTER TAKES QUESTIONS]

We will now begin with the actual experiment. If there are any problems from this point on, raise your hand, and an experimenter will come and assist you.

[START EXPERIMENT]

Rules for Experiment Session #2

This experiment session will also last for 15 matches. The rules are the same as in the first session, with one exception.

[READ A OR B]

- A Recall that in the previous session, your group decision was red if and only if all three members of your group voted red. Otherwise your group decision was blue. In this session there will be a slight change in the voting rule. Now, if a majority (two or more) of your group votes red, then the group decision is red. Otherwise, your group decision is blue. That is, your group's decision will be whichever color receives more votes.
- B Recall that in the previous session, your group decision was red if two or more members of your group voted red. Otherwise your group decision was blue. In this session there will be a slight change in the voting rule. Now, if (and only if) everyone in your group votes red, your group decision is red. Otherwise, your group decision is blue. That is, if anyone in your group votes blue, the group decision is blue.

Are there any questions?

Rules for Experiment Session #3

This experiment session will also last for 15 matches. The rules are the same as experiment session #2, with one exception. In this experiment, each group will conduct one preliminary vote before making the final vote. In the preliminary vote, you may vote for either the red jar or the blue jar. You do not receive any earnings for the preliminary vote. The number of blue and red votes in the preliminary vote will be revealed to you and the other members of your group. After you have been told this preliminary vote outcome, your group will conduct the final vote, in the same manner as in experiment session #2. We will conduct a practice session to illustrate how the preliminary vote is conducted.

Are there any questions?

Rules for Experiment Session #4

This experiment session will also last for 15 matches. The rules are the same as experiment session #1, with one exception. As in the previous experiment, each group will conduct one preliminary vote before making the final vote. You do not receive any earnings for the preliminary vote. The outcome of this preliminary vote will be revealed to you and the other members of your group, including the exact number of red votes and blue votes. You are not paid anything for this outcome. You must record this information on your information and record sheet. Then your group will conduct the final vote, in the same manner as experiment session #1. That is,

[read A or B]

- A Unanimity instructions: If (and only if) everyone in your group votes red, your group decision is red. Otherwise, your group decision is blue. That is, if anyone in your group votes blue, the group decision is blue.
- B Majority rule instructions: If a majority (two or more) of your group votes red, then the group decision is red. Otherwise, your group decision is blue. That is, your group's decision will be whichever color receives more votes.

Are there any questions?

APPENDIX B: SOLUTION FOR THE CASE N = 3, UNANIMITY, WITH STRAW POLL

Notation and Strategles

We use the notation of Feddersen and Pesendorfer (1998) and Coughlan (2000) in this appendix. To translate to the experiment, read guilty as "red," innocent as "blue," convict as "vote red," and acquit as "vote blue."

Denote by:

- $s = \{G, I\}$ the signal (either guilty or innocent);
- p = the probability of receiving the correct signal;
- $v_t = \{C_t, A_t\}$ the vote (either convict or acquit) in the straw poll (t = 0) and in the decisive final vote (t = 1);
- i = {0, 1, 2} the number of other convicting votes in the straw poll;
- $j = \{0, 1, 2\}$ the number of other matching signals.

Payoffs are defined as follows:

 0 for making the right decision (that is, convicting the guilty and acquitting the innocent);

- q for acquitting a guilty defendant (where $q \in [0, 1]$);
- q-1 for convicting an innocent defendant.

Each agent's strategy consists of a pair of probabilities, σ_0 , that specify the likelihood of voting to convict in the straw poll (as a function of one own's signal) and in the final vote (as a function of one's own signal, one's own vote in the straw poll, and others' vote in the straw poll):

$$\sigma_0(s) = \frac{e^{\lambda E U_{EP}(C_0, s)}}{e^{\lambda E U_{EP}(C_0, s)} + e^{\lambda E U_{EP}(A_0, s)}},$$
(3)

$$\sigma_1(s, v_0, i) = \frac{e^{\lambda EU_{PV}(C_1, s, v_0, i)}}{e^{\lambda EU_{PV}(C_1, s, v_0, i)} + e^{\lambda EU_{PV}(A_1, s, v_0, i)}}, \qquad (4)$$

where EU_{SP} (EU_{FV}) is the expected utility for casting a given vote in the straw poll (final vote).

Expected Utility for the Straw Poll

$$EU_{SP}(v_0, s) =$$

$$\begin{cases} \sum_{j=0}^{2} \sum_{i=0}^{2} \pi(j) \mu(i,j) EU_{FIN}(G, \nu_{0}, i) & \text{if } s = G \\ \sum_{j=0}^{2} \sum_{i=0}^{2} \pi(j) \mu(i, 2-j) EU_{FIN}(I, \nu_{0}, i) & \text{if } s = I \end{cases}$$

where $\pi(j)$ is the (conditional) probability of other j matching signals:

$$\pi(j) = \left\{ \begin{array}{ll} p^3 + (1-p)^3 & \text{if } j = 2\\ 2[p^2(1-p)] + p(1-p)^2] & \text{if } = 1\\ p(1-p)^2 + p^2(1-p) & \text{if } j = 0 \end{array} \right.,$$

that is, in a more compact form,

$$\pi(j) = (2 - |j-1|)[p^{j+1}(1-p)^{2-j} + p^{2-j}(1-p)^{j+1}].$$

 $\mu(i, j)$ is the (conditional) probability that i other jurors will vote to convict in the straw poll when the number of their matching signals is j:

$$\mu(i,j) =$$

$$\left\{ \begin{array}{ll} [\sigma_0(G)]^{l}[\sigma_0(I)]^{2-j} & \text{if } i=2 \\ j\sigma_0(G)+(2-j)\sigma_0(I)-2[\sigma_0(G)]^{l}[\sigma_0(I)]^{2-j} & \text{if } i=1 \\ (1-\sigma_0(G))^{l}[1-\sigma_0(I)]^{2-j} & \text{if } i=0 \end{array} \right. ,$$

and EU_{FIN} is the weighted combination of the expected utilities from voting either to convict or acquit in the final vote, where weights are given by the strategy for the final vote:

$$EU_{FIN}(s, v_0, i) = \sigma_1(s, v_0, i)EU_{FV}(C_1, s, v_0, i) + [1 - \sigma_1(s, v_0, i)]EU_{FV}(A_1, s, v_0, i).$$

Expected Utility for the Final Vote

In order to make the analysis clearer, consider first the case of a guilty signal (for now, s = G). By the end of the straw poll, all jurors can observe all the votes (so that they know j), but they still do not know what the true state is (either guilty or innocent) and what signals the other jurors received (where the number of other guilty signals can be 0, 1, or 2). This leads to $2 \cdot 3 = 6$ information sets. The conditional probability of being in any of these sets is given by:

$$\hat{\beta}(\iota, k) = \frac{\beta(\iota, k)}{\sum_{l=1}^{6} \beta(\iota, l)},$$

where

$$\beta(i, k) =$$

$$\begin{cases} (2 - |k - 2|)p^{4-k}(1 - p)^{k-1}\mu(i, 3 - k) & \text{if } k = 1, 2, 3 \\ (2 - |k - 2|)p^{k-1}(1 - p)^{4-k}\mu(i, 3 - k) & \text{if } k = 4, 5, 6 \end{cases},$$

For an intuition about these coefficients, notice that we ordered the information sets so that the defendant is guilty in the first three of them (k = 1, 2, 3), innocent otherwise. So $\beta(l, k)$ are the conditional probabilities for the signal being correct (as in this case we are taking s = G) for k = 1, 2, 3; incorrect otherwise.

For the case of s = I, we can define similar coefficients:

$$\mathcal{E}(i, k) = \frac{\varepsilon(i, k)}{\sum_{l=1}^{6} \varepsilon(i, l)},$$

where

$$\varepsilon(i, k) =$$

$$(2 - |k - 2|)p^{4-k}(1 - p)^{k-1}u(i, k - 1) \quad \text{if } k = 1$$

$$\left\{ \begin{array}{ll} (2-|k-2|)p^{4-k}(1-p)^{k-1}\mu(i,\,k-1) & \text{if } k=1,\,2,\,3 \\ (2-|k-2|)p^{k-1}(1-p)^{4-k}\mu(i,\,k-1) & \text{if } k=4,\,5,\,6 \end{array} \right. .$$

Notice that the unanimity rule in the final vote implies that, whenever a juror votes to acquit, independently of what the other jurors voted, the payoffs are determined (0 if the true state is innocent, q-1 otherwise). So:

$$EU_{FV}(A_1, s, v_0, t) = \begin{cases} (q-1) \sum_{k=1}^{3} \beta(i, k) & \text{if } s = G \\ (q-1) \sum_{k=1}^{3} \mathcal{E}(i, k+3) & \text{if } s = I \end{cases}.$$

The formulas for EU_{FV} when the final vote is to convict are more complicated. Consider the case in which the juror receives a guilty signal and votes to convict in the straw poll:

$$EU_{FV}(C_1, G, C_0, i) = (q - 1) \sum_{k=1}^{3} \beta(i, k)[1 - \theta(i, k)]$$

$$-q\sum_{k=1}^{3}\beta(i,k+3)[\theta(i,k)].$$

To understand this formula, notice that the final payoff is 0 only if the true state is guilty and everyone is voting to convict. The payoff is q-1 (as shown in the first addend) if the true state is guilty (so that the sum is over k=1,2,3) but someone votes to acquit (with probability as expressed in $1-\theta$). The second addend represents the other case, when the true state is innocent, but everyone votes to convict. The θ s are defined as follows:

$$\theta(i, k) = \begin{cases} \sigma_1(G, A_0, 1)^{3-k} \sigma_1(I, A_0, 1)^{k-1} & \text{if } i = 0 \\ \frac{1}{2} [\sigma_1(G, C_0, 1)^{1-\alpha} \sigma_1(I, C_0, 1)^{1-\alpha} & \\ \sigma_1(G, A_0, 2)^{1-w} \sigma_1(I, A_0, 2)^{w} + \\ \sigma_1(G, C_0, 1)^{1-w} \sigma_1(I, C_0, 1)^{w} & \\ \sigma_1(G, A_0, 2)^{2} \sigma_1(I, A_0, 2)^{1-2}] & \text{if } i = 1 \\ \sigma_1(G, C_0, 2)^{3-k} \sigma_1(I, C_0, 2)^{k-1} & \text{if } i = 2 \end{cases}$$

where
$$x = 1.5 - |k - 1.5|$$
 and $w = 1.5 - |k - 2.5|$.

The case for a guilty signal and a straw poll vote to acquit $(EU_{FV}(C_1, G, A_0, \iota))$ is similar, except that everyone else observes one vote to convict less than in the previous case. The formulas are as above, but for subtracting an observed

vote to convict in the θ s; for example, $\sigma_1(G, C_0, 1)$ becomes $\sigma_1(G, C_0, 0)$.

The case for an innocent signal is similar, except that it involves the ê coefficients, and the exponents in the formulas are switched. More precisely, consider the case of a straw poll vote to convict.

$$EU_{FV}(C_1, I, C_0, i) = (q - 1) \sum_{k=1}^{3} \mathcal{E}(i, k + 3)[1 - \eta(i, k)]$$
$$- q \sum_{k=1}^{3} \mathcal{E}(i, k)[\eta(i, k)],$$

where

$$\begin{split} \eta(\iota,k) = \\ \begin{cases} \sigma_1(G,A_0,1)^{k-1}\sigma_1(I,A_0,1)^{3-k} & \text{if } i = 0 \\ \frac{1}{2}[\sigma_1(G,C_0,1)^{\bullet}\sigma_1(I,C_0,1)^{1-\nu} & \\ \sigma_1(G,A_0,2)^{1-x}\sigma_1(I,A_0,2)^{\tau} + \\ \sigma_1(G,C_0,1)^{1-x}\sigma_1(I,C_0,1)^{x} & \\ \sigma_1(G,A_0,2)^{\nu}\sigma_1(I,A_0,2)^{1-\nu}] & \text{if } i = 1 \\ \sigma_1(G,C_0,2)^{k-1}\sigma_1(I,C_0,2)^{3-k} & \text{if } i = 2 \end{cases} \end{split}$$

Using the above analysis, we constructed a numerical algorithm to compute the logit QRE correspondence. The logit equilibrium correspondence has multiple solutions, due to the multiplicity of Nash equilibria in the voting game with a straw poll. There is exactly one of these that converges to the fully informative equilibrium identified by Coughlan (2000). That is, for this branch of the equilibrium correspondence, as λ becomes large, voters vote informatively in the straw poll (i.e., vote the color of their signal), and, in the final vote, they vote according to the majority outcome in the straw poll.

The key feature of the QRE correspondence is that, for all positive values of λ , voter strategies in the final vote stage are influenced by their private signal as well as the outcome of the straw poll, in contrast to the full communication equilibrium. Moreover, this conditioning follows a very logical pattern. For any combination of how the other jurors vote in the straw poll, the QRE predicts that a voter is more likely to vote to acquit if they received an innocent signal than if they had received a guilty signal. For example, consider the QRE choice probabilities at our estimated value of λ . If both other jurors voted to acquit in the straw poll, a voter with an innocent signal is predicted to vote to acquit approximately 75% of the time, while a voter with a guilty signal is predicted to vote to acquit slightly less than half the time. Similar comparisons hold for the cases in which one other voter voted to acquit in the straw poll and in which neither other voter voted to acquit. These qualitative comparisons are all borne out in the data.

We then fit the data to the logit QRE solutions using maximum likelihood analysis. Because there are multiple QRE solutions, this required searching over all branches of the QRE correspondence and over all values of lambda. Perhaps not surprisingly, the branch that best fits the data is the one that converges to the full information equilibrium identified by Coughlan (2000). The estimated value of λ is 38.34. We also fit the NNM model to the data in a similar fashion. Table B-1 shows the parameter estimates.

While the QRE picks up the qualitative features of conditional voting in the second stage, it does not provide a very good fit quantitatively. In fact, the NNM estimates provide a somewhat better fit to the data than the QRE, even though NNM does not make the right qualitative predictions.

TABLE B-1:	Model Estimates for Three-Person Groups with Unanimous Rule and Straw Vote							
•	п	f,	QRE	NNM	Nash			
$\sigma(g)$	151	.954	.882	.904	1.000			
$\sigma(I)$	149	.067	.293	.096	0.000			
σ(gC0)	31	.419	.574	.096	0.000			
σ(gC1)	47	1.000	0.995	0.904	1.000			
σ(gC2)	66	.955	1.000	0.904	1.000			
$\sigma(gA0)$	2	1.000	0.501	0.096	0.000			
$\sigma(gA1)$	2	.500	0.895	0.904	1.000			
σ(gA2)	3	0.000	1.000	0.904	1.000			
σ(IC0)	4	.250	0.093	0.096	0.000			
σ(IC1)	3	0.000	0.099	0.096	0.000			
σ(IC2)	3	0.000	0.751	0.904	1.000			
$\sigma(IA0)$	49	.061	0.259	0.096	0.000			
σ(/A1)	68	.074	0.378	0.096	0.000			
σ(IA2)	22	.682	0.978	0.904	1.000			
$\lambda \dot{\gamma}$			38.42	0.807				
$\lambda_{Io} \gamma_{Io}$			37.28	0.756				
$\lambda_{hi} \gamma_{hi}$			40.00	.850				
- <u></u>			279.63	190.62				

The reason the quantitative fit of QRE is not as tight as the NNM fit is not entirely clear. QRE predictions of choice probabilities vary substantially across information sets. For some of these sets, the QRE choice probabilities converge to 0 or 1 very quickly (especially in the second stage); for others they converge very slowly (especially in the first stage). Yet, the data in the first stage have *less* noise than the data in the second stage! Therefore, we cannot get a good fit either for the first stage or for the second stage with a single value of λ .

There is a second possible explanation. In the QRE, the frequency of voting informatively should be much higher with a guilty signal than with an innocent signal. This reflects the fact that voters who receive innocent signals are less likely to be pivotal in the second stage, so their vote in the first stage is less likely to make a difference. Consequently, the QRE choice probabilities conditional on an innocent signal converge to the informative Nash equilibrium more slowly than the QRE choice probabilities conditional on a guilty signal. The data support this qualitative prediction, but the difference is not statistically significant (.067 for innocent signals, .046 for guilty signals).

Based on this post hoc analysis, several fixes may improve the QRE fit, such as estimating a separate value for λ in the first and second stages, for different signals. Since these fixes are ad hoc, we have not pursued them.¹⁷

¹⁷ A more interesting alternative is to estimate a version of QRE that incorporates a second "tremble" term, similar to NNM, but that (unlike NNM) is accounted for in the computation of equilibrium. This would effectively bound the QRE a fixed distance from 0 and 1. We have not pursued this due to issues of computational feasibility.

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Military Capabilities and Escalation: A Correction to Bueno de Mesquita, Morrow, and Zorick

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Bueno de Mesquita, Morrow, and Zorick analyze the evolution of crises as a two-sided incomplete information game in order to illuminate the relationship between observable military capabilities and the escalation of a dispute to armed conflict. I show that an error in the derivation of the equilibria invalidates their conclusions, and I offer a few suggestions on how to model the evolution of crises as incomplete information games.

n "Capabilities, Perception, and Escalation," Bueno de Mesquita, Morrow, and Zorick (1997) model the evolution of disputes between nations as a game of two-sided incomplete information and test empirically their theoretical results. Their main conclusion is that the relationship between the ex ante probability of a crisis escalating to armed conflict and the countries' observable military capabilities is not monotonic; in particular, this probability is initially decreasing and then increasing. In other words, the ex ante probability of an escalation to armed conflict is at a minimum when power, measured by observable military capabilities, is balanced. Although these findings are supported by the data, the analysis of the equilibria contains a mistake that invalidates some of the statements.

This note explains the nature of the mistake and discusses how the conclusions change once the correct equilibria are considered. I find that the relationship between the observed military capabilities and the probability of armed conflict given a crisis is constant and equal to one. Therefore, observable military capabilities have no role in explaining differences in the evolution of crises, and we are left with no explanations for the empirical findings of Bueno de Mesquita, Morrow, and Zorick.

I close with a few comments about the generality of these new conclusions. In particular, I show that they are related to a result known in the economics literature as the no speculation theorem. This sheds a new light on ways to model the evolution of crises as incomplete information games.

THE UNRAVELING OF NEGOTIATIONS

Bueno de Mesquita, Morrow, and Zorick's escalation game can be summarized as follows. Two countries decide sequentially whether to challenge the status quo and resist the challenge, respectively. If a challenge is made and resisted, a simultaneous choice of either negotiation (N) or hostilities (H) takes place. When at least one country chooses hostilities a war ensues; its outcome depends on two elements: the observable

military capabilities (K) and the unobservable military abilities $(p_i \text{ and } p_j, \text{ respectively, for the two countries})$. Nation i prevails if $p_i + K \ge p_j$.

From the analysis of the equilibria of this game,

From the analysis of the equilibria of this game, Bueno de Mesquita, Morrow, and Zorick draw two main conclusions. First, the relationship between the observable military capabilities K and the probability of war given a crisis is nonmonotonic; in particular, the chance of war decreases and then increases (hypothesis 3). Second, the ex ante probability of a negotiated settlement first rises with K and then falls with K (hypothesis 4). These findings are at odds with the literature but are supported by Bueno de Mesquita, Morrow, and Zorick's data.

The claim relies on the existence of equilibrium 3 described in their article. In this equilibrium, nations whose unobservable military capabilities fall between the thresholds *pil* and *pih* or between *pil* and *pih* do not want to fight; nevertheless, they respectively make and resist demands in the hope of gaining a negotiated settlement. The proof of the existence of equilibrium 3, however, contains a mistake.

On page 26, the authors state that initiating hostilities (H) is a best reply for i if:

$$\frac{p_{i} + K - \beta}{1 - \beta} - c \ge \left(\frac{1 - \gamma}{1 - \beta}\right) \left(\frac{p_{i} + K - \gamma}{1 - \gamma} - c\right) + \left(\frac{\gamma - \beta}{1 - \beta}\right) U_{i}(N)$$

and that this is equivalent to $p_i \ge \theta$. This is incorrect since, by simplifying the inequality, p_i drops out. A few lines below, the same mistake is repeated in showing that h is j's best reply for $p_i \ge \gamma$.

Since the main conclusions of the article rely on the existence of an equilibrium in which disputes sometimes end in negotiations, it is interesting to verify whether such an equilibrium can be found. Unfortunately, this is not the case.²

Proposition 1. In no perfect Bayesian equilibrium does a dispute end in negotiations.

The formal argument is presented in the Appendix, but an informal intuition of the result is as follows. In

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I wish to thank an anonymous referee for pointing out the relevance of the no speculation theorems.

¹ See the original article for a precise description of the game and its

² As pointed out by a referee, negotiation is not an equilibrium outcome in a much larger class of games than that in Bueno de Mesquita, Morrow, and Zonck (1997). See the Appendix.

choosing whether to negotiate, country j is not interested in drawing inferences about country i's unobservable capabilities when the latter is prone to initiate hostilities. This is so because violence occurs when either country chooses it; therefore, country j is going to have a fight with the country that initiates hostilities, regardless of what it chooses.

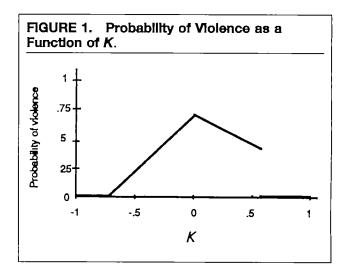
What really matters in choosing a battle over acquiescence is the unobservable military strength of a country that is prone to negotiate. If this is expected to be low, a fight is preferred, because the probability of winning it is high, and the favorable outcome of an armed conflict is preferred to negotiation. If, instead, this is expected to be high, negotiation is a better option.

Suppose that, in a crisis, j expects only countries above a threshold pih < 1 to initiate hostilities. This implies that j expects those willing to negotiate to be weaker than the average country. Therefore, even when country j is somewhat weak, it may still be willing to use force; but this implies that the threshold pjh is low and that the average strength of j (when it is willing to negotiate) is low. This inference induces an even weaker country i to use force, which further reduces the threshold pih, and so on. The equilibrium unravels, and a negotiated settlement is impossible. In other words, the willingness to negotiate is a sign of weakness that spurs more violence.

Proposition 1 implies that hypotheses 3 and 4 of Bueno de Mesquita, Morrow, and Zorick are no longer correct. Once amended, they say that the probability of an armed conflict, conditional on having a crisis, is constant and equal to one and that the ex ante probability of a negotiated settlement is constant and equal to zero. Therefore, differences in observable capabilities are of no use in explaining the evolution of crises in this model, and we are left with no explanation for the empirical evidence illustrated by Bueno de Mesquita, Morrow, and Zorick.

Note, however, that a country's observable military strength K is not trivially related to the ex ante probability of violence. For example, consider the case $U_{\iota}(SQ) = 0, \ U_{\iota}(D) = 1, \ U_{\iota}(SQ) = 1, \ U_{\iota}(D) = 0,$ and $U_{i}(N) = U_{i}(N) = 1/2$, which, for c < 1/2, satisfies all the assumptions on pages 17 and 18. This game has a very intuitive interpretation in terms of a dispute about the sharing of power. The value of power is normalized to 1; the status quo (SQ) corresponds to country i being in power; granting the concession (D)corresponds to letting country j seize power; and negotiations lead to an equal share of power for the two countries. In Figure 1 I plot the ex ante equilibrium probability of violence occurring as a function of K when c = 0.45. As shown, for extreme values of K (equilibrium 1 and 4) the probability of violence is zero, whereas for intermediate values of K the probability increases to a maximum of 0.7 (equilibrium 2) and then declines to a minimum of 0.4 (equilibrium 4).

The relationship depicted in Figure $\hat{1}$ can be explained by interpreting K as a measure of the balance in the observable military capabilities of the two countries. When this balance is tilted in favor of either state



(extreme values of K), the state that is known to be weaker backs out of the fight in order to save the cost of the conflict. When there is no known advantage for either state (intermediate values of K), the uncertainty about the military strength of one's foe makes an aggressive action potentially beneficial, and war is more likely.

These amended conclusions are consistent with the preponderance of power school³ and with the formal results of Smith (1998), which models crises and warfare as complete information games.

CONCLUSION

As we know, and as the data set used by Bueno de Mesquita, Morrow, and Zorick shows, disputes sometimes end in negotiations. Thus, we should ask what is missing in the assumptions of the game. An answer not only would provide a new starting point to explain the empirical findings of Bueno de Mesquita, Morrow, and Zorick but also would illuminate important strategic aspects of the evolution of crises.

The model of perception and capabilities proposed by the authors has much in common with a family of economic models regarding the existence of speculation due to asymmetries of information. (See Geneakoplos 1994 for a survey.) According to one version of the model, two individuals are given one card each from a shuffled deck. After seeing their own card, they simultaneously decide whether to deposit a fee in a pot or to walk away. If they both deposit the fee, the player with the highest card wins the pot, and the other gets nothing. Otherwise, they both get nothing. Choosing to deposit the fee is equivalent to a bet on the value of one's card being higher than the opponent's.

The no bet theorem for this game says that common knowledge of rationality is enough to guarantee that in no Bayesian equilibrium of this game will the bet be taken. In fact, each player knows that the bet will occur only when the other accepts it, but the opponent is more likely to bet when he has a higher card. Because

³ See Blainey 1973 and Wagner 1994 for the dispute between this school and the balance-of-power school.

each knows that his bet will be taken when he is more likely to lose it, he would rather not bet. Notice that the equilibrium outcome is the same as it would be with complete information on the values of the cards (and common prior).

Consider now the perception game. Initiating hostilities can be interpreted as a bet that one's unobservable military capability is greater than that of the opponent. The game is similar to the card game, but in the perception game only one player need take the bet; in other words, each player can force the bet on her opponent. This difference leads to a bet theorem: In every Bayesian equilibrium of the subgame, war occurs because at least one player is willing to engage in hostilities (to bet). As in the card game, the only equilibrium outcome is the one that would occur if the military capabilities of the two players were known (and the countries shared the same prior).

This shows that the perception of the opponent's strength plays no role as long as the conflict is perceived (and therefore modeled) as a bet that can be unilaterally implemented. But is this modeling choice appropriate?

For at least two reasons it might not be. The first concerns the choice of modeling the decision on how to handle a crisis as a bet. In a bet, once each player has shown his card to the other, the result is deterministic, and there is no role for unexpected events or noise to determine the outcome. This assumption is probably inappropriate for wars, whose final outcome often may depend not only on military power, both observable and intangible, of the countries involved but also on random and unexpected events.

The second reason concerns the assumption that crises can be seen as the choice about whether to bet. What is missing in this assumption is that in a dispute the utility of the "no bet" outcome, that is, what one would get by negotiating, is generally not independent of what the two countries have learned about their respective intangible capabilities during the crises. In other words, negotiating offers should be modeled as endogenous (as, for example, in Fearon 1995 and Powell 1996).

APPENDIX

The proof is given for a more general game than that of Bueno de Mesquita, Morrow, and Zorick. In this game, the unobservable military capabilities p, and p, are real numbers with any strictly positive probability distribution.

*Proof.*⁵ Suppose there is a perfect Bayesian equilibrium in which negotiations occur with strictly positive probability, and consider the subgame starting after a demand is made and resisted. Let $F(\cdot)$ and $G(\cdot)$ be the cumulative distribution function representing, respectively, the updated distribution of p_i and p_j in the subgame. Using an incentive compatibility argument, we know there are threshold values pih and pjh such that all types $p_i < pih$ and $p_j < pjh$ who reached the subgame choose to negotiate. Suppose F(pih) and G(pjh) are strictly positive.

Consider type p_i . Its expected utility for hostilities is

$$Pr(p_{i} \leq p_{i} + K) - c = G(p_{i} + K) - c,$$

and its expected utility for negotiating is

$$\begin{split} &G(phy)U_{i}(N) + (1-G(phy))[Pr(p_{j} < p_{i} + K | p_{j} > phy) - c] \\ &= G(phy)U_{i}(N) + (1-G(phy))\bigg[\frac{G(p_{i} + K) - G(phy)}{1-G(phy)} - c\bigg]. \end{split}$$

Thus, type p_i prefers to fight if

$$G(p_i + K) - c > G(phj)U_i(N) + G(p_i + K) - G(phj)$$
$$- (1 - G(phj))c,$$

which simplifies to

$$1-c>U_{\iota}(N).$$

This inequality is true by assumption; therefore, all types p_i prefer to fight, and $F(p_i) = 0$.

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⁴ Notice that, in contrast to the no speculation theorems, violence occurs even if negotiations are ex ante Pareto optimal, that is, when $1/2 - c < U_i(N)$, i = 1,2. This is so because in the no speculation theorems the ex ante Pareto optimality of the initial allocation is required to rule out trade for reasons different from pure speculation; therefore, it plays no role in the speculation result I get.

⁵ I thank a referee for suggesting this line of proof, which is cleaner than the one originally proposed.

Reply to "Military Capabilities and Escalation: A Correction to Bueno de Mesquita, Morrow, and Zorick"

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e thank Professor Molinari for bringing the error in our article (Bueno de Mesquita, Morrow, and Zorick 1997) to the attention of the readers of the *Review*. Despite the inadequacies of the model we presented, we continue our efforts to explain our empirical results. This brief reply sketches lines of argument that we are pursuing in our research.

A state in a crisis judges whether it is willing to accept a negotiated settlement by comparing the settlement to its prospects in a war. Conventionally, the latter is modeled as a known and fixed expected value for a state based on the importance of the stakes, its willingness to suffer costs, and its relative military capabilities (e.g., Fearon 1994; Powell 1996). That expected value is often treated as private information, where the sides do not know one another's value for war but do know their own. War, however, is a contest in which those values for both sides affect the outcome. Consequently, we cannot assume that either side in a crisis knows its expected value for war because the outcome of the war depends on the private information of both sides (Wagner 1998).

In the game analyzed in our article, we attempted to address this issue by treating the decision whether to use violence as simultaneous and by examining the deterministic relationship between private information and war outcomes. We address changes in both these features.

War results when both sides choose to use violence to resolve their dispute. As our data analysis shows (see Bueno de Mesquita, Morrow, and Zorick 1997, Figure 4), unilateral uses of violence occur frequently in crises. One side chooses to use force; the other side must then decide whether to use violent means to resist that force. Our model did not differentiate reciprocated violence from unilateral violence, although our data analysis did. An alternative sequence gives each side a chance to escalate to violence with a subsequent decision by the target to respond with violence. If the target does not respond with force, then the initiator of violence wins the stakes. This sequence differs from some notable models of crises where one party alone can initiate war (e.g., Fearon 1994). It also changes the

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decision involved in escalating to violence because the sequence creates the possibility of gaining the stakes without suffering the costs of war (Bueno de Mesquita and Lalman 1992).

We modeled the outcome of war as deterministic in the sides' private information. Molinari comments that random elements are likely to play a role in war, and we agree. Yet, random factors whose probabilities are common knowledge simply become part of players' expected values for war. Furthermore, such expected values are likely to be less extreme than deterministic outcomes based on private information, making the weaker side more willing to use violence in a crisis. Adding random events then would not address the question of why all actors wish to use violence in any equilibrium of the model. Instead, we suspect that the expectation from war must be more extreme than surrendering the stakes of the crisis by refusing to resist violence. This would result when the victor in a war expands its demands beyond the original stakes of a crisis.

We are exploring these variations and others in our attempt to explain our empirical results with a formal model. Any such revised model must not only explain the patterns we have found so far in the data but also must predict some pattern that we have not yet observed, thus predicting novel events as well as known regularities.

Finally, Molinari also suggests that a model of crisis bargaining should allow the negotiated alternative to war to be endogenous. We agree and look forward to Professor Molinari's efforts in this direction.

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Queer Liberalism?

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Gay and Lesbian Politics: Sexuality and the Emergence of a New Ethic. By Mark Blasius. Philadelphia: Temple University Press, 1994. 239p. \$69.95 cloth, \$22.95 paper.

Sexual Justice: Democratic Citizenship and the Politics of Desire. By Morris Kaplan. New York: Routledge, 1997. 281p. \$65.00 cloth, \$19.99 paper.

Queer Family Values: Debunking the Myth of the Nuclear Family. By Valerie Lehr. Philadelphia: Temple University Press, 1999. 212p. \$59.50 cloth, \$19.95 paper.

Difference Troubles: Queering Social Theory and Sexual Politics. By Steven Seidman. Cambridge: Cambridge University Press, 1997. 307p. \$59.95 cloth, \$23.95 paper.

Virtually Normal: An Argument about Homosexuality. By Andrew Sullivan. New York: Knopf, 1995. 209p. \$22,00 cloth, \$12.00 paper.

Invented Moralities: Sexual Values in an Age of Uncertainty. By Jeffrey Weeks. New York: Columbia University Press, 1995. 209p. \$28.00 cloth, \$17.50 paper.

ncreasingly, demands for liberty and equality have been transmuted into, or combined with, calls for recognition of previously excluded or marginalized groups. This shift is due in part to the fact that simple group separation or autonomy is rarely feasible in postmodern societies. Instead, subordinated groups must find a path that combines integration as equals with retention of and pride in their distinctive lives. Contemporary discussions of recognition have ranged from the most general to those with a specific focus on racial, ethnic, and national minorities or gender or a combination of the above (see, e.g., Benhabib 1995; Honneth 1996; Taylor 1992). Virtually none of these works has approached the question of sexual minorities in heterosexual regimes. The more politically sensitive among them list gays and lesbians in a line of subjugated or excluded peoples, but none has questioned the distinct contours of gay and lesbian oppression and

Although some of the feminist work in political theory mentions lesbians and gays and draws connections between sexism and heterosexism, by and large feminist theory has failed to adequately understand the position of sexual minorities. Feminist analyses understandably center on gender and generally treat sexual oppression (if at all) as a simple consequence of gender oppression. Same-sex couples serve Susan Okin (1989, 140), for example, to demonstrate the link between gender hierarchy and family roles, but they receive no discussion on their own terms; clearly for Okin, as for most feminist theorists, they are the "foreigners" who demonstrate what might be rather than subjects in their own right. Okin is not concerned to investigate the particulars of same-sex couples or to understand how

gender operates within these couples to produce patterns of privilege quite different from those between heterosexual men and women (although, in her defense, most of the empirical work on these patterns was done after her book was published). Instead, her glancing mention of same-sex couples serves as a rebuke to heterosexual inequality. Iris Marion Young's (1990) more nuanced treatment seems to account for the specificities of lesbian and gay existence as distinct from gender and, indeed, highlights homophobia as "the paradigm of ... border anxiety" (p. 146), more abiding and visceral than racism and sexism. Still, Young's aim of a general theoretical argument makes lesbians and gays just one example in an account that largely takes sexism, racism, heterosexism, ageism, and ableism as distinct but equivalent problems.

These projects have been important contributions to political theory. Still, there is a need for theories that begin from the issues and perspectives generated by differences in sexuality. We need to see how the world looks if we begin from the position of lesbians and gays rather than from that of heterosexuals. This includes revisiting theories that have proved both useful and problematic. The putatively neutral subject of liberalism has, as many critics have shown, turned out to be a member of dominant groups whose notions of reason work to silence others rather than include them. How might theories change if that subject is, and knows itself to be, a sexual minority? The authors under review here offer different and illuminating answers.

This essay evaluates the ways in which thinking about lesbian and gay lives can illuminate and improve liberalism and diagnoses the strange persistence of this flawed cluster of theories. The liberal/communitarian debates of the 1980s, and feminist political theory in that same period, demonstrated that liberalism does not require an abstract self and that rights can be construed in a relational framework and within a genealogy that includes the rise of disciplinary apparatuses. These debates have made their way into contemporary lesbian and gay political theory (Phelan 1989, 1997) but not simply in a derivative manner. Instead, recent work demonstrates that using the lens of (homo)sexuality illuminates hidden or muted elements in liberalism. A group that is targeted for discipline, whose rights have been continually unsure, and whose relationships (erotic and otherwise) have had to be fought for must necessarily rethink what sort of self, and what sort of rights, is worth fighting for. Although

¹ Angelia R. Wilson (1994, 36) notes that "gay and lesbian political theory" amounts to a different project from earlier inquiries into homosexuality precisely because "one is about fitting the example of homosexuality, as a moral act, into a known philosophical or moral scheme and the other is about exploring how different philosophical frameworks can facilitate a gay and lesbian political movement which takes as a premise that sexuality is part of individual identity."

the books under review here present many arguments and themes, together they produce a series of reflections on whether liberalism is an adequate or useful political theory for lesbians and gays and on what sort of liberalism might quality for that status.

The most obvious and simple-minded liberalism among advocates for lesbian and gay rights is found in *Virtually Normal*. Andrew Sullivan contends that a sound "politics of homosexuality" consists in the "principle" that "all *public* (as opposed to private) discrimination against homosexuals be ended and that every right and responsibility that heterosexuals enjoy as public citizens be extended to those who grow up and find themselves emotionally different" (p. 171). This means

an end to sodomy laws that apply only to homosexuals; a recourse to the courts if there is not equal protection of heterosexuals and homosexuals in law enforcement; an equal legal age of consent to sexual activity for heterosexuals and homosexuals, where such regulations apply; inclusion of the facts about homosexuality in the curriculum of every government-funded school, in terms no more and no less clear than those applied to heterosexuality (although almost certainly with far less emphasis, because of homosexuality's relative rareness when compared with heterosexuality's recourse to the courts if any governmental body or agency can be proven to be engaged in discrimination against homosexual employees; equal opportunity and inclusion in the military; and legal homosexual marriage and divorce (pp. 171–2).

Sullivan maintains a liberalism that focuses only on the relation between the state and individuals and rejects any state responsibility for the social status or welfare of citizens. His is the most rigorous (one might say rigid) sort of "negative liberty." Ending sodomy laws is for him a classic case of the expansion of freedom from the state rather than freedom via the state. Allowing same-sex marriage and the equal participation of homosexuals in the military is not, for Sullivan, a matter of state support (although I doubt that Orrin Hatch will agree) but of removing state barriers to participation. The latter involves changing the military culture, no small task indeed, but legally it is less a matter of direct intervention than of ceasing active discrimination. Marriage simply involves removing statutes (currently multiplying like rabbits) that ban such marriages. Sullivan rejects other projects, such as employment nondiscrimination, on the grounds that they interfere with private choices, choices based on personal morality that he argues cannot be mandated from above.

Sullivan rests his argument on the distinction between and separation of civil society and the state. He argues that true social change is the province of civil society. In this most writers of whatever stripe would agree. What is occluded, however, is the question of the state's role in civil society. Sullivan treats individuals either as presocially constituted (in the case of sexual desire) or as shaped in realms of family and civil society that are presumed to be separate from, even opposed to, the state. Civil society, in its turn, appears as simply "there," akin to the abstract individual in its lack of intrinsic relation to government apparatus. The

role of the state in fostering public realms, and the reciprocal constitution of the state in civil society, is obfuscated by the liberal separation. This obfuscation prevents us from asking how the state is constituted as a heterosexual body and how heterosexual imperatives constitute citizens. These are precisely the questions that must be asked if we are to challenge heteronormativity. A new liberalism that will speak to the situation of gays and lesbians (as well as others) cannot begin simply from normative theoretical principles; rather, it must unite these with attention to the construction of states and communities in order to find the particular articulations of those principles that will work for those states and communities—not simply reinforce their existing ways of life but enable them to adapt and change.

The problems of orthodox liberalism led gays and lesbians, along with other new social movements, to explore other theoretical resources. Gay liberation theory grew out of Marxism, in particular Marcuse's treatment of sexuality in Eros and Civilization (1955), and focused on the relation between sexuality and capitalism. Dennis Altman ([1971] 1993), Mario Mieli (1977), and Guy Hocquenghem (1978) each offered analyses suggesting that without the guilt and renunciation demanded by capitalist discipline we would all be polymorphously perverse, free to experience pleasure with a variety of different partners. This "liberationist" theorizing is now virtually unknown and/or discredited even by students who see themselves as radical (Lehring 1997). In academic circles Marxism was pushed aside not by liberalism, however, but by poststructuralism. This shift marked the decline of utopian or universalist theories that aimed at the end of repression in favor of theories that sought to account for the particular constructions of self and society that include not only repression but also forces of desire, meaning. and agency—that is, theories that understand the heterosexual self not simply as one forced to abandon its homosexual desires upon pain of expulsion but as a self created and given meaning precisely by the lure of belonging to the "normal."

In the formation of lesbian and gay theory, the version of poststructuralism with the most influence was that of Michel Foucault. Foucault's documentation of the move from punishment to discipline, and the supplementation of sovereignty with normalizing power, was centered around the creation and use of "sexuality" as a mechanism for discipline. Sexuality, Foucault pointed out, was the perfect node for this new constellation of power. Through it, policymakers could combine the macro goal of population management, what he calls "a bio-politics of the population," with the need for control and regulation of individual bodies for capitalist production and consumption—"an anatomo-politics of the human body" (Foucault 1978, 139).

Central to this new regime of biopower was the idea of sexuality as something to be discovered, regulated, and managed. This is evident in everything from the new field of sexology, to eugenics campaigns, to school programs for sex education and hygiene. Sexuality

became something that defined the self, one's innermost truth. Those whose sexuality was found to be different became populations to be studied and cured. Thus, in Foucault's (1978, 43) famous phrase, "the sodomite had been a temporary aberration: the homosexual was now a species." (p. 43). Repression is not abandoned as a problem, but it is elaborated within a larger network of productive power that creates subjects for whom repression appears as an issue: The struggle against repression, central to liberationist theory, is presented by Foucault as "a tactical shift and reversal in the great deployment of sexuality" (p. 131). (Sullivan, who conflates Marcusean liberationism with contemporary "queer theory," completely misses the importance of this point.) Foucault championed the need to make sexuality a project, part of one's subject formation, not in a disciplinary sense but as an ascetic exercise. The disciplinary apparatus, he argued, should be challenged not by simple opposition in the name of some preexisting desire or nature but by the creation of new relationships and forms of meaning. His insistence on the historical nature of sexual identities, which is shared with Jeffrey Weeks (1981, 1985) and later many others, provided an alternative to understandings of sexuality as simply "natural" (or not), immutable on a personal level, with its own transhistorical categories. In the light of Foucault's work, we are cautioned against claims that lesbians and gays form a natural unit or share any sort of continuous history. Gay and lesbian theory, and sexuality studies in general, has been a primary site for thinking about social construction precisely because sexuality is one of the clearest examples of the relation between the construction of subjects, economics, and public policies (Stein 1990). It is also a preeminent site for challenging neat lines between civil society and the state.

Through a constructionist lens, sexual types are "real" but not timeless categories. As Foucault and others have agreed, the biology of sex may or may not show links between bodily differences and differences in "sexual orientation." Constructionists do not (or do not all) deny the "reality" of biology, but they suggest that biology is not the principal site for the organization of sexuality. As Weeks puts it in Invented Moralities (p. 7), "the real problem does not lie in whether homosexuality is inborn or learnt. It lies instead in the question: what are the meanings that this particular culture gives to homosexual behavior, however it may be caused, and what are the effects of those meanings on the ways in which individuals organize their sexual lives." A social constructionist perspective simply makes clear that sexual categories are a human creation, varying across cultures and times, and that the variety of categories gives rise to substantively different lived experiences.

It does not follow from constructionism that a given category or boundary is right or wrong. Rather, social constructionism forces us to abandon arguments founded on God or nature in favor of substantive moral arguments about the sort of people we want to be. As Stanley Fish (1999) has recently noted, nothing follows from social constructionism in terms of partic-

ular moral stances; rather, what changes is the ground of legitimation for one's arguments. As an hypothesis about history, social constructionism mandates attention to the historical formation of categories that we take to be timeless. It is precisely this thesis of historical formation that alarms both those who would bar sexual change and those who would claim rights in the name of their inviolate identities. Sexuality is such a complex and fruitful arena for political theory because it is a major terrain for battles over the relation between identity and behavior, the social construction of political identities (and the political construction of social identifies), the role of the regulation of sexuality in modern governance, and the nature and extent of freedom, power, and justice.

Activists for lesbian and gay equality have by and large sought to bypass these issues by focusing on sexuality as a nexus for identity, a point with which our authors would have few objections. Activists part company with many academics, however, by presenting sexuality as immutable, a matter of ascription rather than choice, so as to bypass moral debates (Currah 1995). This strategy has been at least a partial success, as heterosexual Americans have become more tolerant of those "others" who "cannot help it." It is for this reason that activists have been largely suspicious of constructionist theory. Fearing that construction is too much like arguments centered on behavior or choice (a misconstrual not uncommon even among academic readers of constructionist theory), they charge theorists with hurting "the cause." The reality of the matter is more complex. Constructionism does not eliminate claims to equality, membership, and rights but changes the ground for these claims from the atomistic individual. "born" with a nonnegotiable sexuality (of whatever orientation) that must be respected, to a subject formed through processes of meaning and power that must be critically examined. Our authors disagree on the question of whether, or to what extent, a constructionist ontology necessarily challenges liberalism. They also, as we will see, differ in their careful attention to the implications of constructionism for their consideration of group formation, maintenance, and position.

The relative invisibility and mutability of sexual minorities produce an experience unlike that of most other groups other than (most) religious minorities. As opponents of inclusion are fond of pointing out, we are only attacked or fired or arrested if we make our homosexuality apparent. Of course, current conceptions of gender and sexuality mean that some will always be virtually unable to pass; very butch women or femme men are assumed to be queer and are treated as such. The argument for mutability tends to be a weapon of opponents as well, suggesting that we have a choice and therefore should bear the onus if we choose "sin" or "deviance." (This, in fact, was my mother's response when I reported to her a series of rapes of lesbians while I was in graduate school.) In response, activists have largely insisted that sexuality is immutable, a matter of genes or early conditioning rather than conscious choice.

As Vera Whisman (1997) has shown, however, the

debate about immutability is far from settled; many lesbians, and fewer gay men, understand themselves as "queer by choice." In both visibility and mutability, lesbians and gays are closer to religious group members than to racial or ethnic minorities. Like those whose religion is important to them, their specificity is sometimes easily apparent (e.g., dress codes for Orthodox Jews or Sikhs) and more often not. Like them, too, religion is something that is mutable but also quite stable over time; we acknowledge that someone born "one way" may turn to another in life, but we do not consider the later religion to be less authentic or important for that person's identity as a consequence of being chosen. The irony of this kinship has been noted by several authors (Mohr 1988; Phelan 1994; Richards 1999), but it has yet to make its way into the mainstream of public discussion or legal interpretation.

If the questions of visibility and mutability shape a distinctive experience, perhaps greater is the fact of growing up in communities that do not mirror our distinctive difference. This presents a problem both for individuals and for communities, but it also represents a unique opportunity for what Mark Blasius calls "ethical self-constitution." On his way to characterizing this self-constitution, Blasius offers in Gay and Lesbian Politics the most faithfully "Foucauldian" analysis of the authors under consideration here (indeed, readers of Foucault will find little that is new in the early chapters). Successive chapters on the possibility of gay and lesbian politics, the politics of sexuality, subjectivity and identity, rights, and "an ethos of lesbian and gay existence" follow Foucault's later work on power and subjectivity while developing a specific argument for gays and lesbians.

For Blasius, "being lesbian or gay today is by definition political" (p. 1) insofar as one comes out, that is, figures oneself as lesbian or gay through one's relationships with others. Of course, not all those who have sex with others of the same sex, or all of those who want to, are lesbian and gay; many who do so refuse to acknowledge either the desire, the activity, or the identity. Blasius asserts that those who do not come out "exist in the prepolitical condition of domination" (p. 2) (a situation in which "the actions of one...elicit and guide or command the actions of another with a high degree of certainty" (p. 21). Those who do come out become lesbian or gay, persons formed through "subjection," a process of discipline that forms identities and guides actions without the lack of reciprocity characteristic of domination.

This is still, however, short of agency; Blasius states that agency for lesbians and gays follows from "erotics," the creation of new forms and forums of sexuality to challenge subjectification. His notion of an "ethos" describes a "way of life" that "emerges not so much from moral as from existential criteria" (p. 209), specifically the content of lived experience. In this ethos, lesbians and gays "invent themselves, recognize each other, and establish a relationship to the culture in which they live" (p. 183). He contrasts an ethos with a lifestyle or a community (while acknowledging elements of both in lesbian and gay life): lifestyle is too

narrowly consumerist a notion, while community presumes too much unity and preexisting definition. An ethos, in his terms, allows for self-creation in all seriousness, allowing for both ethical and aesthetic elements without collapsing into either abstract normativism or moral relativism.

Although Blasius devotes considerable time to the distinction between the agency of erotics and the faults of moral relativism, in the end it is less clear what sorts of limits he might accept than what he rejects. He rejects any "codes of behavior" such as Kirk and Madsen's (1989) infamous "Self-Policing Social Code," which urges restraint in front of heterosexuals and incorporation of middle-class white standards of comportment (including rules such as honesty and a ban on negative gossip), but he gives no real clues as to what values might guide us. Blasius cites Sarah Lucia Hoagland's Lesbian Ethics (1988) for support on ideas about community and ethos, but he does not engage her more serious treatments of ethical questions. In the end, it seems, the only thing that clearly does not go for Blasius is moral codes.

Central to the creation of self and ethos, states Blasius, is the elaboration and defense of "lesbian and gay rights," aimed at "self-determination of one's relationships with others" (p. 137). This idea, as we shall see, runs throughout lesbian and gay political theory. This should not be surprising; a group stigmatized on the basis of its relationships might be expected to defend them. What may be more surprising is the richness opened by this thread. The development of relational arguments for rights may be the most significant advance offered by our authors, dovetailing with recent feminist legal and political theory on privacy and "the personal sphere" (e.g., Boling 1996) as well as anthropological accounts of alternative family forms (e.g., Lewin 1993, 1998; Weston 1991).

Blasius's formulation of the "relational right" construes it as the right of "self-determination and actualization" through relationships with others (p. 140). This right encompasses what he describes as the central "moments" in lesbian and gay rights struggles, rights for sexual freedom, equality, and "equity in the cultural and social acknowledgment of one's health needs and the consequent receipt of the benefits of citizenship" (p. 152). Blasius's primary target in this formulation is the primacy of privacy strategies in arguments for rights, a primacy that had already sharply receded after the Supreme Court upheld antisodomy statutes in Bowers v. Hardwick (1986). For Blasius, "privacy" seems to be the name for liberal strategies that do not account for the "normalizing-disciplinary ordering of power" (p. 133) which makes "juridico-discursive" models inadequate. The central insight that Blasius draws from Foucault is the need for rights to account for and improve the ways in which we live our daily lives, in relationship with others, rather than as abstract individuals carrying our rights bundles with us as we jostle with others to reach our goals. Because gays and lesbians are not vilified for some internal essence (although that has been introduced into arguments) but for the ways we interact with others, rights that

presume and defend our relationships with others are essential to equal citizenship. (Of course, this is the case for all people, as Weeks will make clear; a queer angle may give a certain salience to this issue, but it is not an issue for queers alone. Indeed, such an insight is at the heart of all communitarian thought and most feminism as well as poststructuralism.)

Blasius's claim to an equity right is elaborated through the example of AIDS politics, but it extends beyond that to a general claim for "a right of access to protection from any biological risks derived from sexual relations" (p. 151). Blasius derives this right from the larger right to sexual self-determination, arguing that in an era of biopower and population management the government has an affirmative responsibility to ensure the health of its citizens. This point clearly reflects his anger over the U.S. government's (non) handling of AIDS for the first several years when it was taking hold here and its continuing stigmatization of nonheterosexual people with AIDS. There is still enough in his argument, however, to give the reader pause. Do governments, even ones heavily invested in biopower and strategies of governmentality, have a responsibility to protect citizens from all health consequences of their behavior? If this holds only for sexual behavior but not for other forms of self-creation and expression (and it is not clear that Blasius would so limit it), what would justify such privileging? Blasius notes that sexual relationships are a central means through which we (gays and lesbians, at least) define ourselves. Do governments then have an obligation to support all the central ways in which we define ourselves? This argument lurks in the text, but it is not sufficiently spelled out to be defended against those who might object. What of those whose means of definition is a religion that disapproves of homosexuality? Should they be allowed to discriminate against people whose sexual tastes they abhor? If not, why exactly should sexuality be privileged over other forms of identity formation and maintenance?

The reason is likely to be more personal than philosophical. Many lesbians and gays of color have criticized white male politics for treating sexuality as a variable that can be understood in isolation from race, gender, and class. For otherwise privileged men, their homosexuality stands out as the barrier to equality and integration. Other differences, while acknowledged, are less likely to be integrated into analysis or action. Given Blasius's stated concern for "lived experience," it is striking how little the author acknowledges the differences between the experiences of lesbians and those of gays, or those of varying racial or class backgrounds. Throughout the 1970s and 1980s (and to a large extent in earlier and later periods), lesbians remained separate from gay organizations and communities. Although heterosexuals and gay men often interpret lesbianism solely through the lens of "homosexuality," lesbian theory and politics throughout the twentieth century have sought to incorporate the lens of homosexuality with the experience of female assign-

Blasius denies this distinctive perspective, and in so

doing he crucially misreads the few lesbian sources he cites. Lesbians appear in Blasius's work with their feminist commitments and analyses elided. In her classic essay, "Compulsory Heterosexuality and Lesbian Existence," Adrienne Rich (1980) analyzed "compulsory heterosexuality" for women as a primary instrument of women's subordination and argued for a desexualized lesbianism ranging from Chinese marriage resisters, to nuns, to "Boston marriages," to contemporary lesbians under the sign of female-identification and resistance to patriarchy, but it emerges here (as in much recent gay writing) as a critique of enforced heterosexuality for both men and women. Likewise, Hoagland's Lesbian Ethics, on which Blasius relies for descriptions of culture and community, loses the lesbian separatism at its heart in favor of claims for "a common lesbian and gay male culture" (p. 201). Of course, authors are not required to read and use texts only as their authors understood them; but given Blasius's concern for concrete historical experience, his abstraction and flattening of historical and contemporary differences sharply limits his claims. As Lehr points out in Queer Family Values (citing L. Brown 1995) lesbians and gays not only experience different social positions and challenges but also do not even agree on conceptions of sexual identity and its stability. An "ethos of lesbian and gay existence" that does not include acknowledgement of these differences will be of limited use.

For all his language of community, culture, and ethos, Blasius seems to see these as detached from issues of state or polity. Quite strikingly, his argument for equity bypasses traditional social-democratic or welfare-state principles that would mandate accessible and knowledgeable health care for all citizens, no matter what the genesis of their malady, on grounds of the common welfare or minimal standards of civility and decency. Instead, he proceeds down a route much more in keeping with individualism. Not our common concerns, but our need and right to form intimate relationships, justify equity rights. The relationships that he deems ontologically basic to humans do not extend beyond the intimate sphere, or the zone of communities based on a shared ethos, to include the much more distant and yet inescapable relations of modern bureaucracy and urbanization. The government, even after biopower, appears in strikingly liberal guise as the guarantor of basic services rather than a creative power, although Blasius agrees with Foucault's diagnosis of the risk of governmentality, his concern for private creation in resistance to medical authority leads him to conceive of government in strikingly liberal

Blasius's focus on an ethos reflects another peculiarity of lesbian and gay existence. Unlike most other marginalized groups, as Lehr notes in her work (p. 139), lesbians and gays grow up in communities that actively stigmatize them even before they know that they "belong" to the group. As a result, both the families and the communities shaped among lesbians and gays tend to consist only of adults. One of the strongest contributions in *Queer Family Values* is the

discussion of the question: "Who are our children?" Lehr argues that the current focus on creating families, either through adoption or with the children of earlier marriages, must be combined with the creation of service agencies and programs that provide models of homosexual maturity. This is part of a larger project of a "politics of family" that is organized around care rather than rights (although, as I will note later, rights are not so easily disposed of). Successive chapters move between treatments of feminist and radical democratic theory and analysis of current trends in "family politics" that are too often treated in isolation from lesbian and gay issues.

The chapter on social problems and family ideology is a must read not only for those in lesbian and gay studies but also for anyone concerned about the implications of the "ideology of the heterosexual nuclear family as the means of raising good citizens" (p. 107). Lehr powerfully engages conservative theorists such as David Blankenhorn, who argues for the necessity of heterosexual, two-parent families, and she demonstrates the ways in which conservative gays and lesbians share their premises and fears about social change. Lehr argues that the current drive for marriage and "normal" families must be seen within the context of larger American family politics, including the increasing conservatism manifest not only among the religious Right but also among communitarian and neoliberal thinkers. She suggests that prioritizing marriage colludes in a larger sexual conservatism, manifest among lesbians and gays as well as heterosexuals. This conservative politics takes sexual orientation as fixed but detaches it from gender so as to suggest that gender nonconformity is a sign of immaturity (see also Gamson 1998).

Lehr's discussion of the political stakes in sexual immutability is invaluable. She draws examples from youth support group manuals, from legal arguments, and from legal and political theorists such as Thomas Eskridge and Andrew Sullivan. For Eskridge and Sullivan, gender nonconformity is a product of oppression that will vanish as equality nears. Yet, since Eskridge (1996) believes that male sexuality is inherently unruly, requiring social constraints, gender conformity does not equal "maturity" for men; maturity must be acquired through marriage and the renunciation of other partners, as "sexual liberty" gives way to "civilized commitment." The widespread popularity of these ideas testifies to the fact that sexual oppression does not guarantee a critical perspective on gender. It is not accidental that Lehr is more centrally involved with feminist theory than queer theory or lesbian and gay studies; as Martin (1996) has noted, recent queer theory has largely abandoned or ignored gender, and many authors have faulted it for its blindness about class and economic issues (Evans 1993; Hennessy

Lehr challenges virtually every comfortable element in contemporary lesbian and gay politics. Along with immutability and the desirability of marriage, she challenges readers to come to terms with polygamy, racial differences in understandings of sexuality, and violence

within marriage. Her critique of identity politics in a book focused on gays and lesbians points to the paradox of constructionist theorizing about lesbians and gays. Lehr recognizes that such groups exist, but she does not accept that they are natural or fixed, nor does she believe that a politics centered simply around sexual identity is sufficient for social change. "To reinforce an understanding of identity as unified around a single aspect of our experience through identity politics is to accept an understanding of the self and definitions of naturalized social reality that make systemic change difficult, if not impossible" (p. 81). Change is made difficult, she argues, because a politics in which some groups simply "assert their separate identity" leaves others (such as "those who are straight, male, or white") unable to respond (p. 83). One wishes that Blasius had had this book available to him when he wrote.

In general I agree with Lehr's critique of identity politics, but it is important to distinguish two issues that are often conflated. First is the question of the naturalization of identities, exempting them from negotiation and change. Related, but not the same, is the political question of whether one's struggle and issues articulate with those of others. Although we often see these two together (as in the insistence of many gay men, recently highlighted at the annual convention of Log Cabin Republicans, that abortion and other "women's issues" are not "gay issues"), they are not inevitably linked. One may understand one's homosexuality as fixed and innate and still understand one's oppression as generated by the same forces that produce other oppressions; one may also be an ontological constructionist and see one's situation in isolation from all others. Constructedness does not guarantee alliance, although many of us would like it to; nor does a belief in immutability necessarily equal consumerist lifestyle politics (Evans 1993).

Lehr opens Queer Family Values with a sharp critique of "rights talk," suggesting that "the extension of rights depoliticizes issues that need to be subject to public debate and discussion" (p. 14). Drawing centrally upon Wendy Brown's (1995) recent work, Lehr states that conceptualizing freedom in terms of rights "keeps us from asking what 'freedom' means" (p. 15) and prevents us from understanding the importance of our collective decision making and mutual self-creation. Questions about "the right to marriage," for example, do not seriously question the institution of marriage itself but, rather, presume both its value and its structure—presumptions that Lehr sees as problematic for gay and lesbian equality. She follows Joan Tronto's (1993) work on care ethics and argues for a more public culture of care to replace the relegation of care and caring activity to the family. These are all important points. Liberal individualism has consistently privatized the most important issues of human life, including those that are in fact decided or structured in "public" arenas. Lehr, however, relies on a narrow interpretation of liberalism to make this point. For her, liberalism is represented by Locke (and, secondarily and by implication, Rawls). She is entirely on target

with her criticism of Lockean liberalism, but that is not the only liberal tradition, indeed, not even the only American liberal tradition. John Stuart Mill's attempt to combine individual liberty of action and expression with economic coordination and John Dewey's liberalism of care and community are absent from the book. Thus, Lehr does not dispose of liberalism as thoroughly as she would like. Her preferred option, radical democracy, in fact has emerged from a synthesis of Gramsci, poststructuralism, and liberalism. Lehr's analysis perhaps would have been sharper had she articulated her target not as "liberalism" per se but as individualism and the remnants of patriarchy. These are her actual targets, and the book is strongest when she trains her sights on them through the reading of contemporary profamily literature of all persuasions. The question of liberalism's usefulness to lesbians and gays is not disposed of by either Blasius or Lehr, it awaits a more direct confrontation.

Lesbian and gay political theory not only includes theory directly focused on the situation(s) of gay and lesbian people but also affords new perspectives on sexual values and the role of sexuality in politics. Just as feminist theorists have not contented themselves with writing about "women" but have used the analytical lens of gender to examine (re)production, the "social contract," citizenship, freedom, obligation, and other "traditional" issues of political theory, so lesbian and gay theorists are focusing on sexuality, and sometimes through sexuality, to gain new insights.

Jeffrey Weeks (1981, 1985) and Steven Seidman (1996), both sociologists of a historical bent, recently have offered considerations of what Weeks calls "sexual values in an age of uncertainty." Drawing on Anthony Giddens, Zygmunt Bauman, Agnes Heller, Michael Walzer, and Charles Taylor, Weeks fashions an argument in Invented Moralities for a "radical humanism" that centers on the values of care, responsibility, respect, and knowledge. His previous work on the history of sexuality and lesbian and gay history has been a major resource for social constructionist arguments. Weeks bases his position on a "historical approach to the erotic" (p. 5), which assumes that sexuality is not simply "there" as a bodily impulse but is subject to social forces. The consequence of this is not the end of values, as conservative opponents allege, but the need for a conscious reexamination and reconstruction of sexual values. Successive chapters outline "the postmodern condition" of uncertainty and its implications for the active shaping of moralities before moving directly to the question of sexual identities; the relationships among privacy, intimacy, and everyday life; and the implications for rights struggles. Throughout, Weeks moves between the general theme of sexual values and the specific situations of lesbians and gays, but his clear intent is not to write a book "about gays and lesbians" but to address all moderns about sexuality.

As his sources might suggest, Weeks begins and ends with a sort of postmodern communitarian position. His premise, a problematic one for the United States, is that "we" live uniformly in a "post-Christian" age,

having "escaped the ethical codes of the Christian era" in favor of a no less oppressive scientific-disciplinary ethical system. This, of course, is a theme shared by Giddens, Foucault, Bauman, and others, but those in the United States may pause. It is surely the case that the ground for values, and the need for ground, has shifted; but a book that begins with the premise that Christianity is no longer a dominant force in public and private life assumes too much for American readers. This premise, however, serves Weeks in his concern about "sexual libertarianism" (p. 29) and "radical individualism." These follow from the belief that "if God is dead, everything is permitted"; they are the continuing companions of traditional onto-theological systems, not the product of (post)modern thought itself. In offering a postmodern sexual ethics, Weeks aims to "reground" ethics in actually existing communities as they foster the autonomy of individuals through mutual support.

Throughout the book Weeks stresses the social supports needed for autonomy and the importance of rights, not in order to escape or construct barriers against others but to enter and be included. In this he is joined by feminist theorists of care as well as communitarians of all stripes. Indeed, the catchwords of "responsibility," "respect," and "care" are as much central to New Right discourse as to progressive politics in the past two decades. Weeks devotes several pages in the last chapter to distinguishing his understanding of care, responsibility, respect, and knowledge from those of the British New Right, for whom these words have signaled the need for privatization of social services, traditional values, and the exclusion of alternative family forms. For Weeks these values are part of an ethic that can stand in the face of death, exemplified by the devastation of AIDS. As he puts it, a recognition of our "equality of vulnerability" in life and death should form the foundation of our values and our rights (p. 141).

If Weeks's largest concerns are communitarian, his particular awareness of marginalization and oppression leads him to a healthy respect for individual rights. Like Lehr, Weeks's concern for care and his appreciation of contingency and construction take him beyond individualism. Unlike Lehr, however, Weeks's rejection of individualism does not lead him to abandon rights; as he says, "the inadequacy of rights-based arguments lies not in the claim to right in itself, but in the absence of a wider social context in which the notion of rights becomes meaningful" (p. 141). Rights should not be seen as the possession of sovereign selves but as an element in the shape of relationships between incomplete and vulnerable selves. It is not our sovereign subjectivity that merits and demands rights but our need for protection and support.

The neediness of the self runs through all the works reviewed here. This is not simply a theoretical fashion or trend. Much less even than gender can we imagine a politics of sexuality that does not center the self in need of others. Sexuality is understood relationally, even in the absence of actual sexual partners. We might imagine sovereign selves in sexuality as in politics, indeed,

many desire exactly that; but bisexual, lesbian, and gay theorists in all fields have made clear the centrality to sexuality not only of relationship but also of vulnerability. For some this relationality seems to mandate a rejection of liberalism, but just as powerful is its reformulation with stronger "communitarian" ontologies. Basing rights not in a sovereign self but in a constructed and vulnerable one makes all the difference. This vulnerable self does not demand privacy, but needs it in order to associate with the intimate and perhaps not-so-intimate others who make up its world. As Morris Kaplan shows us in Sexual Justice, this vulnerability fosters what is best in liberalism over its atomistic contenders,

Although Weeks is part of this reformulation of liberalism, in the end his book is disappointing. Those new to the field will find much to stimulate them, and its lucidity makes it useful in classes, but political theorists will likely find that Weeks does not offer new ideas. This is not a consequence of training but perhaps rather a matter of caution: He seems to see the merit in every view, and explains that merit clearly, but he continually backs away from points that might be controversial. Weeks has interesting ideas, and his analyses of the linkage among rights to difference, to space, and to exit and voice (chap. 4) are often original. but he too rarely argues those points; instead, he asserts and cites others. For example, Weeks does not abandon the idea of authentic sexuality, so central to the modern Western experience of the self, but he proposes to understand it not as "realization of a true self, or a dissolution of the self in the pursuit of polymorphous pleasures," but in the context of "the meaningfulness of our practices of freedom" (p. 69). Yes, I think, but then wonder: What exactly does he mean? It is clear what he rejects (Thatcherite conservatism), but what exactly is he recommending? Annoving hints such as this too often substitute for argument. I found myself agreeing with him but was rarely surprised or challenged.

There is more challenge in Steven Seidman's Difference Troubles. The author begins from quite a different place and assumes a different audience. This fine book consists of several essays published over the last twelve years and many new ones (indeed, there are too many chapters, which results in some repetition). Seidman's theme is the rejection of difference in both sociology and in social theory and politics and the need for a "pragmatic turn" in social theory. The first five chapters address the resistance of sociological theory to conceptualizing otherness, especially sexual difference. The second and third parts move directly to questions of sexual identity in postmodern culture before returning to epistemological concerns, and an epilogue briefly explicates Seidman's view of the relation among pragmatism, difference, and democracy. Perhaps reflecting his position in the U.S. academy rather than the British, Seidman offers extensive notes on the role of religion in education and the diversity of public spheres, and his view of postmodern societies, although largely in accord with Weeks, is much more nuanced.

Seidman's overarching argument is that social theory

has been unable to conceive of differences other than as "problems." He links methodological issues to political and ethical concerns and argues that "epistemological pluralism" and "postdisciplinarity" are not only methodological virtues but also political ones, for "a vital, democratic intellectual culture should include, at its core, the recognition of epistemic and social differences" (p. 41). He faults contemporary sociological theory for abandoning the "moral intent" that he sees as integral to the history of social theory. Political theorists will recognize his critique of a narrowly quantitative sociology and the theory entwined with it as reminiscent of the concern for the political as well as conceptual consequences of behaviorism elaborated by Charles Taylor (1985) and William Connolly (1980). Like Weeks, Seidman hopes that social theorists will again become participants in "elaborated social and moral argumentation over consequences and social values" (p. 59) rather than isolated specialists divorced from public discussion. The criteria for evaluating social theories would be "pragmatic," with an eye to "their intellectual, social, moral, and political consequences" rather than "absolutist justifications," "theoretical logics and epistemic casuistries" that remove them from their contexts (p. 50).

All very well, but one might ask: But what has this to do with queer politics? Seidman has several answers. First, his indictment of sociological theory is focused through that theory's neglect of sexuality: "If our view of modernity derived exclusively from the sociological classics, we would not know that a central part of the great transformation were efforts to create a sphere of sexuality, to organize bodies, pleasures, desires, and acts as they relate to personal and public life" (p. 82). Contemporary sociology incorporates sexuality, but often in ways that do not reflect the concerns or self-understandings of those studied. Those who attempt to "queer" sociology are met not only with sexual stigma but also often with methodological and episemological restrictions that prevent the recognition and study of "sexual regimes," those structures of "sexual meanings, discourses, and practices that are interlaced with social institutions and movements" (p. 86). The study of such regimes is central to the development of both social and political theory away from treating the politics of sexuality as simply another interest group or identity.

Seidman is at his best in the middle chapters. He offers one of the most accessible and helpful treatments of the difference between "studying queers" and "queer theory" that I have read. Unlike "studying queers" (or gays, lesbians, bisexuals, and transgendered people), in which a population is examined using the methods and perspectives "appropriate" to a discipline (e.g., voting studies, the history of lesbians and gays in a given time/place, the effect of violence on sexual minorities—what feminists referred to as the "add women and stir" approach—queer theory begins from the positions and perspectives of sexual minorities in order to reread the social world. Queer theorists have resided primarily in literature and humanities departments, but writers such as Judith Butler (1990,

1993, 1997), Eve Sedgwick (1990, 1993), and David Halperin (1990, 1995) are widely read by scholars in social science departments. Queer theory is overwhelmingly poststructuralist, looking for the ways that sexual identities are constructed in social movements, texts, films, and other cultural media as well as in law and politics. Queer theorists also focus on "queering" ideas, concepts, and practices that are currently exempt from scrutiny, looking both for the ways that queers might become more included or central and for the changes required in the categories for that to happen.

Although much queer theory is now read and taught in sociology and political science departments, humanities scholars have not made comparable use of social science scholarship. Seidman challenges the division between humanities and social science, demonstrating that queer theory has as much to learn from social science as social science from queer theory.2 He faults the lack of "institutional analysis" in queer theory, a lack that produces readings divorced from historical or sociopolitical research. Seidman offers a historical sociology that traces the development of modern gay and lesbian identities and offers a concrete location for critique of the new "ethnic politics" of gays and lesbians. He makes good on the promises of queer theorists to demonstrate that queering identity politics fosters a linkage between specific group politics and a broader democratic agenda.

The concluding chapters of Difference Troubles return to the theme of difference and epistemology, this time through explicitly normative concerns about sexual ethics. Seidman advocates a "communicative sexual ethic" in place of the "morality of the sex act." He argues that the site of normative dispute is not acts or individuals but sexual communities that negotiate over meanings, "moral logics," and institutions (p. 226). A communicative sexual ethic, he suggests, would provide that judgments be "focused on particular acts and ... guided by contextually informed, minimalist, and consequentialist reasoning," with the result that "large stretches of social practice related to bodies and intimacies would lack moral weight" (p. 232). The guiding values of such an ethic would be consent, responsibility, and reciprocity. Seidman is confident that such a "formalist" position affords the best hope for negotiating sexual difference in modern democracies.

Although (or perhaps because) these chapters are the most directly addressed to debates in political theory, they are also the most frustrating. Issues such as sex between children (which he prohibits) and sadomasochism (which he allows) are held to be quite straightforwardly resolved by such an ethic, but reasons are not always sufficiently elaborated. What do "we" mean by "responsibility" in communicative practice? Far from buying general agreement, Seidman's formulation postpones the inevitable conflict over nonformalist substantive values. Because he has substantive

commitments that he knows are controversial, Seidman begins the liberal strategy of bracketing issues from substantive discussion. This leads him to betray other commitments he has made. Seidman's rejection of "reason-based consent" in favor of substantial areas of "pragmatic accommodation" belies his earlier nuanced postmodernism, which persuasively rejected the split between reason and interest. Indeed, notions of consent and responsibility rely on some idea of reason for their content. We might suggest that Seidman's target is the disembodied reason of the foundationalists he abhors, but we should have more guidance from him as we do so.

It must be noted that neither Seidman nor Weeks seems to thoroughly "get" feminism. This is most evident in Weeks, who seems to have no knowledge of feminist scholarship and too little awareness of women's continuing subordination; for example, he characterizes a British feminist journalist as "not marginalized but on the contrary much admired" (p. 166), as though these two are mutually exclusive. The lack appears as well in Seidman's contention that, unlike issues such as the choice (and age) of one's partner and what consenting adults do together, issues clearly derived from battles over gay and lesbian rights, questions of pornography, and abortion rights are "outside the purview of the communicative sexual ethic" (p. 235). These issues will be "resolved at the level of interest and pragmatics." But why should this be? Why does his sexual ethic speak directly to the sexual concerns of one group but not of another? Why are consent, responsibility, and reciprocity so feeble before these questions? Consent and reciprocity seem central to evaluation of pornography, and consent, responsibility, and reciprocity (not only between sexual partners, but between pregnant women and the state) have been key ingredients in the abortion controversy. The relevance and scope of this ethic is not a given matter at all, not in the nature of the questions, but is itself a result of "interest and pragmatics"—in this case, Seidman's interest in sexual freedom for gays and lesbians (qua sexual minorities, rather than lesbians qua women). We can see, then, that working from "the perspectives of gays and lesbians" is not more complete than other perspectives but simply shines the light differently. It does not supplant, but complements, other angles of view.

The treatment of sexuality in isolation from other avenues of power will only take us so far. Like feminist theorists who treat women as implicitly heterosexual, or whose understanding of lesbianism is limited to the "womanness" of it, lesbian and gay theorists who attempt to study sexuality in isolation from race, gender, and economic structures will inevitably provide partial answers at best. Although conceptual separation of systems is useful at certain moments, that separation cannot violate the complexity of lived reality without doing damage to our ability to formulate rights and responsibilities that address the range of needs and conflicts that we encounter in our common life.

In contrast to Blasius and Lehr, who deny a liberalism that they nonetheless seem unable to abandon,

² It should be noted that Seidman has contributed mightily to this synthesis through his edited collections, *Queer Theory/Sociology* (1996) and *Social Postmodernism* (edited with Linda Nicholson, 1996).

Seidman and Weeks offer a pluralism, perhaps even (in Weeks's case) a liberalism, that remains incognito. They adopt positions and authorities that are understood themselves as part of an enlarged liberal project, but they avoid the name that now seems synonymous with abstractions that have been roundly discredited in postfoundationalist theory. In Sexual Justice, Morris Kaplan both defends and extends liberalism, but his liberalism is substantially richer than Sullivan's. His aim is "to go beyond the thin conceptions of legal personality and negative freedom that inform liberal theory and to insist on the concrete social dimension of the assertion of equal citizenship by lesbians and gays" (p. 13). This is quite explicitly not a repudiation of liberalism but a renewal. Kaplan begins from the three "moments" or kinds of claims currently being made: for decriminalization of acts between adults; against discrimination in housing, employment, and accommodations; and for "legal and social recognition of the ethical status of lesbian and gay relationships and community institutions" (p. 14). He demonstrates how these may be justified through (quite different) liberal principles. The first claim may safely rely on negative liberty and the "right to be let alone," but the others require quite different grounds. The second, in common with other civil rights struggles, asks the state to function as a positive guarantor of fairness. The third, as many opponents correctly suggest and defenders often hedge about, requires a commitment to "the moral legitimacy and ethical validity of lesbian and gay ways of life" (p. 17).

Rather than craft an argument that subsumes homosexuality into a more general claim about sexual autonomy, as Weeks does, or focus on gays and lesbians so specifically that heterosexuals may feel themselves to be spectators at an internal debate, as may happen for both Blasius and Lehr, Kaplan simultaneously addresses the specificities of lesbian and gay ways of life (and notes their plurality) and provides general principles for understanding the relation between sexuality and politics. Three chapters act as bookends, addressing the historical construction of lesbian and gay identities and arguing for the three claims described above. These chapters are linked through chapters on Plato, Freud, Arendt, and Thoreau, in each of whom is found components of the author's larger argument. From a reading of Plato's Symposium Kaplan discerns a general "queerness" in sexuality that "derives not simply from its resistance to whatever social norms are historically in place but rather from its figuring of neediness and excess in a dynamic interplay that reveals the human condition as inherently divided and incomplete" (p. 83). This dynamic prevents us from certainties about hierarchies and oppositions between "masculine and feminine, straight and gay," and so might afford us greater humility in our judgments and conclusions. Just as important, the distance between Plato's world and ours, including Greek acceptance of pederasty and the gap between pederasty and modern homosexuality, reminds the reader of the historically bounded nature of ideas about sexuality.

Plato's recognition of the role of the erotic in binding

male citizens is pursued in the chapters on Freud and Thoreau. Freud's teaching of "the pervasiveness of sexuality and the multiplicity of its forms" (p. 118), which has made him so central to contemporary queer theory, also should be important for political theorists seeking to understand the foundations of common life. Thoreau emerges from Kaplan's pen as a teacher of interdependence and friendship, of homoerotic (masculine) democracy. More important for his argument, however, is Kaplan's reading of resistance not as the act of isolated individuals but as a form of action through which "even the worst off" may exercise agency while exhorting "those better off to recognize their involvement in the fates of all who are excluded or marginalized by contemporary regimes" (p. 185). Resistance is a central locus of self-creation and the formation of desire.

Self-creation is central to Kaplan's discussion of Arendt. Arendt treats the public sphere as the central location for self-making and disclosure, both as individuals and as groups. This does not mean, however, that "the public" is the only important location for citizens. Kaplan follows through on his commitment to link theory to concrete circumstances by focusing on Arendt's more directly engaged work, such as her essays on Jewish politics and The Origins of Totalitarianism. He extends Arendt's brief discussion in Origins of the comparisons between Judaism/Jewishness and sodomy/homosexuality, in which she acutely notes (without Foucault's help) the rise of the "homosexual as personage" with an etiology, characteristic flaws, and so on, and sees in it the same hand of medicalization that creates "Jewishness" as a quality extending far beyond religions or cultural specificity to describe an essential type. In both cases, Arendt observes that the transition from "crime" to "vice" was not a benevolent improvement for subject populations. Such transformation allowed the bourgeoisie to combine a fascination with the "interesting" homosexual or Jew with scorn or hatred for the mass of those populations (Arendt, 1968, 80ff). Through readings of contemporary scholars in queer theory and Jewish studies, Kaplan develops Arendt's remarks to argue against essentialism. Kaplan links the critique of essentialism to the goal of equal citizenship, citing Arendt's belief that self-creation is central to democracy. Those whose practices of self-creation are banned, or whose practices construct identities that are legally stigmatized and devalued, are not full citizens. They will instead ride the see-saw of parvenus and pariahs, "good homosexuals" and "bad queers" (Smith 1997), a see-saw shared with racial, religious, and national minorities.

The final chapter returns to contemporary politics to argue for lesbian and gay marriage. Unlike neoconservative gay authors on the subject, such as Sullivan and Eskridge, Kaplan does not argue that marriage is important for "civilizing" men or that it is uniquely sanctified among forms of human relations. Nor, he asserts, should monogamous, sexually exclusive marriage be privileged among family forms. Returning to the discussion in the first chapter of Blackmun's dissent in *Bowers*, Kaplan argues that relational autonomy

requires public recognition of the relationships that give meaning to our lives and that marriage is the premier vehicle of this recognition (p. 210). The fight for marriage is for him the most visible manifestation of the shift from "a defensive fight for mere survival towards an effort to secure the social conditions of human flourishing on equal terms with other citizens" (p. 211). These conditions, furthermore, cannot be mandated but must be continually reinvented and negotiated, as Kaplan's defense of bathhouses and sex clubs demonstrates: He argues for them on the same relational basis he employs for marriage.

In this defense of a "variety of institutions of intimate association" (p. 238), Kaplan reiterates the theme that runs through all these works. Along with Blasius, Lehr, Weeks, and Seidman, Kaplan draws on the experiences and institutions of lesbian and gay life in contemporary Western democracies to adumbrate an understanding of the relation between individuality and relationality that appears in various guises in Hegel, Tocqueville, Mill, Dewey, and Arendt. These authors are not generally treated as a "school" or a "natural group," but they share a distinctively modern concern for individuality and freedom that is combined (in quite different ways) with a "communitarian" recognition of the social networks that create and sustain individuals. Even Kantian liberals, who tend to highlight the individual in contrast to her environment, have learned this lesson. Yet, the slip from the general to the particular is where many liberals fail (in varying degrees) to afford sexual minorities the purchase they need to gain recognition as equals. Abstract individualism is not the only threat to actual equality; more concrete or communitarian formulations that theorize solely from heterosexual presumptions also fail to account adequately for the other possibilities that are the daily realities of many. Our authors demonstrate that the major social cost of lesbian and gay existence is not (only) formal or legal exclusion but the invalidation of the ethical legitimacy of our lives. Denied acknowledgment, we are stripped of the supports of family that heterosexuals take for granted. This denial has spurred a host of creative institution-building, but it is denial nonetheless. Being barred from the bedsides of our loved ones (or their funerals) because we are "not family," rejection from our families of birth (or acceptance conditional on pretending not to have relationships), and unanswered vilification in public are as damaging to the creation of the individuality vaunted by liberalism as physical violence and legal

The authors under consideration here all recognize that such a situation requires the remedy of rights, but they clearly disagree about what rights mean. Blasius's formulation of relational rights suggests that they are needed specifically in order to counter disciplinary power, whereas Kaplan, Weeks, and Seidman argue from a more general recognition of the limits of abstract individualism and the need to locate subjects and their rights in the larger contexts of their lives. Lehr, the sharpest critic of rights, argues for the necessity of meaningful changes in economic systems if

rights are to be effective for more than a minority. The fact of their agreement that rights are needed, while they offer different (sometimes overlapping) arguments for rights, leads us to wonder what each finds in rights that is so important. Simplifying, I would say that the shared thread they find is protection. As Patricia Williams (1990) has noted, white critical legal theorists may reject rights with more confidence than do critical race theorists precisely because they are less endangered in a world without rights. Like racial minorities, sexual minorities find themselves in need of rights and rights talk to protect their lives, their relationships, and their presence in schools, at work, and in a host of other institutions that might otherwise simply dismiss or persecute them. Even the most critical of queer theorists returns to rights when needed.

Of course, there is more to liberalism than rights. Liberalism includes claims about the sanctity of the private (neatly marked off in ways that never quite succeed)—the separation of church from state, economy from government, and bedroom from polity-that provide endless ground for rearticulation. Liberalism's principles of tolerance leave it open to castigation from right, left, and elsewhere. Yet, liberalism funnels all these controversies through the narrow path of rights. And it is precisely the intense need for rights that continually leads minorities to reencounter, reformulate principles that are so obviously defective on logical, ontological, and (finally) political grounds. In the current state of things, only a close relative of liberalism can assist those who seek to live among the heterosexuals. That does not mean that they must endorse a particular version of liberalism; these books make clear that liberal concerns are not always or only addressed by Rawls or Dworkin or Habermas, and that "liberal concerns" might include the needs of a vulnerable self for whom relationships and recognition are as basic as the need for food. Although these authors range in their attitudes toward liberalism, from Kaplan's defense to Lehr's critique, in the last analysis they do not so much dispose of it as transpose it to another key. This transposed liberalism allows for complex melodies of interdependence in which that interdependence requires not conformity and unity, as more faithfully communitarian siren songs have claimed, but proliferation of opportunities for selfcreation. As radical democrats such as Chantal Mouffe (1993) argue in criticizing Rawlsian liberalism, and as Seidman and Weeks emphasize, this proliferation does not lead to chaos but to new counterpoints that enrich the theme of liberty and equality.

Social change is enabled, as well as blocked, by the institutions of liberal democracy. These institutions, in turn, depend upon citizens' understanding of their importance for their lives as individuals and as a community. In their concern both for their particular communities and for the relation between those communities and wider polities, these books all speak to the heart of political theory—who we are and how we live together. Readers of all persuasions will find much to work with in these books, and in the scholarship that they make possible.

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BOOK REVIEWS

Political Theory

Ethics for Adversaries: The Morality of Roles in Public and Professional Life. By Arthur Isak Applbaum. Princeton, NJ: Princeton University Press, 1999. 288p. \$29.95.

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Impressive substantive knowledge, analytical rigor, and Kantian and liberal democratic commitments are all in play in this careful exploration of the morality of roles in adversary institutions. Extending the concept from its usual location in accounts of legal, especially criminal, representation in the American legal system, adversary institutions and professions are conceived as practices in which role players like business executives, lawyers, public officials and bureaucrats adopt partisan and limited, and therefore often conflicting, perspectives and interests, and claim the right to intentionally harm others, on the ground that such partiality, conflict, and conduct will help promote or secure valued outcomes or goods such as prosperity, justice, and legitimate government.

The "central question" addressed in the book is how acting in an adversary institution and role can morally justify or "morally permit actions that otherwise would be morally wrong" (p. 10). Many common justifications are examined, and all are found to be "weaker than [often] supposed." Adversary "institutions and the roles they create ordinarily cannot mint moral permissions to do what otherwise would

be morally prohibited" (p. 3).

A very brief account of the main justifications considered, and criticisms made, follows. Adversaries may try to defend themselves by appealing to role prescriptions, including permissions to harm, as if their mere existence justified compliance. But the argument of chapter 3 is that roles have no "independent moral force or ... grounding," and therefore cannot by themselves create moral obligations or permissions to do anything, much less anything wrong (p. 45). Another defense, explored in chapter 4, is that a role which has moral value imposes on its occupants moral obligations "to defer to authority and comply with its rules," including the common rule demanding neutrality, or the suspension of personal judgment regarding the substantive merits of what one is supposed to do (p. 60). Focusing on civil servants in a liberal democratic setting, the author contends to the contrary that the suspension of political and moral judgments regarding both the legitimacy and justice of authoritative orders and policies is unacceptable since the morality of the role and its obligations presuppose the legitimacy of both and of any actions they may prescribe. Unreflective deference is never justified; resistance and disobedience sometimes is.

Ensuing chapters canvass more interesting and challenging defenses. Chapter 5 examines "the strategy of redescription" or the claim that professional practices and roles "create new ways of acting that can be judged only by the rules of the practice" (p. 10). Rules governing the legal profession, for instance, permit or require a variety of deceitful actions which, however, are said to be misdescribed as deceitful; actually, these actions are forms of zealous advocacy, and they are legitimate, even obligatory, and cannot possibly be moral wrongs. Acknowledging that this strategy tracks compelling accounts of how social practices can constitute action descriptions (by, e.g., John R. Searle, Speech Acts, 1969), the author nonetheless mounts a successful critique of it. The essential point is that the strategy fails because it forgets that institutionally constituted action descriptions (or redescriptions) are not exclusive or even privileged. Although it is true that "practice-defined descriptions can be judged only by the terms set out by the practice," it is not true that a "practice-defined description is the only apt description" (p. 91). Thus, while advocacy that involves misleading jurors or litigants is on most accounts of legal practice truly good or competent legal behavior, it is also deceitful behavior that needs to be, if one cares about morality, justified. Moral scrutiny is not blocked by redescription.

Another justification of harmful advocacy behavior, discussed in chapter 6, appeals to rules of the game. The defense is that victims consent to, or as a matter of fairness must accept, their treatment, much as players in a game of poker must accept being targets of deceit. The key criticism of the consent argument, made before by many writers (e.g., Sissela Bok, Lying, 1978, and Dennis Thompson, Political Ethics and Public Office, 1987), is that it asserts what is usually false: Victims of adversary practices typically do not consent to being mistreated. The argument from fair play reflects a careful reworking of well-known literature asserting, and questioning, the idea that persons may incur obligations to do their part in a just scheme of cooperation by virtue of their voluntary acceptance of benefits provided by others in that scheme; the reworking holds that permissions to harm persons are justified if they accept or receive benefits in a just scheme. The author's basic criticism of this argument is that, at least in games of market competition, electoral politics, and adversary law, consumers, citizens, and litigants are typically neither advantaged by, nor do they voluntarily seek, the games' benefits. "Business managers, public officials, and lawyers may satisfy the conditions of fair play in their deceptive or coercive dealings with one another," however, so when they are deceived or coerced they "may have no reasonable grounds for complaint" (p. 135).

In the longest and most technical chapter of the book, chapter 7, the author explores defenses that reflect and develop debates between consequentialists and Kantians concerning the permissibility of intentional harm. Considerable effort is expended establishing a "conceptual case for morally permissible violations of persons [on nonconsequentialist or Kantian grounds]" (p. 173). When refusing to violate a person(s) is "self-defeating" because the person(s) will suffer a more serious violation as a result, or when refusing is "Pareto-inferior" because the person(s) will suffer anyway and others will as well, a Kantian can ("with reluctance and regret") justifiably violate the person(s), knowing that "reasonable" persons would, ex ante, consent to such treatment (pp. 150, 165). After making this "conceptual" argument, the author (with unintended irony) explains that it provides little help for those who would defend most of the violations adversary practices permit, since those practices typically permit violations in circumstances that would not warrant reasonable consent (although there are exceptions which the author describes).

A final defense, or set of related defenses, is examined in chapter 7, where the focus is on appeals to the social goods secured by adversary institutions. One central criticism of such appeals is that they characteristically exaggerate the good results adversarial practices manage to produce. Another is that, even if some adversary practice does produce, overall, good outcomes, this can at best justify institutional permissions to harm but not, necessarily or automatically, intentional harming. This is so for two reasons: First, particular cases of harming may not serve, they may even undermine or subvert, the ends which justify institutional permissions; second, even if the end and action of an adversary agent in a particular case does serve good institutional purposes, intentionally harming others is not thereby justified. Good ends alone do not justify wrongful behavior.

This last point, of course, is a conclusion compelled by the author's commitment to Kantianism; it is not, therefore, wholly compelling. It would not persuade adversaries and defenders of adversarial practices who knowingly embrace some variety of moral consequentialism. Perhaps the central weakness of this thoughtful book is the author's admission that "if consequentialism is the correct moral theory, there is no serious objection to an adversary institution that produces enough social good to outweigh... the bad" (pp. 176-7). A related weakness is his relative neglect of what might be termed the phenomenology of role playing, which can explain not only why adversaries and other role occupants may be deaf to the demands of morality but also may make particular moral judgments of which he cannot approve (see, e.g., Elizabeth Wolgast, Ethics of an Artificial Person, 1992).

Because I have focused on the central question addressed by the author, I have neglected other chapters, and virtues, in the book. Well-written, full of interesting cases and real and imagined examples, Applbaum has drawn on much of his previously published work to present an illuminating and forceful critique of the arguments even good people in professional and political life employ to justify the bad things they do. For those interested in role morality and professional ethics, and who appreciate the style and results of analytical moral and political philosophy done by the likes of Nagel and Williams, Rawls and Dworkin, David Luban and Dennis Thompson, Ethics for Adversaries is a must read. Undergraduates would probably find it a hard go.

Why I Am Not a Secularist. By William E. Connolly. Minneapolis: University of Minnesota Press, 1999. 210p. \$29.95.

J. Donald Moon, Wesleyan University

This is the latest in a series of works in which Connolly has been interrogating liberalism and its fellow travelers. Drawing on thinkers such as Nietzsche, Foucault, Deleuze, and Arendt, he has become one of liberalism's deepest and most original critics today. And he is one of liberalism's most valuable critics, in part because his own intellectual journey begins with the experiences and preoccupations that led to liberalism. This is evident in the title of this work, which echoes and pays homage to Bertrand Russell's Why I Am Not a Christian, a text that is very much in the tradition of the secularism Connolly criticizes. It is also reflected in an engaging biographical story that introduces the work, in which Connolly describes how at a tender age he came to be an atheist. But for Connolly, liberalism generally and secularism in particular do not provide viable accounts of these experiences, or answers to these concerns.

Connolly conceives of "secularism" broadly, as the "wish to provide an authoritative and self-sufficient public space equipped to regulate and limit 'religious' disputes in public life" (p. 5). Thus, secularism offers itself as the alternative to the aggrandizing claims of religious truth to constitute the moral and political center of society, an alternative that becomes increasingly attractive as religious truth becomes religious truths and competing visions lead to sectarian conflict and violence. But in offering itself as the new center for political life, secularism commits the same errors as its traditional antagonists; although only one voice in the conversation, it sets itself up as the single, authoritative source to adjudicate the differences among the others. In doing so it fails to see its own limitations, and it creates its own forms of dogmatism and intolerance, which, ironically, intensify the

very forms of sectarian conflict that it was intended to overcome.

Perhaps the greatest failing of secularism is its faith in reason, or rather a certain conception of reason, one that is blind to what Connolly calls "the visceral register of subjectivity and intersubjectivity" (p. 24). Drawing on various sources, including recent brain research, Connolly insists that thinking, remembering, and feeling cannot be reduced to or modeled in terms of conscious, linguistically formulated processes of thought. Ethical life, similarly, should not be seen as enacting (or conforming our actions to) our beliefs and ideals but includes practices that actively engage "the visceral register of being" (p. 29). Public life cannot even in principle be a matter of pure argument delivering authoritative judgments that all rational citizens must, on account of their being rational, accept; rather, it is "always accompanied and informed . . . by visceral intensities of thinking, prejudgment, and sensibility" (p. 36). And that can be a good thing, since it can lead to creativity and new forms of communica-

Not surprisingly, if there is a single figure with whom Connolly is wrestling in these pages, it is Kant and his contemporary descendants, notably Habermas and Rawls. Much of the book consists of sustained engagements with these figures, but it also addresses the political failings of a secular vision of politics, including the war against drugs, the use of the death penalty, and the debate over a right to assisted suicide. Running throughout these analyses is what we might call a therapeutic intention, an effort to wean us away from the need for unity, for structure, for authoritative centers and control. One of Connolly's favorite images is the rhizome, literally a root-like structure consisting of a system of interconnected roots, tubers, stems, and so on, with no single or main stem to which they all feed. Culturally and politically, Connolly calls on us to embrace what he calls 'deep pluralism," whereby we are connected to others in a variety of ways, organizing ourselves into a complex array of partially overlapping and intersecting associations, without the need for a common center, whether it be the idealized nation or the majesty of public reason or even the purity of Arendtian politics. Against the secularist strategy of insulating political life from contested religious or metaphysical views, we should embrace a politics of contestation in which conflicting perspectives are admitted into political life. By embracing these conflicts, rather than seeking to transcend them, we are more likely to come to see the contestability of our own moral sources and to discover new possibilitiesnew questions, new ways of engaging with others, new alternatives. The strategy of secularism was an effort to avoid conflict by retreating to an ever more rarified conception of the public realm, and it has failed. Instead, we need a "democratic ethos of engagement across multidimensional lines of difference" that is "jointed to a sensibility that affirms the ambiguity of being and the deep contingency of things" (p. 186).

This is an attractive vision. Connolly is correct to argue that the "reciprocal acknowledgment" by all that "no party has shown its perspective to be undeniable" could widen the scope for a "politics of forbearance, generosity, and selective collaboration between interdependent partisans" (p. 187). But one wonders how real a break this is from the politics of secularism. In the first place, secularism also demands that we reciprocally acknowledge the contestability of our own positions, and it is precisely this demand that has been—and is being—resisted by those who seek to embody their own ideals in legislation and public policy. But more important, it is hard to see how Connolly's "ethos of forbearance and

generosity" could dispense with the basic strategy of secularism. While admirable in avoiding false claims to finality and universality, rhizomatic pluralism would, like secularism, have to limit the scope of political authority and action to create spaces within which differences can be expressed and partial, limited collaboration among antagonists could be achieved. Furthermore, an ethos of forbearance and generosity could hardly endorse the use of political power for ends that citizens have good reason to reject. Perhaps these similarities are not surprising, for Connolly does not reject secularism but, rather, seeks to chasten it.

The Dark Side of the Left: Illiberal Egalitarianism in America. By Richard J. Ellis. Lawrence: University Press of Kansas, 1998. 426p. \$34.95.

Don Herzog, University of Michigan

In this elegantly written, provocative, and sometimes just plain provoking book, punctuated by bits of anguish and rather more pique, Richard Ellis worries that the American Left has been so passionate about equality that it has run roughshod over liberty. So put, the thesis is not exactly news. It has been the recurrent lament of conservative indictments—Tocqueville's is the canonical statement, but he has plenty of precursors and followers. And it has its scholarly variations, too, such as Arthur Lipow, Authoritarian Socialism in America: Edward Bellamy and the Nationalist Movement (1982). No profound surprises are on offer here.

So what does Ellis bring to the hoary old wisdom? For one, he writes as himself a liberal, "a card-carrying member of the American Civil Liberties Union and an avid supporter of public broadcasting and Big Bird" (p. ix). He is unwilling simply to write off leftist causes as so much pernicious tripe, so his criticism does not just sound different; it has different force. For another, he does not rest content with stylized abstractions, or the same old two or three fabled stories you have heard countless times before. The book is a colorful and detailed, if opportunistic, tour through American history. Ellis has done some primary research, but he also draws heavily on familiar published works by American historians. Then again, historians have something to learn from his blunt and well-done challenge to Richard Hofstadter's influential view that we should see this illiberal terrain as the property of paranoid right-wingers.

The tale opens with abolitionist opponents of slavery, whose fierce moralism, their conviction that slavery was evil incarnate, made them famously uncompromising. (I do not think Ellis quite wants to counsel, against Garrison, compromise with slaveholders, but he does want to warn us right away of the political dangers of striking such a pose.) One might wish the tale had started earlier, indeed, as early as the Puritan settlers and their sustained efforts to run pure, even holy, commonwealths. It would be nice to know what, if anything, changed after the ratification of the Constitution and the arrival of the United States of America.

Ellis moves on, rapidly, through tours of utopianism from Bellamy to Llano and left-wing contempt for the "masses" of ordinary people from Walt Whitman to communist writer Mike Gold. Picking up steam, he offers a genuinely savage skewering of the New Left, its romanticization of the lumpen-proletariat and other benighted groups and polities, and the pathologies and idiocies surrounding their abortive campaigns to have political organizations without leadership or authority. Turning a now thoroughly jaundiced eye to the contemporary scene, Ellis pillories radical feminists—his

account of Catharine MacKinnon is blurry at best, sometimes downright misleading—and environmentalists.

There is much food for thought here, but too much of the digestion is left for the reader. Ellis owes us a more precise account than the one he furnishes of just which versions of "equality" conflict with just which versions of "liberty." If you think equality means everyone has identical income and wealth, and liberty means an unregulated free market, then hey, prestol the two conflict. But suppose you think equality is a matter of the state ignoring the religious attachments of its citizens; of the law ignoring irrelevant facts about race, poverty, sexual orientation, social status, and the like; more generally of all institutions' ignoring contextually irrelevant facts. This is itself just a classical liberal demand. Does it conflict with any interesting sense of liberty? Or, indeed, is it partly constitutive of what we mean by liberty?

Or try this: Ellis's strategy is to locate leftists who say and do disturbingly illiberal things and then notice that they are egalitarians. But it does not yet follow that their egalitarianism is the source of their illiberalism. Are egalitarians always illiberal? (Ellis acknowledges that William Randolph Bourne's "radical egalitarian vision" [p. 99] offers much for liberals as well as radicals, but then he suggests very quickly that it cannot be sustained for long. I suppose Bourne died too young for us to be sure. To give one of many examples he simply does not discuss, what of the politics that led Emma Goldman and Alexander Berkman to abominate both World War I America and its hundred-percenters and the Soviet Union?) Ellis notices (pp. 279–84) that more remains to be said on this subject, that the historical vignettes cannot interpret themselves or settle anything.

Fair enough. But in fact, his own book says more. For instance, Ellis has intriguing things to say about radical and utopian social organizations that cut off contact with the broader society, painting it as corrupt; and about how such dynamics help set up or reinforce Manichaean us/them distinctions that invite contempt for the other. So the machinery he actually develops has more working parts, more nuance, than any simple wire running straight from equality to illiberalism. My suspicion is that the most we can say in these matters is that there are loosely knit, highly contingent, but still recognizable constellations of political positions featuring both some kinds of egalitarianism and some kinds of illiberalism, and that it is just mistaken to assign priority to any one position in such constellations. But even if I am right, this would not make it sensible to set aside Ellis's book. His evidence and argument are better than his title and official statement of purpose suggest.

New Wine and Old Bottles: International Politics and Ethical Discourse. By Jean Bethke Elshtain. Notre Dame, IN: University of Notre Dame Press, 1998. 104p. \$14.95 paper.

Douglas P. Lackey, Baruch College and the Graduate Center Ctty University of New York

This book provides a revised text of Jean Elshtain's Hesburgh lectures, delivered at Notre Dame in 1996, together with comments by Fred Dallmayr, Martha Merritt, and Raimo Vayrynen.

Élshtain, Laura Spelman Rockefeller Professor of Social and Political Ethics, University of Chicago Divinity School, combines in her books a capacity for philosophical dialectic with a keen eye for historical detail, somewhat in the manner of Hannah Arendt, whose mantle, these lectures show, she has inherited. There is the same sense of deep moral concern, the same ability to view history from an almost Hegelian

height, the same impression of historical processes unfolding with tragic grandeur, the same somewhat paradoxical emphasis on individual responsibility in the midst of these processes.

The grand concepts around which these lectures are organized are sovereignty, nationalism, and forgiveness (in the sense of forgiving political injustices, not personal affronts). The first two concepts blur together in the phrase "national sovereignty," but Elshtain distinguishes them by distinguishing their histories. The notion of sovereignty was developed by such late-medieval theorists as Jean Bodin and found concrete expression in arrangements associated with the Treaty of Westphalia; it insists on the distinction between "external matters" and "internal matters" (usually via reference to geographical borders) and defines sovereignty as final control over internal matters. The program of nationalism was developed by late-eighteenth-century thinkers and found concrete expression in nineteenth-century antiimperial and twentieth-century anticolonial movements. It insists that membership in a nation is essential to personal identity and maintains that suppression of distinctive national ways of life is an evil more grave than war. The two concepts combine in the principle of national self-determination, which seeks to remedy the political suppression of national life by assigning to each nation or people a separate sovereign state.

It is a weary fact of recent history that movements for self-determination do well in overthrowing oppressive empires but do poorly in suppressing their own chauvinistic urges. Given a conceptual separation of sovereignty from nationhood, the apparent remedy for the excesses of nationalism is the relocation of sovereignty in transnational entities. But Elshtain steadfastly refuses to give up the primacy of national sovereignty. Instead, she advocates an attenuated or "chastened" version of it.

Why the insistence of national sovereignty? Because

the nation state is a phenomenon that cannot be imagined or legislated out of existence. Needing others to define ourselves, we will remain inside a state-centered discourse, for better or worse... Human beings require concrete reference groups in order to attain or secure individual identity... In order to sustain crivic life with any degree of robustness, a perceived sense of shared identity is necessary.... A world of many nations each with its own particular marks of identity, reminds us that we are not alone and that we cannot and ought not to make the world 'one' by cruelly obliterating diverse ways of life (pp. 23, 24, 28, 33).

Enunciated with such passion, this identity thesis demands careful reflection and attention to counterexamples. Consider someone who loves mathematics, undertakes mathematical research, posts results in journals and on the Internet, corresponds daily with mathematicians worldwide via email, and obtains a sense of identity in terms of this work. Does this mathematician not have an identity, and one that has nothing to do with national boundaries? Likewise the American Christian who is as concerned with non-American souls as American ones, and likewise the parent whose sense of purpose derives primarily from family life. They all require peace and order, but peace and order can be supplied by empires as well as nation-states. For all of them the nationstate is a means, not an end. Saying so is no endorsement of conformity. Diverse communities do not require diverse nations.

Why is Elahtain averse to transnational solutions? One problem is that a larger confederation cannot be checked by any of its component parts, and totalitarianism looms (p. 7). This point was urged before by the Whiskey Rebels of 1793, but the United States, despite lapses at Waco and Ruby Ridge, has thus far avoided the full collapse of liberty. A second problem is that confederations and empires do not

evoke the love and loyalty that nations do, and hence they lack the necessary spiritual glue that supplies order without force. Here perhaps a possible reply is that affective loyalty can be invested in local communities and affinity groups, and a different, self-interested loyalty can be directed to the larger entities that provide safety for the smaller.

Reason and self-interest play a small role in Elshtain's thinking and remedies. But the sense of self-interest that ended the religious wars of the seventeenth century in favor of Westphalian arrangements may prevail over the nationalist irredentisms of the late twentieth century, which cannot claim to be more passionate. Elshtain's suggested alternative escape from the cycles of passion and revenge is forgiveness, which "disrupts automatic processes and opens a space for something new to begin" (p. 41). But forgiveness is sometimes prudent, sometimes not, and reason is needed to tell us when the time is right for forgiveness.

According to Elshtain, for example, "the Japanese government has apologized to all women still alive who were forced into prostitution as 'comfort ladies' under Japanese dominion in World War II" (p. 45). But in truth there has been no official apology (only an acknowledgment of fact in 1993), and compensation has been doled out indirectly through a nongovernmental organization, so no Japanese official touches money that is received by a victim of these crimes. Forgiveness in the face of such duplicity goes against all that is reasonable.

Elshtain includes a derisive paragraph about Sir Julian Huxley, who predicted that science would prevail and religion disappear by the end of the century. Myths indeed we still have with us, and they inspire and move many to do great things. Elshtain is right to remind us of the power and virtue of myth. The problem comes, and here the line must be drawn, when myths get armies.

The Community of Rights. By Alan Gewirth. Chicago: University of Chicago Press, 1996. 396p. \$39.95 paper.

Benjamin R. Barber, Rutgers University

We live in an era of the triumph of market ideology—an era of privatization and commercialization after the "end of big government," in which no government intervention (however mild) is safe from criticism, and no market mechanism (however violent and unjust) is subject to rebuke. It is thus of more than merely scholastic interest that the distinguished philosopher Alan Gewirth has written a sequel to his classic Reason and Morality that sets out a powerful case for an active government role in securing positive economic and social rights. The Community of Rights is premised on the passionate conviction that there are "vast unfulfilled needs for freedom and well-being on the part of masses of persons in the United States and throughout the world" (pp. 356-7) that are not being addressed by conventional political rights or the conventional centrist politics that accompany them. Gewirth means to make a philosophical argument around the construct of "the community of rights" that brings into a "relation of mutual support" the seemingly antithetical notions of rights and community.

Gewirth's project engages him in a challenging defense of positive rights, a critical analysis of community and communitarianism, a discussion of welfare statism and its costs (especially dependency), and an elucidation of concrete governmental strategies for securing community and positive rights—above all economic rights. His argument unfolds as a logical exploration of the entailments of agency (at the core of rights discourse) in a world of action that reveals agency's

primal interdependence. Because, Gewirth argues, agency entails action, and action requires intentionality (purposiveness) whose end is well-being, rights holders "logically must acknowledge they have correlative duties" and recognize that their rights commit them to the proposition that "all other agents have the same rights equally" (p. 17). From this consideration comes what Gewirth calls the "Principle of Generic Consistency," which dictates that one must "act in accord with the generic rights of your recipients as well as yourself" (p. 19). That is, "the agent recognizes that other persons are similar to her in being prospective purposive agents and having the needs of agency, and on this ground she rationally accepts that they have the same positive rights she necessarily claims for herself" (p. 19). In this manner, rights and community are brought together.

Claims of this kind are not, of course, without precedent. Gewirth comes out of a long tradition, about which he is curiously mute, a tradition that can be traced from Rousseau and Hegel through T. H. Green. As did Rousseau, such thinkers know that rights "must be based on convention" (The Social Contract). Gewirth would certainly agree with Cass Sunstein and Steven Holmes, who in their new book, The Cost of Rights (1999), posit that "rights are public goods: tax-payer-funded and government managed social services designed to improve collective and individual well-being. All rights are positive rights" (Sunstein and Holmes 1999, 48).

Gewirth applies himself to rooting his argument in logical consistency (a philosophical task), but he also demonstrates how positive rights grounded in agency play out in the justification of interventionist social and economic policies by a strong government (a political task). Hence, despite its philosophical Kantian injunction to obey consistency, The Community of Rights gives a political utilitarian inflection to rights discourse: It is rooted in the value of individual agency (as are negative rights arguments), but it affirms that rights are necessarily positive and communitarian-vehicles for sustaining "more healthy and happy persons" whose needs are accommodated by the whole community, even when that means compromising or abrogating private rights to property. Autonomy cannot, Gewirth is sure, entail an absolutist defense of individualism or property, for the very existence of poverty is a "prima facie violation of human rights" (p. 108). Moreover, while Gewirth wishes to soften the opposition between freedom and common ground that characterizes the rift between liberals and communitarians, he is also clear that "the scope of economic democracy must be broadened so that it encompasses a whole national economy" (p. 298).

The power of this approach is that in ameliorating the stark dichotomies of individual and community it provides a broad justification, compatible with rights discourse, for positive governmental health, education, and welfare policies as well as for a politics of redistribution. Yet, it would underestimate the ambition of Gewirth's work to understate the problems it encounters. Some are theoretical, and some are very practical and political in nature.

On the philosophical plain, I do not find the argument from rational consistency to be persuasive. Political agents obey interests, not formal rationality or logical necessity. Kantian moral formulas that enjoin people to act as if the principles underlying their actions were to become universal may describe a moral deontology, but they do not capture political conduct in a consequentialist world of the kind to which Gewirth's utilitarianism commits him, a world inhabited by free riders, interest-mongering egoists, and their unruly cousins.

More than such theoretical questions, however, it is prac-

tical political dilemmas that are posed by Gewirth's analysis. It is not always clear that Gewirth has fully absorbed the lessons of the failures of progressive politics in America and Europe over the last half century. (I say this as a progressive.) It is not simply that as a utilitarian he seems at times a little tone deaf to the claims of individuals, as when he subordinates private property (liberty's twin) to the claims of liberty properly understood (the claims of the common good to which agency logically must be wedded). It is that he seems blind to the political and historical consequences of his rationally grounded refusal to distinguish liberty's negative imperative (Leave me alone! Laissez faire!) from its putative positive entailments (Help me! Do for me!).

However powerful the philosophical case for the commingling of the two on logical grounds, the politics of negative rights is limited government liberalism, while the politics of positive rights is a potentially limitless governmental encroachment, as with the formula "poverty is a prima facie violation of human rights." Gewirth does not back off from acknowledging the socialist consequences of his positive liberty logic, but he obviously worries about the political consequences of his socialist logic (so readily apparent in the history of socialist political experiments in the twentieth century.) The kinds of command economies mandated by social justice and the cry "eradicate poverty" have often failed to forge justice, have endangered liberty, and have sometimes made for poor economics as well. (Again, I say this as a progressive.)

Perhaps these difficulties are related to the portentous asymmetry between negative rights arguments rooted in the safe cry "Don't do that!" and positive rights arguments rooted in the alarming injunction "You must do that!" It is certainly true that "leaving people alone" to enjoy their freedom who do not possess the means to be free in the first place is a treacherous kind of hypocrisy, but it is also true that demanding from those who are free a supererogatory effort on behalf of those who are not may compromise generic rights fatally. Gewirth acknowledges as much when he offers balancing warnings about the dangers of "duty overload" and reminds welfare statists that when they take from some to empower others to acquire liberty, those who give must be in "a position to give it without comparable cost," in other words must have a "surplus."

But who measures the "comparable cost?" How are we to determine when the free have a "surplus" from which the unfree can draw to increase their own share? If the choice is discretionary—dependent upon supererogatory choice—there is no moral problem of encroachment, but there is not very much likelihood of redistribution either. If the choice is coercively imposed, it can hardly be regarded as consistent with agency and is likely to be politically tyrannical as well. Just how far-reaching the encroachment may be is evident in Gewirth's striking claim that the haves may be obligated not only to guarantee the have-nots welfare assistance or jobs but also to assure that they have creative jobs. That is to say, welfare recipients must literally be "given realistic hope that they can develop their abilities of productive agency so that they are not forced to work at the lowest levels of their competence, but instead are enabled to come as close as their latent abilities permit to the creative mode of work" (p. 131). What the costs in coercion (some might say totalitarianism) of legislating this excessively positive notion of rights might be is scarcely to be imagined (or perhaps all too readily imagined).

Greater attention to Rousseau's formula making ongoing participation in the creation of the general rules regarding

justice the condition for obedience to just community principles might have helped Gewirth soften the coercive entailments of positive rights political strategies, but he seems more interested in getting rights to secure just ends than in legitimating them through consent, participation, and political engagement, and thus protecting autonomy. He tries always to contain the contraries of individual and community in his own formulas, but at times this has more the feel of fudge than dialectic. Will workers use the "creative potential" caveat to "shirk" work? Perhaps, but shirking is "a more complex phenomenon." Should welfare mothers be constrained from bringing more children into the world? "There is merit" on both sides of the argument is his evasive reply. Even on the large issues, Gewirth displays an ambivalence camouflaged as dialectic: "In one respect," he writes, "there should be common or public ownership of the means of production," but then, thinking better of so radical a statement, he adds: "but in another respect not" (p. 196). This is not very helpful.

Perhaps Gewirth's approach to the hard questions of welfare would have been helped by greater reliance on the empirical social science research of such scholars as Bill Wilson, Frances Fox Piven, and Andrew Hacker, who take up the moral and rights questions that exercise Gewirth from a sociological perspective on welfare realities. As it is, his political good will and enthusiasm sometimes outrun his empirical political grasp, putting outcomes in front of democracy and justice before autonomy.

These criticisms are meant as a tribute to a remarkable book that more than any other in the last decade marries philosophical and political analysis and grounds the argument for governmental involvement in issues of welfare, redistribution, and social justice in a rigorously rationalist examination of the logic of rights. If it is not fully persuasive in its conclusions, this is only because Gewirth is grappling with the most intractable dilemmas of political philosophy, and in the best tradition of Rousseau, Kant, and Rawls he is attempting to give political expression to his deep sense of justice without violating the spirit of autonomous agency on which—if it is not to become that "gentle despotism of reason" favored by Enlightenment rationalists—justice must ultimately rest.

The Inclusion of the Other: Studies in Political Theory. By Jürgen Habermas. Cambridge, MA: MIT Press. 1998. 300p. \$32.00.

James Johnson, University of Rochester

This volume contains ten essays that Habermas wrote in the mid-1990s. The essays trace the connections between his claim that morality has universalistic bases and pressing political issues raised by what we might loosely call "multiculturalism." The volume thus is valuable to the extent that it renders Habermas's earlier philosophical work accessible by extending it to more topical issues of politics. Yet, because they presuppose familiarity with that earlier work (as developed in *The Theory of Communicative Action* [1985/1987] and Between Facts and Norms [1996]), the essays place a considerable burden on the reader.

The first essay goes some distance toward relieving this burden. There Habermas seeks to defend what he calls the "cognitive content of morality" by arguing that moral claims can be justified with reasons. To do so he invokes the central tenet of his theory of communicative action, namely, that reasons, depending on their subject, necessarily embody validity claims to truth, normative rightness, or sincerity. On

this basis he endeavors to identify the conditions of successful deliberation on moral, legal, or political questions. He develops his own views in ongoing contrast with a variety of philosophical alternatives and in the process inadvertently underscores the parochialism of much analytical political thought. Although Habermas discerns and explicates the central arguments of a wide array of prominent and even relatively obscure Anglo-American philosophers and political theorists, those same authors typically demonstrate scant ability or willingness to reciprocate.

The point of the opening essay is not merely to reiterate and summarize Habermas's views. Rather, it is meant to show how, even under conditions of irreducible ethical and cultural pluralism, the communicative processes in which all political agents unavoidably participate afford the conditions needed for "a nonleveling and nonappropriating inclusion of the other in his otherness" (p. 40, emphasis in original). Stated oversimply, communicative action necessarily trades on the force of validity claims that in principle can be criticized. It is this susceptibility to criticism that highlights the importance of norms of openness and reciprocity. Discourse, understood as the reflexive form of everyday communication (p. 41), thus itself defines the permanently permeable bounds of an inclusive community within which political agents might, as equals, negotiate ethical and cultural differences.

The remaining essays in the volume are distributed across four sections that address, respectively, Habermas's extended exchange with John Rawls, questions of citizenship and nationalism raised by (among other things) the reunification of Germany, the status and force of international human rights, and problems with deliberative conceptions of politics. In all these essays Habermas confronts what he takes as the unavoidable, unprecedented religious, moral, ethnic, and cultural pluralism of contemporary societies (p. 117). His aim is to offer theoretical resources that might "sharpen sensitivity to the diversity and integrity of the different forms of life coexisting within a multicultural society" (p. 225).

Habermas first contrasts his own discourse theoretic approach to this problem with the "political liberalism" that Rawls defends. He charges that in the latest development of his thought Rawls abandons the terrain of "a universally binding practical reason" and instead relies on the fortuitous "convergence of reasonable worldviews" to sustain an overlapping consensus on justice (pp. 82-3). The problem with this, as Habermas sees it, is that the parties to any overlapping consensus might, as observers, agree that they have attained such convergence, but their diverse individual ethical, religious, aesthetic, and political commitments nevertheless remain opaque to one another. Within the Rawlsian "political" conception of justice, the inhabitants of an overlapping consensus are under no pressure, as participants, to defend any "metaphysical" position that they might embrace. Each simply must ascertain whether the consensus, in fact, accommodates their own comprehensive metaphysical doctrine (p. 91). By contrast, Habermas argues that the validity claims we necessarily raise in communicative action, just insofar as they are criticizable and so demand defense, press us to justify our commitments through discourse. He thus contends that, unlike the position Rawls advances, his own discourse theoretic approach "does not bracket the pluralism of convictions and worldviews from the outset" (p. 59).

From his engagement with Rawls, Habermas proceeds to more directly practical, political issues. He first turns his attention to problems of European nationalism. He offers an historical vignette intended to establish that within European nation states citizenship has encapsulated both a legal status defined by rights established in positive law and a sense of

cultural membership. Habermas argues that these components of citizenship coexist uneasily and that our contemporary confrontation with ethnic, linguistic, and religious diversity underscores that tension. Habermas notes that a common response to this tension is to treat cultural membership as "the prepolitical fact of a quasi-natural people" (pp. 115, 219), and he reminds us that recent experience affords numerous examples of the dangers that attend any such interpretation. In the face of such dangers he rightly thinks it imperative that the rights of citizens be disentangled from any claim to membership in a homogeneous, prepolitical group. He advocates a discursively grounded "constitutional patriotism" in which members of each nation state elaborate 'a distinctive interpretation of those constitutional principles that are equally embodied in other republican constitutions—such as popular sovereignty and human rights—in light of its own national history" (pp. 118, 225). He suggests that a minimal core of any such accommodation very likely will be solid recognition of social and cultural rights in addition to less controversial liberal rights of individuals.

This last claim raises the question of whether the sorts of collective security and identity that such measures are meant to provide can "be reconciled with a theory of rights that is individualistically designed" (p. 204). Habermas insists that only an unsupportable "selective reading of the theory of rights" (p. 210) can sustain the view that collective rights to economic security or cultural identity are fundamentally incompatible with the traditional liberal preoccupation with individual rights. His claim is based on the observation that the autonomy of individuals to whom we ascribe rights itself necessarily trades upon intersubjectively recognized collective identities (pp. 207–8, 221). And these identities, in turn, must be understood (as above) not as prepolitical social facts but as the product of discursive political interpretation and deliberation.

We thus readily can see why Habermas aims to make a "proceduralist conception of deliberative politics the cornerstone of the theory of democracy" (p. 246). It is only under procedures that insure inclusive, free, and equal participation that collective identities, political traditions, or legal enactments might be justified to diverse populations who inhabit contemporary polities. It is crucial to recognize this, according to Habermas, if we hope to specify a conception of democratic institutions that might simultaneously include now marginal constituencies without absorbing them into a culturally homogeneous polity (p. 139). And that, he rightly insists, precisely is the challenge that confronts those who pursue democratic politics in multicultural societies.

Reconstituting Social Criticism: Political Morality in an Age of Scepticism. Edited by Iain MacKenzie and Shane O'Neill. New York: St. Martin's, 1999. 217p. \$65.00.

Wayne Gabardi, Idaho State University

The eleven essays in this volume are the product of a conference held in June 1996 at Queen's University, Belfast. As the title indicates, the theme was the current status (O'Neill refers to it as "the contemporary crisis," p. 1) of philosophically grounded social criticism in a time of postsocialism, neoliberal globalization, and postmodern skepticism. As O'Neill makes clear in the Introduction, the Left must regain its theoretical self-confidence and advance a powerful, alternative progressive vision to the present order of things.

O'Neill's introductory essay does a good job of defining the challenges facing contemporary critical theorists and summarizing the work of the eleven contributors. The essays are

lucid, topical, significant, and short. All these qualities are virtues. The essays also offer a rich variety of alternative theoretical strategies for advancing progressive social and political critique. They strive to clarify and modify the work of Marx, Walzer, Foucault, Castoriadis, Rorty, Habermas, and Rawls, among others.

This book is divided into four sections. The essays by Simon Caney, Keith Graham, and John Baker advance arguments in defense of moral universalism as a normative foundation for social and political critique. Those by Aletta J. Norval, Jon Simons, and Caroline Williams reaffirm the validity of contextual critique and the poststructuralist contesting of boundaries. The contribution by Nicholas H. Smith sorts out the positions of Richard Rorty and Jürgen Habermas. Norman Geras revisits the Holocaust in order to illuminate that "a contract of mutual indifference is a reality in the contemporary world" (pp. 10-1). The concluding essays, by Richard Bellamy, Maeve Cooke, and Thomas McCarthy, deal with the task of negotiating normative critique in a time of cultural pluralism from the respective perspectives of liberalism, deliberative democracy, and neo-Kantian conceptions of cosmopolitan justice. The essays by Norval, Simons, Smith, Cooke, and McCarthy speak most directly and significantly to the book's stated challenge of reconstituting philosophically grounded social criticism at the present time.

Norval draws upon Derrida's thesis of "iterability" to bolster Walzer's account of thick and thin moralities. Walzer has argued that morality is thick (deeply embedded in history and culture) and thus critique must be immanent and interpretive, but local critique can become global through an existential or crisis encounter in which a thin, universal morality emerges out of the experiential interchange of cultures. The problem, however, is that Walzer conceptualizes cultures, and therefore moralities, as bounded, intact, univocal voices (a closed "we"). Derrida's thesis of the repetitive alteration of a constructed identity or sign allows us both easier movement between thick and thin narratives and a greater appreciation of the complexity of situated and reconstituted critique.

Simons employs Fredric Jameson's idea of aesthetic "cognitive mapping," Foucault's critical genealogies, and the work of Virginia Woolf to make the case for "fictive theory." Unlike critical theory, which is committed to epistemological foundationalism and emancipatory social scientific knowledge, fictive theory employs aesthetic cognition in developing critical narratives and images that can contest existing power networks. Simons makes a compelling argument that an aesthetic politics is often more effective and resonates better with its interpretive audience or community of action than does critical theory's appeal to epistemological rigor and "the force of the better argument." I agree. In a postmodernized world, the pragmatic power of fictive representations and symbolic politics allows us more scope for effective action.

Smith tackles the attempts made by Rorty and Habermas to advance new modes of critique while acknowledging contingency. In the case of Rorty, the human condition is structured by radical contingency. In the case of Habermas, the formal structures of speech and moral claims about justice or "right" transcend contingency. Smith's insightful exposition and critique of both positions leads him in a thought-provoking direction. Smith reminds us, and Rorty, that a commitment to radical contingency in all things can undermine both the pursuit of self-creation and social solidarity. As for Habermas, his "postmetaphysical" model of procedural-discursive reason results in both a too narrow and too demanding idea of a philosophically constituted social

criticism. By contrast, Smith opts for a greater appreciation of "ontological reflection" linked to substantive ethical orientations. Although Smith does not develop this approach, significant work has been done in this area by Fred Dallmayr.

Contending that ethical conflicts are reconcilable in principle, Maeve Cooke makes the case for a modified version of Habermas's model of deliberative democracy. Cooke believes that substantive ethical values can be given more weight in relation to procedures of moral justification and that the regulative ideal of rational consensus needs more slack. Despite its weaknesses, Habermas's model of democracy is better than those advanced by postmodernists because the latter lead us in the direction of a privatization of ethical matters. On this point I find Cooke unconvincing. As I read William Connolly and Chantal Mouffe, their postmodern models of radical democracy do not, in principle, result in an ethos and politics of privatization.

McCarthy, convinced that the time is ripe for rethinking cosmopolitan ideals, turns to the theories of justice advanced by Rawls and Habermas to illuminate this conviction. Rawls's recent attempt to develop a theory of international justice in "The Law of Peoples" (Critical Inquiry 20 [Autumn 1993]: 36-69) is a disappointment for McCarthy. In order to accommodate "well-ordered, non-liberal societies," Rawls's liberal ideas of justice and "reasonableness" undergo "considerable dilution" (p. 197). McCarthy concludes that Rawls jettisons distributive and democratic concerns and reduces civil liberties to a bare minimum in order to make justice acceptable to nonliberal societies. The result is cosmopolitan but not very just. McCarthy concludes with a very brief sketch of "an alternative route" (p. 207) closer to the spirit of Kant and Habermas and designed to accommodate both multicultural cosmopolitanism and a legal-political structure of human rights and global federalism. If one is searching for a much more sophisticated neo-Kantian account of cosmopolitan justice, however, I would recommend David Held's model of cosmopolitan democracy in Democracy and the Global Order (1995).

One section is missing in this fine collection of essays. This would map out the existing power structure to which all these critical theories and alternative models of criticism are opposed. Not one essay seeks to give us an idea of what our current postsocialist, neoliberal, globalized condition looks like. Should not this be our starting point in the development of critical theories and progressive political agendas?

Capitalism, Democracy, and Ralph's Pretty Good Grocery. By John Mueller. Princeton, NJ: Princeton University Press, 1999. 335p. \$29.95.

Paul Cammack, University of Manchester

Mueller wants us to ask less of democracy than some of its keener advocates have promised; to value capitalism, particularly as a moralizing force, more highly than it has generally been valued; and overall to accept that the two together provide pretty much all we might need. When the two coincide, we are in Ralph's pretty good grocery, Garrison Keillor's fictitious Lake Wobegon store, where, if you cannot get it, you can probably get along without it. After a brief description of the contrasting "image mismatches" to which capitalism and democracy are subject—capitalism having an unjust negative image, and democracy an unrealistic positive one—four chapters advance the case for capitalism as a force for virtue, and three warn of the dangers of expecting too much of democracy. The book then concludes with a brief discussion of connections and disconnections between the

two and an inventory of seventy-six propositions that helpfully restate the argument. Mueller makes his case in a deceptively folksy style, but at the same time he is explicit about his purpose of creating an enabling myth for capitalism, and he is ruthless in pursuing it. If he is not entirely successful, it is because of the glaring contrast between his treatment of capitalism, on the one hand, and democracy, on the other, a fundamental ambiguity in the case made for capitalism; and a range of reference so narrow and selective that reasoned argument continually gives way to crass boosterism and caricature.

At the close of his brief discussion of the sources of capitalism's negative image (chap. 2), Mueller recalls Paul Samuelson's description of capitalism as an "efficient but unlovable system with no mystique to protect it" (p. 68), and he laments the fact that the virtues of capitalism—honesty, integrity, compassion, and civility (elsewhere supplemented by fairness and heroism)—are rarely stressed by "propagandists for the system as a whole" (p. 69). These passages give the game away. Mueller is out not to present a balanced analysis of capitalism but, rather, to create a noble mystique to support it. He does so first by proposing that honesty pays in business, and all that is needed is for one or two entrepreneurs to realize this and act upon it in order for the rest to learn and prosper. His pioneering heroes, who make early and repeated appearances in the text, are P. T. Barnum, the nineteenth-century showman, and John Wanamaker, the nineteenth-century retailer; their virtue consists in giving customers a square deal. Leaping to the late twentieth century, Mueller claims that governments now understand how to make capitalism work by promoting free trade, leaving wages and prices to find their own level, abandoning enterprises that can be handled by the private sector and allowing those that cannot compete to fail, keeping regulation to a minimum and taxes low, and accepting a considerable amount of economic inequality (pp. 114-22). In these circumstances capitalism can flourish if only people will learn to love it. Mueller's contribution is to create folk heroes from the past to provide a supporting mystique for capitalism in the present.

This beguiling construction is undermined, however, in two ways. First, Mueller equivocates, arguing that "capitalist morality," being nothing more than hard-headed business sense, is not strictly speaking morality at all but calculation, and he escapes with the lame proposition that it is nevertheless only convincing when sincerely espoused. Second, he backtracks, arguing at times not that capitalism's virtues are bound to assert themselves but that they would assert themselves if only capitalists could see their utility. This strengthens the case for propaganda but only by weakening the supposed causal mechanism that necessarily generalizes the virtues of capitalism in the first place.

In stark contrast to this romanticized and idealized depiction of capitalism, the chapters on democracy take a diametrically opposed approach. Mueller attacks the image of participation and equality, captured in a Norman Rockwell painting of a working man putting forth his point of view in a New England town meeting, as a "misty-eyed, idealized snapshot" with "almost nothing to do with democracy in actual practice" (p. 137). He mercilessly debunks "democratic myth-builders" who have "chosen to emphasize the occasional political success of upstarts raised in log cabins" (p. 150), and he dismisses democracy's supposed virtues in favor of the proposition that it is "essentially a governmental gimmick" that has succeeded as a result of astute marketing (p. 203). Mueller then argues that democracy is a system in which apathy is to be applauded and in which the play of

special interests in petition and lobbying, in which those with greater material resources necessarily win out, is the "whole point" (pp. 153, 171–85). These are familiar themes in empirical democratic theory, but even so Mueller is unceremonious in the extreme, questioning the need for elections and scorning Dahl and others for suggesting that new mechanisms should be devised to enhance popular participation.

In sum, Mueller is out to puncture a myth associated with democracy and to inflate another associated with capitalism. The more or less conscious purpose of the whole proceeding is to make capitalism the direct source of virtue and reduce democracy to what contemporary capitalism can bear. For my money, the effort is too transparent and too contradictory to convince. In any case, Mueller himself has to concede at the close that his version of democracy and capitalism may lack an answer to the great philosophical themes of truth, the good, and the meaning of life (p. 240). He responds, with Voltaire's Candide, by shrugging and proposing to cultivate the garden. Were he to venture into it, he might meet an individual who does not figure in the text but who hovers over it throughout—the unlucky Dr. Pangloss, whose perpetual response to the horrors of the world was the dogged insistence that everything in it was ordained for the best.

Barbarism and Religion, Vol. 1: The Enlightenment of Edward Gibbon, 1737-1764. By J. G. A. Pocock. Cambridge: Cambridge University Press, 1999. 323p. \$49.95.
Barbarism and Religion, Vol. 2: Narratives of Civil Government. By J. G. A. Pocock. Cambridge: Cambridge University Press, 1999. 413p. \$49.95. Two-volume set \$90.00.

Robert Booth Fowler, University of Wisconsin-Madison

These two substantial volumes are part of J. G. A. Pocock's projected multivolume exploration of the thought of Edward Gibbon (1737–94), the still controversial author of the even longer Decline and Fall of the Roman Empire (1776–88). Pocock devotes so much effort to Gibbon because he believes Gibbon was a "very great figure" (Vol. 2, 402), although exactly why he makes this judgment is not especially clear in this study.

In volume 1, Pocock endeavors to situate Gibbon in terms of his intellectual journeys—both geographic and mental—during his younger years. This is absolutely essential for Pocock's entire project, since he believes that Gibbon and his work cannot be understood unless they are explained contextually. Pocock succeeds splendidly in this effort, constructing a picture of the worlds of the young Edward Gibbon of considerable nuance and texture.

Less interesting is his other objective in volume 1 (and volume 2 as well), his determination to show that there were several Enlightenments, not just one. Pocock makes this case skillfully, as always, but this claim is now so widely shared that it is not obvious why it is worth his while to make it.

In volume 2 Pocock undertakes to set the Enlightenment intellectual scene for *The Decline and Fall of the Roman Empire*. He does so by exploring in characteristic depth other historians in the Enlightenment. The result of this effort is a marvelous, remarkable achievement. In the masterful manner we know to expect from Pocock, one which reflects the "complexity" (Pocock's favorite word) of his subjects, Pocock considers the major Enlightenment historians who were contemporaries of Gibbon. Some, such as David Hume and Adam Furguson or Voltaire and D'Almbert are common

currency in Enlightenment studies; others, such as Pietro Giannone or Freret, are much less so. In each case, though, Pocock provides a similar serious treatment, usually several chapters long. His discussions are models of judicious presentation of the historian's work and thoughtful analysis of his ideas and historical context.

Along the way, Pocock expresses gentle admiration for these (and other) Enlightenment historians, however they proceed. He consistently shows an understanding and respect for other scholars, past and present, that is generous and never detracts from his commitment to serious analysis and reflective judgement.

Pocock's volumes are least warm to Voltaire and his histories. In part, this is because Pocock's narrative of Gibbon involves the story of Gibbon's rejection of Voltaire's method of historical study. But it is not only Gibbon who is critical of Voltaire. Pocock obviously shares Gibbon's negative conclusion. Gibbon rejects Voltaire's history as "superficial" (Vol. 1, 153), flashy and unsubtle, too committed to history as a species of grand philosophy, reflecting Voltaire's too easy radical skepticism (about most anything or everything), and Pocock evidently agrees.

In these volumes, Gibbon emerges as someone who over time learned the demanding craft of writing history, in a manner that turns out to be close to that which Pocock most respects. Thus, these two magnificent volumes are not, one soon appreciates, just about Gibbon and his intellectual context or the historiography of the Enlightenment historians. These tasks are central for Pocock, and they are wonderfully achieved. But in Barbarism and Religion Pocock has also written a major treatise on historical method, especially regarding the history of ideas. Granted, Pocock honors his well-known avoidance of ideologically driven and presentist scholarship in this work. But he hardly hides his affection for Gibbon's project, because he and Gibbon share much in common in their methodology.

This methodology focuses, above all, on erudition, which for Pocock seems to mean a deep and serious scholarly engagement with authors' works and with their intellectual and cultural contexts. It also involves commitment to accurate and lively narrative history as well as a degree of overall philosophical exploration and unity. It always includes a profound appreciation for complexity and irony in history. For example, Pocock makes a major point of insisting that the involvement of the Anglican Church in the English Enlightenment involved a complexity both in terms of conflicts within the Church of England and with the larger state and society that simply cannot be grasped neatly as too many Enlightenment discussions try to do. And he lauds Gibbon because he loved the ironies of history and of his own history while he wisely rejected "following philosophy at the expense of facts" (Vol. 2, 157). For example, Gibbon perceived the irony revealed by close study of early popes that they were often friends as well as enemies of human freedom (Vol. 2, 71).

As yet another example of Pocock's laudable scholarly studies, the two volumes of Barbarism and Religion offer the author's fellow scholars a great deal: a study of Gibbon in his intellectual and personal contexts, a splendid discussion of historians in the Enlightenment, and a pointed discourse on method in history. While this work is hardly for the general reader, Barbarism and Religion is a reassuring reminder that serious historical scholarship continues to appear and inform us. And it is yet more evidence that J. G. A. Pocock is a master of this increasingly precious art.

Equality, Responsibility, and the Law. By Arthur Ripstein. New York: Cambridge University Press, 1999. 319p. \$54.95.

Mark A. Graber, University of Maryland

Legal systems are concerned both with standards of responsibility and with allocating the consequences of luck. Whether a person is held liable in tort for dropping a hammer depends partly on whether the person should be blamed for dropping the hammer and partly on whether the hammer fell harmlessly to the ground or hit another person. Sometimes persons who shot at what they believed was a deer and hit a human being are punished as severely as persons who shot at what they believed was a human being and hit a deer. Distributive justice presents similar mixed questions of responsibility and luck. Americans debate both what constitutes "unearned" income and whether such income should be taxed more than earned income.

Arthur Ripstein has written an important work on the interaction among responsibility, luck, and justice. His study details and largely defends the circumstances under which present Anglo-American law holds persons hostage to fortune. Equality, Responsibility, and the Law maintains that the law uses, and should use, objective reasonableness standards in those instances "where luck has a role to play-accidents, mistakes, emergencies and attempts" (p. 6). "Rather than asking everyone to expend the same degree of effort," Ripstein insists that "the law demands the same degree of care from everyone and protects all to the same degree" (p. 86). Criminal liability does not depend on whether I shot at what I thought was a deer or even if I did my best to ensure that my target was a deer, but on whether a reasonable person would have thought the target a deer. Reasonableness standards similarly determine tort liability and the resources necessary to live a responsible life.

This objective reasonableness standard is necessary to preserve the "fair terms of interaction" (p. 105). Subjective tests are unfair because they make the rights of persons depend on the particular capacities of other persons. Whether A consents to a trade would depend entirely on whether B thought A consented. This unfairly makes "one person's security depend on another's state of mind" (p. 105). Similarly, the law should not obligate persons to perform easy rescues because such a rule "would make the security of those in peril depend on considerations about the welfare of those positioned to rescue them" (p. 92). Rather, Equality powerfully argues that the law treats people as equals only when "the line between the choices for which a person may be held responsible, and circumstances for which he may not, is set objectively" (p. 266). Persons are negligent when they fail to take reasonable precautions against injuring others. Persons are excused from crime when a reasonable person would have made the same factual mistake. Ripstein does go too far when he claims that "the criminal law serves primarily to protect and vindicate fair terms of interaction" (p. 134). Most societies punish some "victimless" crimes. Most people do not think the main fault with murder is that victims are deprived of the right to choose suicide.

Equality will serve as an excellent guide to the issues of responsibility central to much tort and criminal law. Ripstein has mastered the law and scholarship on these subjects, writes clearly, and offers intelligent solutions to important legal problems. Still, his arguments are hardly airtight. Objective standards are no more equal in the abstract than subjective standards. Some difference theorists maintain that persons are treated equally only when they are treated differently. This view may be wrong, but it is never discussed

in the book. Moreover, contrary to Ripstein's claims, one person's security always depends on the mental state of other persons. All we can do is our best. Telling us that we will be held liable in tort or criminally punished if our best does not meet a reasonableness standard is not particularly helpful in the absence of any specific public understanding of what acts are reasonable. Equality rarely discusses what acts are reasonable, or whose rationality a reasonableness standard at different times and in different places is likely to privilege.

Ripstein does do a nice job of demonstrating the close connections in the way tort law, criminal law, and distributive justice understand the relationship between luck and responsibility. His study demonstrates that what may seem obscure questions of common law raise fundamental issues of justice. In particular, Equality recognizes that legal liability is only meaningful when principles of distributive justice are satisfled. "If someone's opportunities are severely limited . . . by poverty or ill health," Ripstein declares, "it is unfair to hold him responsible for the costs of his choices" (p. 272). The chapter on distributive justice then provides an interesting exploration of the nature of the social safety net. The book does not, however, examine at length the extent to which persons who start with scarce holdings are excused from legal liability. Readers do not learn whether the desperately poor may steal a loaf of bread or the desperately ill may illegally use marijuana to ease their pain.

Equality, Responsibility, and the Law will interest more academic lawyers and philosophers than political theorists and political scientists. This is a fair description of the book the author wrote, not a complaint that a professor of philosophy and law should have written a different book. The main body of the work is devoted to an analysis of specific problems of tort and criminal law. Only the last two chapters discuss those broader issues of social organization that concern students of politics. The literature discussed and critiqued is largely legal and philosophical. Distinctively political science perspectives on tort and crime are not considered. Ripstein does recognize that problems of implementation are central to a just society, but he asserts that these issues are not his present concern. Thus, readers will not find out how objective a reasonableness standard enforced by juries is likely to be. The redistribution schemes necessary to ensure the fair terms of interaction are also beyond the scope of Equality. For these and numerous other reasons, the book is not likely to be assigned in many undergraduate or graduate political science courses. Still, good books do not have to be good political science books, and political scientists who read nothing but political science are likely to have little to say to the vast majority of persons who are not political scientists. Scholars interested in "a principled account of where the results of luck properly lie" (p. 2) will find Equality, Responsibility, and the Law an interesting, provocative read.

The Problem of Trust. By Adam B. Seligman. Princeton, NJ: Princeton University Press, 1997. 231p. \$29.95.

Steven M. DeLue, Miami University Ohio

As Adam Seligman points out, recent discussions of civil society in the work of Robert Putnam and Francis Fukuyama emphasize trust and civic engagement as critical to securing both economic flourishing and regard for individual agency. The basis for trust here is the existence of social roles, the norms attached to them, and the confidence that individuals will conform to each of these norms. But for Seligman, since the spaces in which individuals often live extend beyond the

reach of particular roles and the norms attached to them, trust is more enigmatic for civil society thinking than is portrayed in recent thought on this subject.

Following Durkheim, Seligman accepts the view that in a highly complex division of labor each individual is a particular agent who makes her own choices about the best way to navigate her life through role diffusion. But how is social stability maintained when individuals have the freedom and indeed the obligation to choose their own life plans in the midst of such role-complex differentiation? Others, as they look at me overriding the restraints and boundaries of various social roles during the course of the pursuit of my own agenda, may conclude that I have no commitment to limit my freedom in deference to their own. When this mentality is multiplied throughout society, the bonds of social cohesion are challenged.

Durkheim's approach to sustaining social stability was grounded in the view that the shared values attached to social roles restrict conduct to clear boundaries. These norms provide the basis for having "confidence," as Seligman uses the term, that others will live within expected restraints as they exercise their freedom. As people manifest confidence in one another's role compliance, stability throughout society is secured. But for Seligman, Durkheim fails to address how in a role-diffuse world autonomous agents seeking to cement their own identities upon their own choices cope with the risks associated with the exercise of their freedom. If I want to be a lawyer, musician, and family man, then I have to make decisions about how to marshal my capacities. But in trying to excel in so many different roles, I realize that I am vulnerable to those who demand conformity to this or that role. Indeed, my risks are many. I may lose my job as a lawyer, be ridiculed for my music, or be labeled a bad father by my children.

What sustains me throughout this endeavor is the trust I have in those who support my decisional autonomy and whose help allows me to make the best use possible of my various capacities as I negotiate my life across different roles. The kind of trust that serves this end is only found in the private sphere. The relationship most fundamental to securing freedom in the private sphere for Seligman is friendship, which evolves from trust among people who care enough for one another to make decisional autonomy possible. The location of the private sphere is found in the interstices existing between our diffuse roles. In these spaces we are free to build, outside the administration of role regimens, friendships with others, and through these friendships we are empowered to make our own judgments about the best course for ourselves in a role-diffuse world.

Civil society from Seligman's point of view is predicated since the Scottish Enlightenment tradition on those characteristics of personal integrity that secure trust. Seligman expertly outlines this tradition and demonstrates the way in which it helps shore up the private space that makes possible freedom without undermining trust or spawning social instability. Seligman's chapter demarcating the civic tradition from the civil society tradition succeeds in demonstrating the reasons for the demise of the former in deference to the latter. The civic tradition of Rousseau, for instance, turns individuals into citizens who subordinate their private yearnings to the society's collective, shared values. But for Seligman, the civic tradition cannot survive in a role-complex world in which, as a result of role differentiation, different and competing values become the norm, pushing shared values to the background. Civil society houses a private realm in which people maintain sufficient trust to permit individuals the freedom to negotiate their diverse roles in life, without cost to freedom or to the bonds needed for social stability.

Still, trust is fragile in the modern world. Why is this the case? In Seligman's account, social stability is threatened when role diffusion undermines shared core values. Trust is viewed as the alternative glue that holds together a society destabilized by the loss of common shared values. But, ironically, trust rests on shared core values, too, and once these values are undermined, so is trust. Seligman says that where forms of social solidarity based on shared values are no longer possible, "the individual as [the] basis of personal identity may disappear. In this disappearance the preconditions for trust do themselves disappear as well" (p. 172).

Seligman's argument would have been strengthened if he had addressed the irony of his own account of trust within a civil society setting. If trust can survive only in the presence of certain core values, what in fact is the nature of these values? Will those connected to the Scottish Enlightenment tradition, such as sympathy, do? Moreover, what is the role of the public realm in securing the bases for trust? The public realm often functions in Seligman's account to repair the damage to society from the loss of trust. The public realm provides necessary cohesive ties through bureaucratic programs designed to achieve full equality, often at the cost of the freedom needed to establish individual identity. To compensate for this loss, people substitute for trust an alternative cohesive force—group identities based on such characteristics as race, gender, and sexual orientation. Here, a particular group identity is given priority over the quest for individual agency protected by civil society.

None of what I have written is meant to detract from the contribution of this book to civil society thinking. Through a richly textured synthesis of major sociological and political theorists, Seligman demonstrates why trust is an issue for the modern person who finds herself negotiating an ever more differentiated and complex set of roles. Seligman's discussions contrasting faith and trust as well as his treatment of the evolution of conscience and the construction of the private sphere within Christian thinking are especially important contributions. Those who wish a treatment of civil society that incorporates trust would do well to read this book.

Sovereign Grace: The Place and Significance of Christian Freedom in John Calvin's Political Thought. By William R. Stevenson, Jr. New York: Oxford University Press, 1999. 200p. \$49.45.

H. M. Höpfl, Lancaster University

The main value of this book is that it is a "whole gospel" account of Calvin. Stevenson writes as a disciple and an exegete of Calvin from within the authentic Calvinist tradition. His work is therefore a valuable corrective to the usual accounts of Calvin written from the outside (so to speak), to say nothing of interpretations based exclusively on reach-medowns, procrustean generalizations, periodizations, ideal typifications, and all the rest. Stevenson's exegesis is neither apologia nor historical interpretation but simply the exposition, largely within Calvin's own vocabulary and horizon, of the coherence of Calvin's thought. Stevenson's tentative attempts to "sell" Calvin to a modern public do not try to modernize him or even to extenuate some of his more repellent doctrines (notably "double predestination" or the intolerant aspects of the Calvinist polity). Stevenson merely explains why to Calvin such doctrines connoted no arbitrariness or injustice on the part of God. For him, the truth and scriptural authenticity of Calvin's faith are entirely unproblematic: The Christianity of sacraments, contemplation, tradition, and the Church's magisterium only appear in terms of the construction placed on it by Calvin if at all. Stevenson is not fazed by the objections in terms of the psychological precariousness of faith understood as the believer's sense of certitude, confidence, and trust, a difficulty first raised long before Weber by sixteenth-century Catholic critics. He acknowledges freely that the Christian will look for external signs of his salvation, and he restates without equivocation the Calvinist notion that the providence of God is to be discerned by scrutinizing the course of events in this world.

Stevenson is, of course, not a know-nothing fundamentalist. On the contrary, he imperturbably and generously acknowledges everything modern scholarship since Weber has offered; the only acerbity is toward Voegelin and Walzer, whom Stevenson suspects of not having read Calvin; if anything, he takes too much notice of other scholars, with tortuous diversions via expositions of their contributions before he returns to his own. In other respects, the book is reader-friendly, providing prolegomena and summaries all along the way.

Stevenson's thesis is that Christian freedom is the (or a) buckle that joins Calvin's theology and his political thought, and it reconciles apparently contradictory tendencies in his thought and one-sided interpretations based on them. Stevenson does not claim that the doctrine is the center of Calvin's theology, only that it is a more significant and integral component than is generally appreciated. He also makes some more general claims for the doctrine, especially that it reveals the superficiality and shallowness of modern individualistic, communal, and historical-developmental conceptions of freedom. But he admits that he has demonstrated nothing of the kind (p. 151), and his postulate of perennial questions to which Calvin is all or part of the answer could only convince the Calvinist faithful. The substance of his argument about the place of Christian freedom in Calvin's thought seems to me ultimately unconvincing for precisely the reason Stevenson cites for regarding it as central: Namely, that unlike his political theory, ecclesiology, or the doctrine of predestination, Calvin never reconsidered, elaborated, or amended the formulation he gave it in the first (1536) edition of the Institution of the Christian Religion; in other words, he felt that there was no more to be said about it. Nevertheless, Stevenson convinces at least this reader that much of Calvin's political thought as well as his theology can be summarized in terms of what he says about Christian freedom, and that his discussion of it was not merely a matter of finding a way of accommodating an uncongenial item in the reformational inheritance which he could not jettison, coupled with an attempt to neutralize its anarchic potential.

On Stevenson's more nuanciated reading, Calvin's Christian freedom is freedom from punishment and from subjection

tion to human institutions, for obedience and willing service, in world-transforming corporate action, without subjection to indifferent things, adiaphora, such as local custom, tradition, ceremonies (described here anachronistically as "the tutelage of history"). Given this understanding of the situation of the redeemed Christian, it can be seen how faith (entirely unmerited certitude of salvation by "sovereign grace") will generate a certain individualism (in some respects resembling liberal individualism), activism, possibly revolutionary activism, and certainly a distanciated attitude to cultural artefacts and other time-bound things, such as custom, tradition, and established authority. This terminology is indiscriminate, and Stevenson's repeated recourse to the methodologically infelicitious concepts of "contribution" and "anticipation" leave uncertain what relationship he means to assert between Calvin and later thinkers and currents of thought: Sometimes it seems to be some idea of "influence" of the former, as when he implausibly speaks of the "Calvinist roots" of "modern activism" (p. 103).

But Stevenson is clear enough about the distance between Calvinian "individualism" and "activism" and its later analogues (e.g., pp. 56, 63); and although he does not reject the concept of revolution as anachronistic for the sixteenth century, he is properly skeptical about the term's appropriateness to Calvin. What he is concerned to stress is that these moments in Calvin's thought and teaching are held in check by other moments, notably the recognition of the individual's dependence on God, the duty of love and consideration for weaker brethren (a concept not explained here) and a signal illustration of his providence, and the collective character of Calvinist endeavors: building up the Church is not a private enterprise. Thus, the "authoritarian" Calvin and Calvin the ancestor of the liberal individualists, of constitutionalism, and of resistance to tyranny are equally authentic, but the tension between them is inherent in the biblically gounded notion of sovereign grace and is one that Calvin deliberately stressed and cultivated.

The weaknesses of this work are mostly the obverse of its strengths. The interpretation considers almost no context for Calvin apart from Scripture, and its *genuinus sensus* of which Calvin is the authoritative expositor at that. The contrast Stevenson draws between Calvin and Luther's understanding of the Law (pp. 45–6) is unsatisfactory, and Calvin's relationship with Luther and other reformers is not explored. Stevenson also does not consider whether it is plausible to present Calvin's reflections about the secular commonwealth as solely inferences from scriptural Christianity, moderated by pastoral concerns. But alternative interpretations are available in plenty elsewhere (and may be found in his excellent bibliography).

American Politics

Shaping and Signaling Presidential Policy: The National Security Decision Making of Eisenhower and Kennedy. By Meena Bose. College Station: Texas A&M University Press, 1998. 197p. \$29.95.

Kevin V. Mulcahy, Louisiana State University

The reputations of Eisenhower and Kennedy have, at least in professional circles, undergone an amazing transformation. The former was dismissed (incorrectly) as a disengaged manager and uninformed figurehead, and the latter was often

lauded in journalistic circles as the modern model of a president: a personally engaged and highly interventionist chief executive. The last two decades have witnessed a rehabilitation of Eisenhower's reputation for presidential leadership. This process was aided by a good biographer in Stephen Ambrose and by Fred Greenstein's The Hidden Hand Presidency (1982), which argued the case for a more nuanced assessment of Eisenhower's public persona and decision-making processes. As might be expected from a study that began as a doctoral dissertation under Greenstein's direction, Meena Bose's Shaping and Signaling Presidential Policy makes a significant contribution to our

understanding of the policymaking process in the 1950s and 1960s.

Five aspects of Bose's work are noteworthy. First, it is well grounded in the archival sources, which (as the author observes) can be daunting to navigate. Second, Bose has interviewed several of the surviving members of the Eisenhower and Kennedy national security apparatus. Third, the study is well organized and clearly written. Fourth, its explicitly comparative approach allows for broader generalizations than is usual with case studies. Fifth, the analysis is grounded in the theoretical literature on presidential decision making, particularly on Alexander George's concept of "multiple advocacy," with the national security adviser as "custodian-manager" of a process of formalized disputation.

Essentially, Eisenhower and Kennedy represent two competing concepts of national security policy and two contrasting processes for its formation. The "New Look" defense budget promulgated by the Eisenhower administration was designed to minimize excessive military spending through adherence to a strategic doctrine that relied heavily on nuclear deterrence. Sometimes described as the "biggest bang for the buck" policy, the New Look was meant to provide a nuclear umbrella that would contain any Soviet expansionism during a long-term Cold War without an economically debilitating level of military spending. Long a critic of the Eisenhower administration's defense policy, Kennedy argued that "we have been driving ourselves into a corner where the only choice is all or nothing at all, world devastation or submission" (p. 49). Accordingly, he called for a buildup of conventional as well as nuclear forces. According to Robert McNamara, Kennedy firmly believed that the United States needed a "flexible response, a response tailored to the specific level of political or military aggression" (p. 53).

Eisenhower and Kennedy also differed sharply in their processes for national security decision making. The so-called policy hill constructed in the Eisenhower administration comprised a planning board to debate alternatives for eventual determination by the National Security Council and the Operations Coordinating Board, to monitor policy implementation. Often derided as excessively "formalistic," this decision-making process was designed by Eisenhower "to bring a wide range of policy options to his attention" (p. 99). Yet, just as "formalistic" does not accurately describe Eisenhower's decisional process, "collegial" is for Bose an imperfect description of Kennedy's management style. "At least in 1961, those processes were so fluid that Kennedy really did not represent the hub of an informally coordinated advisory network" (p. 100). For Bose, Kennedy's informal leadership style is highly problematic. "While formal processes are not problem-free, they pose less risk of resulting in policy failures, as the president does not make decisions before thoroughly examining policy alternatives with his associates" (p. 102).

Overall, Bose argues convincingly for the importance of structured decision making and institutionalized multiple advocacy; accordingly, Eisenhower's policymaking process is more favorably evaluated. Yet, there is a less convincing case to be made for Eisenhower's communication of policy. Despite Bose's strenuous efforts to characterize Eisenhower's rhetorical strategy favorably, one remembers more his Secretary of State, John Foster Dulles, calling for the "rollback" of Communism, advocating "brinkmanship," threatening "massive retaliation," and fulminating against "godless, atheistic Communism." It is difficult to see any "hidden-hand leadership" here. Of course, Kennedy's rhetoric can be judged far more portentous, "hinting that even the best

efforts of the United States might not enable it to survive" (p. 70). In sum, the second part of this work lacks the persuasive argument of the first, on which Bose demonstrates much firmer footing.

Surprisingly, there are some lacunae in this study. For example, there is no mention of Dillon Anderson, who along with Robert Cutler was Eisenhower's other national security advisor. Furthermore, although the book version of the Jackson Sub-committee report is cited, there is no discussion of the hearings themselves. Yet, these multivolume hearings were the most extensive, contemporary critique of the national security policymaking process. Furthermore, the important work of Alexander George is much in evidence, but there is little discussion of other classic analyses of the national security process, such as Keith Clark and Laurence Legere, The President and the Management of National Security (1969); Anna Kasten Nelson in Hugh Heclo and Lester Salamon, The Illusion of Presidential Government (1981); and I. M. Destler, Leslie Geld, and Anthony Lake, Our Own Worst Enemy: The Unmaking of American Foreign Policy

One also would have wished a more developed model for analyzing the different roles played by national security advisors, as attempted in Cecil V. Crabb and Kevin V. Mulcahy, American National Security (1987). "Coordinators" like Robert Cutler and "counselors" like McGeorge Bundy are the preferred roles, but there have been "agents" like Henry Kissinger and mere "administrators" like Sidney Souers. Bose may wish to go beyond this worthy, if narrowly focused, comparative case study to create a full-scale analysis of the range of advisory structures available to presidents in national security policymaking.

Demanding Choices: Opinion, Voting, and Direct Democracy. By Shaun Bowler and Todd Donovan. Ann Arbor: University of Michigan Press, 1998. 198p. \$42.50.

Simon Hug, Université de Genève

Interest in the instruments of direct democracy (e.g., referendums and initiatives) has increased considerably over the last few years. Their more frequent use at the state level and cross-nationally, as well as the introduction of provisions for initiative and referendum in several new constitutions, largely explains this interest. Debates about the advantages and disadvantages of these instruments of popular decision making often focus on whether citizens are competent enough to make important and often complex policy choices. Shaun Bowler and Todd Donovan address this central issue and provide a fresh, systematic, and theoretically guided look at this core topic.

As Bowler and Donovan correctly argue (p. 9), most of the debate on citizen competence in direct democracy gets quickly sidetracked into normative quarrels about whether particular outcomes of popular votes were "good" or "bad." The authors of *Demanding Cholces* attempt to avoid this normative pitfall by focusing exclusively on "how voters manage the demands imposed on them by the process [of direct democracy]" (p. xiii). Using as a starting point the notion of the "reasoning voter," Bowler and Donovan derive a theoretical framework applicable to voting in direct democracy. Relying on the notions of heuristics and decision cues, they focus on three elements they deem central in the voter's decision: "avoiding uncertainty by voting no, using instrumental evaluations of propositions, and taking cues from political elites" (p. 33).

In the context of elections the "reasoning voter" may rely

on a recurring set of decision cues, which are largely absent in the realm of direct democracy. Issues change from one vote to the next, the campaign environment differs, partisan cues are often absent, and so on. Faced with this challenge, Bowler and Donovan provide empirical illustrations from ballot contests in several states, mostly California. Using both individual-level survey data and aggregate data, they demonstrate in various contexts how citizens employ cues and heuristics in direct democracy. They show that voters, faced with ever longer ballots and increasing informational demands, vote selectively and more frequently "no" on proposals that appear at the end of the ballot. Similarly, economic conditions in a state are sometimes used as cues by voters with lower levels of education, who are more likely to vote against ballot measures when economic times are bad. Bowler and Donovan also report evidence for instrumental voting. The strongest results come from their aggregate analyses, more specifically from votes on prohibition measures. Systematically, counties with higher dry wine production voted more strongly against prohibition at the beginning of the twentieth century. At the individual level, some scant evidence suggests instrumental voting on California's Proposition 13 (tax revolt) among voters with lower levels of education.

Linked to the analyses of the decision heuristics, two chapters focus on initiative logrolling and the effect of campaign spending. Both elements affect the informational demands imposed on voters. An important trend in several states is the appearance of initiatives stitching together various proposals that benefit a series of constituencies. Such distributive logrolls appear to result in instrumental voting among those with high education levels, since they vote more strongly in favor of one such proposal if they live in a targeted area. Some of the other evidence presented on competing ballot measures is less convincing, however. For example, Bowler and Donovan analyze voting on two initiatives on flshing in the Columbia River at the beginning of the twentieth century. Since the initiatives aimed at making fishing almost impossible either upstream or downstream, the geographical distribution of the vote strongly related to the distance from the mouth of the river. But given the aggregate nature of the voting data on these two propositions, it is hard to determine how the voters coped with these competing proposals, and whether they simply decided instrumentally to vote in favor of one and against the other proposal.

On much more solid ground is the argument on campaign spending. Spending decisions by interest groups often reveal a considerable amount of information about the proposals on the ballot. Hence, even for high-spending narrow interest groups it proves difficult to pass an initiative proposal. Such high spending often reveals the nature of the true beneficiaries of particular ballot measures. The effect of high spending seems much stronger, however, in "no" campaigns. Spending on the "no" side often appears intended to increase uncertainty, which, as demonstrated by Bowler and Donovan, may lead voters to cast a "no."

Generally, these analyses on the role of heuristics in votes on ballot measures are of great value. They draw on diverse empirical materials and employ a range of data sources and methods. For the reader interested in the specifics of the empirical cases analyzed, a bit more information would have been welcome. For instance, an empirical model with type of ballot proposal introduced as a series of dummy variables and a constant term should be accompanied with an indication of the omitted category (p. 53). The interpretation of the results would be considerably enhanced. Similarly, some empirical models are probably underspecified (such as county-level

vote on prohibition as a function of religious affiliation and dry wine production), and problems of ecological inferences sometimes are hardly addressed. With respect to the three types of heuristics employed by voters, an interesting addition would have been a joint analysis for a particular vote. Obviously, such analyses are difficult to carry out, given the varied nature of ballot measures.

Nevertheless, *Demanding Choices* provides a thorough and theoretically guided look at the competence of citizens in direct democracy. Fruitful next steps in this wider research agenda would consist of systematic comparisons between voter and legislator competence as well as comparisons of policy consequences of direct democracy. The Bowler and Donovan book, together with such further research, would lead to a much more informed debate between advocates and opponents of direct democracy.

Environmental Injustices, Political Struggles: Race, Class, and the Environment. Edited by David E. Camacho. Durham, NC: Duke University Press, 1998. 232p. \$17.95 paper.

James P. Lester, Colorado State University

There is a burgeoning literature on "environmental racism," or environmental injustice, the thrust of which is that a disproportionate share of toxic harms are born by persons of color and/or the poor. This literature, which has developed largely since the late 1980s, has grown enormously and has passed through several stages. The first wave (roughly during the 1980s and early 1990s) was quite crude and largely limited to case studies of so-called instances of environmental racism. The second wave, beginning in the mid-1990s, is much more sophisticated and increasingly calls into question some of the racist claims of the earlier literature. At present, the literature on environmental injustice reflects the debate over whether environmental racism exists and what should be done about it as far as policy design is concerned.

In this book, David Camacho and his colleagues aim to offer a fresh look at a set of environmental injustice issues and what they believe to be "a more useful perspective on the topic than the empiricist approach common to much policy analysis" (p. 2). Indeed, Camacho asserts "that the distinction between what can be known and measured (facts) and that which is not verifiable (values) is particularly limiting in thinking about and resolving complex social problems" (p. 2). In essence, he invites readers to take a postpositivist journey with him and his colleagues as they attempt to convince us that the causes and consequences of environmental injustice may be located in hierarchies of political and economic power in the United States. According to this logic, "political solutions must be stressed over legal or scientific ones" (p. 4). We are immediately led to believe that this study will not adopt scientific (logical-positivist) techniques of analysis and attempt to reach a policy recommendation based on such criteria as rationality, equity, and/or efficiency; instead, we can expect a normative or political approach to policy analysis.

The editor and contributors do not disappoint. The book is rife with examples of environmental injustices in various contexts, including Arizona, Colorado, New Mexico, Ohio, and Texas. These examples include discussions of Native Americans and women as they confront instances of environmental injustice. Typical of the arguments in this book is the one by Mary Timney, who maintains that "it is irrelevant whether environmental injustice represents conscious racism, or classism, on the part of policymakers, either now or in the past... what truly matters is how the problems are addressed

by policymakers and business interests in the present" (p. 181). Camacho closes the book by arguing that "a change in individual attitudes and behavior is necessary before deliberations begin... This will require strong moral action... [which includes]: (1) direct control of access to ecosystems; (2) economic influence over markets through (a) the levying of taxes, (b) the imposition of fines, (c) subsidies for specified forms of ecosystem usage, and (d) various price controls over market producers" (pp. 210–22).

If one adopts the perspective used by Camacho and his

colleagues (described in the Introduction), then the "evidence" presented in the case studies leads inevitably to the conclusions stated by Camacho and reinforced throughout this book. What is sorely missing is any countervailing evidence that would call into serious question the claims made. Such evidence exists (see Christopher H. Foreman, The Promise and Peril of Environmental Justice, 1998; John A. Hird, "Environmental Policy and Equity: The Case of Superfund," Journal of Policy Analysis and Management 12 [Spring 1993]: 323-43; Douglas L. Anderton, "Methodological Issues in the Spatiotemporal Analysis of Environmental Equity," Social Science Quarterly 77 [September 1996]: 508-15; Vicki Been, "Analyzing Evidence of Environmental Justice," Journal of Land Use and Environmental Law 11 [Fall 1995]: 1-36; Susan Cutter, "Race, Class, and Environmental Justice, Progress in Human Geography 19 [March 1995]: 107-18; James T. Hamilton, "Testing for Environmental Racism: Prejudice, Profits, and Political Power," Journal of Policy Analysis and Management 14 [Winter 1995]: 107-32), but the editor and his colleagues do not cite it. Such an approach is unfortunate because, as Christopher Foreman argues in his recent book, The Promise and Peril of Environmental Justice (1998, p. 117), "the advocates of environmental justice pose a potentially serious . . . danger to the minority and low-income communities [they aspire] to help. By discouraging citizens from thinking in terms of health and risk priorities . . . environmental justice can deflect attention from serious hazards to less serious or perhaps trivial ones."

In other words, if policy remedies are to be developed, they need to be targeted to the appropriate cause; if not, the remedies may not work. Thus, we are at a crucial point in terms of policy designs to alleviate the potential problems of environmental injustice. Policy analyses could help in the formulation of a policy that is just, efficient, and rational, but the authors of this book do not provide them. Instead, they offer their assessment of the problem and attendant solutions without benefit of a careful analysis. Their "solutions," however, might make the problem worse by wasting scarce federal and state resources, which would be targeted on the basis of race or class or politics rather than on the basis of the health and safety risks posed to minorities and nonminorities as well as the poor.

In summary, this book will be useful primarily to advocates of environmental justice who have made up their mind about the nature of the problem and the solution; it will be less useful to policy scholars seeking to understand the root causes of "environmental racism or classism" and the most rational, equitable, and efficient ways to remedy it.

The Boundaries of Blackness: AIDS and the Breakdown of Black Politics. By Cathy J. Cohen. Chicago: University of Chicago Press. 1999. 394p. \$18.00 paper.

Michael K. Brown, *University of California, Santa Cruz*What defines the political interests of African Americans? It is usually assumed that black politics centers on a political

agenda defined by racial issues. African Americans see themselves bound together by a linked fate grounded in a common experience of oppression and mistreatment. This linked fate overrides other divisions among them, notably social class and gender; race comes to be seen as a proxy for individual self-interest and the crucial resource for political mobilization. In *The Boundaries of Blackness*, Cathy Cohen takes dead aim at the notion of a widely shared political agenda at the core of black politics. She states that "where once consensus issues dominated the political agendas of most black organizations, these concerns are now being challenged and sometimes replaced by cross-cutting issues and crises rooted in or built on the often hidden differences, cleavages, or fault lines of marginal communities" (p. 9).

Cohen examines the rupture in black politics by looking at the response of African American opinion makers and political elites to the AIDS crisis, an event that has disproportionately affected African Americans. Cohen is interested in understanding "how groups, in particular African Americans, came to conceptualize [the AIDS] epidemic and their relationship to it" (p. 119). To answer this question she examined the response of black newspapers and magazines; black political organizations, such as the NAACP, Urban League, and SCLC; and the congressional black caucus. Her research is as revealing as it is meticulous. Cohen documents a failure of leadership. Black elites were slow to respond, and when they did, their response was grudging and failed to acknowledge the dimensions of the problem. Eventually, they lent their support to provision of health and social services to those afflicted by AIDS but continued to condemn the life-styles of black gay men and drug users. The AIDS crisis was more or less addressed, but black elites never embraced it as a political issue vital to all African Americans and never took it upon themselves to "transform fundamental thinking in black communities about this crisis" (p. 118).

This distinction between ameliorative and transformative leadership runs throughout the book and is central to Cohen's analysis of black politics. Cohen frames her study of the response to the AIDS crisis with the concept of marginality. Marginal groups are excluded from social and political institutions, stigmatized, and lack control over their lives. Consequently, they must rely on indigenous resources to launch political resistance (pp. 52-3). Problems arise with the internal stratification of marginal groups, that is, a despised minority within a marginal group is ignored, denied resources, and excluded—treated much the way the marginal group is treated. Cohen views this secondary marginalization and its implications for black politics as the real subject of her book. The failure of black leaders to transform the understanding of AIDS matters precisely because it calls into question the underlying premise of black politics. If the assumption of a linked fate and the reliance on indigenous resources serves only one strata of the African American community, then a "reevaluation of black communities' reliance on linked-fate political mobilization strategies" may be necessary (p. 292).

What is one to make of this argument? Assertions that class or gender divisions matter to black politics are commonplace. Cohen's distinctive contribution is to set the analysis of internal stratification of marginal groups within the logic of oppression experienced by such groups. Black elites face powerful demands to conform to values of the white majority, which in turn lead to incentives to regulate or suppress the behavior of group members who might call the credibility of blacks into question. The dilemma is that this undermines the legitimacy of a common black political agenda while leaving black gays and lesbians subject to

mistreatment or neglect not only by white racists and homophobes but also by members of their own group.

This is an important and insightful statement of the fragmentation of contemporary black politics. Yet, two questions should be raised about Cohen's account of the AIDS crisis. Much hangs on how the concept of secondary marginalization is understood and measured. The key measure appears to be the failure of black political elites to transform the thinking of African Americans on the issue or use it as vehicle for political mobilization (pp. 247-8, 253, 267-8, 270, 275, 291-2, 294, 311). Although the black political agenda on AIDS did change—Cohen scrupulously acknowledges the efforts by 1990 of black elites to deal with AIDS—it did not go far enough. It appears that Cohen considers the absence of transformative leadership a form of secondary marginalization. This may be so, but the argument is never clearly made, which leaves the question of how one determines when secondary marginalization occurs or does not occur. This matters if one wants to apply the theory to other groups, as I think Cohen does.

The other question has to do with the transformation of the black political agenda. Cohen raises the important question of how the political agenda of a marginal group changes, be it a racial group, gays and lesbians, or a marginalized religious sect. She appears to believe that political agendas are reshaped by pressure from below, and one of the more interesting parts of the book is her account of the political struggles of black gays and lesbians to change the thinking among blacks about AIDS. But this is not the full story. In fact, Cohen's analysis seems to suggest that AIDS became part of the black political agenda as it became a significant political issue within American society. What is missing from her account is what Hanes Walton (African American Power and Politics, 1997) calls political context. Walton uses this idea to understand how changes in the actions and beliefs of whites shape black politics. Similarly, one might ask how changes in the broader society toward gays and lesbians over the last twenty years shaped the response to AIDS within the black community. A comparison of the black response to AIDS with the white response would have illuminated this

This provocative and thoughtful book will be of interest to anyone concerned with the political dilemmas facing exploited groups, not just African Americans.

Making Democracy Work Better: Mediating Structures, Social Capital, and the Democratic Prospect. By Richard A. Couto, with Catherine S. Guthrie. Chapel Hill: University of North Carolina Press, 1999. 336p. \$49.95 cloth, \$19.95 paper.

Harry C. Boyte, University of Minnesota

Despite the successes of the American economy, many have been left out of the economic expansion. The nation's poverty rate declined during the 1990s, but it has yet to fall below the 12% mark. Poverty is not uniformly distributed. Critics have described the plight of inner cities and rust belt industrial regions. What is less well known is the continuing economic hardship in the Appalachian region of the southern United States. Appalachia is a land known for beautiful fall colors, the Blue Ridge Parkway, and bluegrass music. A strength of Richard Couto's new book is to give the region more human texture. The author describes its continuing poverty. Moreover, building on field interviews and case studies by Catherine Guthrie, Couto also makes a contribution through chronicling the stories of citizen groups who

have struggled in the last generation against poverty, environmental degradation, and other social ills.

Far less successful is Couto's attempt to theorize about these citizen efforts. Following the lead especially of Robert Putnam's famous article ("Bowling Alone," Journal of Democracy 6 [January 1995]: 65–79), Couto uses the concept of "social capital" as his guiding theme. Indeed, his title is a gloss on Putnam's earlier book, Making Democracy Work: Civic Traditions in Modern Italy (1994), which is an interesting treatment of the interplay between civic engagement in voluntary associations and the functioning of regional governments and economic development. Couto seeks to redress what he perceives as weaknesses in Putnam's treatment of social capital, but he misses the forest for the trees. It is the concept of social capital itself that is flawed by the effort to turn into the categories of social science what are better understood as questions of politics.

Making Democracy Work Better has lively treatments of citizen organizations in central Appalachia and adjacent areas. Couto and Guthrie studied 23 groups that the authors call "community based mediating structures," all of which received funding from the Commission on Religion in Appalachia. The commission's explicit grant criteria included the requirement that groups have representation from people who are served and employ strategies for mobilizing citizens. In practice, the commission, a liberal church group, favors groups tilted toward the Left—none of Couto and Guthrie's choices seem to be identified with Christian conservatives, the Republican Party, or the like. But the activist flavor of the groups and the nature of their issues enlist participants with a mix of ideologies. They are better called "populist" than "liberal."

We read about groups such as the Appalachian Center for Economic Networks (ACEnet), which implemented strategies for economic development in eight counties of southern Ohio; it drew inspiration from the Mondragon area of northern Spain and from development approaches in Denmark and northern Italy. Couto and Guthrie also treat groups such as the Bumley Gap Concerned Citizens (BGCC) in Washington County, Virginia. BGCC members waged a five-year battle that was ultimately successful against a plan by the enormous Appalachian Power Company to flood their valley. Many residents' motivations are better understood as conservative than as liberal. "There isn't much of this heritage left in America," explained one (p. 88). Countless community organizations have drawn from similar wellsprings in recent decades, battling the advocates of progress.

Couto and Guthrie hint at the inadequacies of current conventional civic theory, such as the notion of citizen as "volunteer." For example, BGCC organizing was launched by a Presbyterian minister, Richard Cartwright Austin, whose work took on new meaning. Austin was a citizen activist, educator, and organizer rolled into one. "Tve never had a clearer experience of working as a catalyst," he recalled. "Everybody was ready to do something, but no one knew where to start" (p. 86). Small business owners, federal employees, and sympathetic journalists all became involved. What were the new understandings of work that their activism generated? Did the organizing affect their wider institutional cultures? Did their careers continue to have new public meanings? Such questions about the public dimensions of work and institutional cultures are rarely asked in a current theoretical context that locates citizen action in the "voluntary sector" or "civil society" and advances the volunteer as the model citizen.

Couto does not ask these questions either. Instead, he

takes Putnam and other theorists of social capital to task for what he sees as their conservative ideological effects. In his view, social capital as a generator of social trust obscures dynamics of power and patterns of inequality. He seeks to add what he calls political dimensions to the concept, arguing for the idea of "mediating structure" as a force in political struggle. Couto's agenda is unabashedly progressive. The title of an old labor song from the 1930s is his touchstone: "Which Side Are You On?"

Couto's ideological version of social capital is ultimately no more successful than Putnam's effort to diagnose the ills of American civic life in terms of levels of social trust and social networks. In both cases, civic relationships are conceived in instrumental terms. Put differently, the "publicness" of citizens of diverse ideologies, experiences, and perspectives who join together in action on the tasks of a common world, as Hannah Arendt (*The Human Condition*, 1958) would have put it, disappears. With the loss of such publicness, the generativity and spirit of public life erodes, and our sense of politics is radically impoverished.

Interestingly, it is precisely such rediscovery of public action by citizens that has made citizen organizing most interesting, in practice and in civic language alike, over the last generation. To cite one example, the network of large citizen organizations in the Industrial Areas Foundation explicitly takes up projects that cross diverse lines of ideology, class, religious belief, and culture. It articulates the Arendtian idea of care for the world. It sees itself functioning in (and helping to create) a public space of diverse and heterogeneous relationships.

If we are to understand either the discontents or the possibilities of democracy, then we need studies that understand citizens to be more than community members, volunteers, advocates, or ideological partisans. We need a theory that views citizens as multidimensional public actors and a politics worthy of the calling.

Identity and Power: Puerto Rican Politics and the Challenge of Ethnicity. By José E. Cruz. Philadelphia: Temple University Press. 1999. 278p. \$59.95 cloth, \$22.95 paper.

Kim Geron, California State University, Hayward

Demographic changes in the United States are creating a new dynamic as nonwhite immigrants transform the social and political landscape not only in such traditional entry cities as New York, Los Angeles, and Miami but also and increasingly in small and mid-size cities. In response to these rapid population changes, and the ethnic strife in Eastern Europe, some scholars view the promotion of ethnic identity politics, in a multiethnic context, as leading to separation and balkanization. José Cruz challenges this one-sided perspective of ethnic identity. Through an exploration of the political experiences of Puerto Ricans in Hartford, Connecticut, from the 1950s to the 1990s, Cruz finds that identity politics served as "a voice for a marginal group, structuring their agenda, and facilitating their incorporation" (p. 207).

The focus of the book is on Puerto Rican immigrants, who were granted U.S. citizenship early in the twentieth century but were viewed by the local political establishment as the other, similar to other nonwhite immigrants, upon arrival to the mainland in significant numbers in the 1950s. Until the late 1960s, the Puerto Rican community played by the rules of the Democratic Party's local machine politics. As poverty, discrimination, and joblessness grew and political benefits did not materialize, Puerto Ricans reacted with street protests in the late 1960s to demand inclusion and equality. Later, to

overcome exclusion from machine politics, they skillfully organized their community around electoral mobilization politics. Using a variety of electoral vehicles, most notably the Puerto Rican Political Action Committee (PRPAC), Puerto Ricans began to be elected to local and state offices. Among the many accomplishments of the PRPAC was the formation of a progressive multiethnic coalition that challenged the local machine organization and achieved electoral victory in 1991. Just two years later, the voters turned the multiracial coalition out of office.

To explain the dramatic rise to power and the subsequent defeat of a promising electoral coalition, Cruz explores the relationship between identity politics and urban power. He argues that the construction of an ethnic identity served as an organizing beacon for newly arrived Puerto Ricans, who were poor, spoke a different language, and had no job skills. The central argument is that "identity politics was not a stage in the process of incorporation, one to be left behind by integration. It was instead both the medium for and outcome of political mobilization" (p. 10). Identity is a constant, but identity politics is an ever-changing process based on the "ebb and flow of issues, agendas, and available resources" (p. 11).

In Cruz's view, there is a two-stage process of identity politics and urban politics. First, there is a mobilization based on identity politics for political representation; second, following an election, a new stage occurs, based on power relationships and policymaking that extends beyond identity politics. The notion of urban power is used to explain what happened when Puerto Rican identity candidates were elected to office.

This book is situated in the context of the ongoing debate about the limits of local government in an era of structural constraints. Cruz attempts to synthesize previous explanations of power in an urban setting and build "a contingency theory of power" (p. 17) to explain how access to power takes place, and he identifies some of the elements that affect its exercise. Puerto Ricans were unable to move from representation to effective policy responsiveness because of the economic, political, and social context when power was achieved. The unseating of the progressive coalition in 1993 was the result of several factors, including weak internal cohesion among the Puerto Rican officeholders, interethnic conflicts with African Americans over political spoils, fiscal crises, and the failure to understand how policymaking is accomplished in a complex, business-dominated climate. Ethnic identity served as a mechanism to win elections, but identity politics was not sufficient to govern in a multiethnic environment.

Cruz makes an important contribution to the contemporary literature on Latinos in politics and the broader question of the value of identity politics for power achievement. Studies on Latino participation in politics are growing in number and scope, with some scholars focusing on the attitudes and voting behavior of Latinos. Recent research has moved away from case studies of grassroots efforts to achieve democratic rights, but this does not mean that Latinos have ceased to struggle for political empowerment. This book is reminiscent of case studies conducted on urban Latinos in the 1970s and 1980s. It is rich in detail and offers a nice counterbalance to recent macro and quantitative studies of Latinos. By zeroing in on the variable of political agency and conducting in-depth interviews with many of the key participants, Cruz is able to give voice to the activists' motivation and strategic considerations for their actions.

Cruz also challenges some contemporary notions of identity politics and refuses to fall into the trap of glorifying or reifying identity politics. His book is a sharp rebuke to those

who argue that diversity and ethnic identity politics are causing the disuniting of America (Arthur Schlesinger, The Disuniting of America, 1992). In Hartford, Puerto Ricans used ethnic identity politics in a positive manner, not to divide but for positive reinforcement. As the country grows increasingly diverse, multiethnic coalitions loom large on the horizon in urban centers, and the experience of Hartford's Puerto Rican community is revealing. The dilemma of officeholding and being responsive to the policy needs of one's ethnic community while simultaneously reaching out to other ethnic and issue-oriented groups is a topic that needs considerable study. If the purpose of ethnic politics is to elect ethnic candidates with the understanding that there will be increased policy benefits, then the evidence provided by Cruz is that there are two different rubrics to consider, office achievement and officeholding.

The book is not without shortcomings. First, in a detailed discussion of an active community with numerous political organizations, a system of categorizing the various groups, key issues, leaders, and time span would strengthen Cruz's argument of endogenous diversity. Second, the commonwealth status of Puerto Rico poses unique issues for Puerto Ricans who migrate to the mainland. There is substantial discussion about why Puerto Rican immigrants focused more on local politics than island politics, but there is little analysis of the value and symbolism of U.S. citizenship that Puerto Ricans enjoy upon arrival but that other immigrant groups do not. This appears to be a major factor that enabled the Puerto Rican community to move rapidly from new immigrants to officeholders in less than three decades.

Finally, Cruz's attempt to link identity politics with urban power requires a deeper discussion of the overall development of progressive politics in Hartford. It is difficult to grasp the mobilization strategies of the other coalition forces, such as the People for Change (PFC), a progressive white group, labor activists, and African Americans—the largest nonwhite racial group. The strength of this book, the focus on power attainment by Puerto Ricans, limits a fuller discussion of citywide politics and how urban power relations unfolded in the critical period 1985–91.

These minor criticisms notwithstanding, Cruz overall does an admirable job of describing the complex process of identity politics at the local level and how a new immigrant group was able to build electoral strength. One hopes this study is a portent of future ethnic and multiethnic case studies documenting the transformation of America into a majority nonwhite society in the new century.

The Constitutional Underclass: Gays, Lesbians, and the Failure of Class-Based Equal Protection. By Evan Gerstmann. Chicago and London: University of Chicago Press, 1999. 196p. \$35.00 cloth, \$15.00 paper.

Ronald J. Hunt, Ohio University

At the heart of Evan Gerstmann's book is the argument that class-based equal protection jurisprudence has failed to protect the interests of gays and lesbians as well as other minority groups, such as the poor and the elderly. As evidence, the author cites a vast array of court decisions that have denied gays and lesbians equal protection of the law: decisions upholding the prohibition against immigration to the United States and against employment by the FBI, the "don't ask, don't tell" policy, and state sodomy and custody laws.

The author presents his case in three parts. In the first part, he explains the origins of class-based equal protection; the

differences among "strict" scrutiny, "intermediate" scrutiny, and "rational-basis" scrutiny; and the underlying inequity and incoherence inherent in this system. The Supreme Court has devoted an inordinate amount of time and energy to elaborating the principles governing its decision making, but in Gerstmann's judgment there have never been clear standards defining the levels of scrutiny, and this has led to an invidious discrimination against many groups by the federal and state courts.

Gerstmann describes the gradual incorporation of the Carolene Products framework into the Court's decisions governing African Americans in such cases as McLaughlin v. Florida and Loving v. Virginia, cases in which the Court established that race-based classifications would be subject to the highest level of scrutiny and also extended strict scrutiny to "alienage" as well as a growing list of "fundamental rights" (e.g., the right to vote) deserving protection.

For obvious reasons, gays, lesbians, and other groups who claim a history of discrimination have sought to establish their entitlement to strict scrutiny by the courts. From Gerstmann's point of view, however, this outcome is highly unlikely. Why? "It is no small step to declare that factors such as prejudice toward a particular group render democratic decisions regarding that group 'suspect.' It means putting on the judicial table all manner of laws regarding that group" (p. 36). With the growth in the 1960s and 1970s of the number of groups who claimed discrimination, the Court realized the Pandora's box it had created and slammed the door on all groups claiming suspect status. "No group since the mid-1970s has been found to be a discrete and insular minority and it is unlikely that any new group ever will be" (p. 29).

In the second part of the book, Gerstmann discusses the failure of the class-based equal protection framework to protect the rights of gays, lesbians, and, by extension, other similarly situated groups. His focus is Amendment 2, enacted by Colorado voters in 1992. It overturned existing civil rights protections for gays and lesbians in Boulder, Denver, and several other jurisdictions, and it forbade the enactment of similar protections by the state or any of its agencies, subdivisions, or municipalities. The author provides great insight into various aspects of the political struggle that surrounded the adoption of this amendment, including the highly novel argument that the equal protection framework of the Court was used by proponents of Amendment 2 to convince the public that gays and lesbians would soon be claiming affirmative action status if the measure was not adopted. Gerstmann also demonstrates the ultimate incoherence of the class-based equal protection framework through his analysis of the wildly varying judicial opinions written about the applicability of strict scrutiny to gays and lesbians and of the constitutionality of Amendment 2. "None gave any clear reason why Amendment #2 does or does not violate the equal protection clause" (p. 115). Ultimately, the U.S. Supreme Court in Romer v. Evans sustained both the Colorado District Court and the Colorado Supreme Court in upholding an injunction against the enforcement of the amendment. According to Gerstmann, however, the opinion of the U.S. Supreme Court can only be understood as an emotional response to the imbroglio. "The ostensible standards for deciding equal protection cases were largely ignored, and the case ultimately came down to the Supreme Court's intuitions about how the Colorado voters felt about gays and lesbians" (p. 115)

Most fundamentally, the author is convinced that both the federal and state courts apply the class-based equal protection framework in a highly arbitrary and biased manner, not so much because of their prejudices as because of the U.S.

Supreme Court's unwillingness to extend strict scrutiny to more and more groups, something the Court undoubtedly feels would expose the institution to further criticism for legislating from the bench. This has led the Court, among other things, to invent conventions such as intermediate scrutiny to cover issues related to gender discrimination and to apply differential levels of rigor to the rational basis test, a practice that the author calls "cheating" and that illustrates the incoherence and confusion generated when groups with clear histories of discrimination are denied the same level of protection as African Americans.

The final section of this stimulating book discusses alternatives to the current approach. Gerstmann concludes that, since the Court cannot apply strict scrutiny to all laws, there must be a clearly defined method for differentiating among levels of scrutiny. Following the lead of J. Harvie Wilkinson ("The Supreme Court, the Equal Protection Clause, and the Three Faces of Constitutional Equality," Virgunia Law Review 61 [1975]: 945–1018), Gerstmann elaborates and comments on Wilkinson's argument that issues related to who has been denied equality should be subordinated to what kind of equality has been denied. The balance of this insightful and provocative book is devoted to a critical reflection on the merits and limitations of Wilkinson's ideas, a reflection that should add to the existing scholarship on the limits of class-based equal protection jurisprudence.

The Constitutional Underclass is thought provoking, insightful, and readable. That there are both legal and political problems associated with class-based equal protection is obvious. Gerstmann succeeds in elaborating the full extent of the problems, the innumerable ramifications, and the potential of alternatives to the status quo.

Forged Consensus: Science, Technology, and Economic Policy in the United States, 1921–1953. By David M. Hart. Princeton, NJ: Princeton University Press, 1998. 267p. \$39.95.

Roger G. Noll, Stanford University

Research on technological change and the policies affecting it is voluminous. Nevertheless, as David Hart notes, this literature hardly rests at the center of any discipline. Despite the importance of technology in modern society, most scholars pay little attention to scholarship about it, and the lessons from this research are accorded an incidental role in undergraduate and graduate education.

Hart's goal is ambitious: to bring the political economy of technology policy to the forefront of research on American politics. He seeks to do so by building a stronger, more sophisticated theory of science policy that is embedded in broader theories of policymaking m the United States (chap. 1) and by using the historical development of science and technology policy in the crucial period since World War I to test his theory (the rest of the book).

Hart's foundational assumption is that policy arises from the pursuit of goals by political officials that emanate from "ideas" (opinions about cause-effect relationships melded with normative views of the nature of a good society and the proper role of government in attaining it), as constrained by the necessity to mobilize support among interests, to overcome institutional resistance, and to make compromises to form coalitions with others pursuing their own idea-driven goals. Thus, whereas self-interest, collective-action problems, and agenda-setting effects of institutions have roles, the centerpiece is ideas.

Hart believes that research on the politics of technology

policy has little effect because it rests on a simplistic, two-part 'conventional wisdom," derived from Vannevar Bush's activities to institutionalize government research and development after World War II. First, postwar technology policy is said to be highly stable, reflecting a broad consensus about the proper role of government in generating new technical knowledge. Second, this consensus is said to arise from two widely shared normative values: economic liberalism (giving privileged status to the market as a means for advancing the welfare of society) and "transaction cost economics," by which Hart means market failures (identifying specific areas in which market incentives are insufficient to generate socially desirable activities, such as basic research; research and development related to government activities such as national defense; and technology transfer to generate commercial "spin-offs" from government research and development).

Hart contends that technology policy is not stable and does not reflect an enduring political consensus; instead, it is an eclectic grab bag that periodically changes dramatically. Moreover, Hart argues that the conventional explanation cannot identify with acceptable precision the actual programs and policies that government adopts. The trouble, according to Hart, is that economic liberalism plus market failures do not clearly delineate what government will do; they are contested concepts with changing definitions and boundaries that reflect changes in the underlying normative values that motivate government decisions.

As an alternative, Hart offers five "visions" of the role of government in the economy that motivate different political actors, and he hypothesizes that technology policy changed during the twentieth century due to changes in the relative influence of these visions within governing coalitions. These five visions are: (1) traditional conservatism, which is skeptical of any role for government in the economy; (2) associationalism, that is, government can help overcome market failures that harm business by supporting information exchange, standardization, cooperative applied research, and basic research that underpins commercial technology; (3) reform liberalism, or government can overcome market failures that harm consumers and that arise from myopia, greed, and economic power; (4) Keynesianism, or the main market failure is macroeconomic instability, which government can control through taxes, expenditures, and monetary policy, which are selected in part on the basis of their effects on long-term economic growth; and (5) the "national security state," that is, a hostile external world gives privileged status to national security policies, including defense-related research. A sixth vision, a Marxist neo-Luddism that sees technology as a means to generate unemployment and cut labor costs, periodically is on the scene, but its adherents never manage to influence technology policy.

The book's strength is detailed histories of technology policies from Hoover's establishment of the Bureau of Standards through the hardening of the Cold War during the Truman administration (chaps. 2–7). Most readers probably will learn quite a bit about the nuts and bolts of technology policy during this era. In recounting this history, Hart emphasizes the controversies and differing perspectives of officials concerning the government's role in R&D. He succeeds in demonstrating his two main points: Most important technology programs arose amid controversy, and they reflected compromises among different perspectives about the importance of market failures in R&D and the ability of government to enhance technological progress.

Whether these accomplishments constitute an important new way to conceptualize technology policy that will propel the field into the mainstream of American politics is less certain. Some scholars may be perplexed by Hart's argument. His "conventional wisdom" accurately characterizes how many public figures frame policy debates, including some technology policy scholars whose disciplinary roots are in science and engineering. But relatively little work in economics and political science adopts this approach. Many scholars regard the market failure notion as useful for identifying what government ought to do and for evaluating the success of government policies, but few regard this normative model as a useful positive theory of policy choice. Finally, the ideas that motivate political actors surely are important, and characterizing them in a nuanced, comprehensive manner advances understanding of policy, but the importance that the book accords them is debatable. Other concepts in American politics are mentioned, such as reelection motives of elected officials, career motives of bureaucrats, economic interests, mobilization bias, political entrepreneurship, and the agenda-setting role of bureaucratic and legislative institutions, but they are given less emphasis than in most contemporary studies of the politics of policymaking in the United States.

Of course, to expect a book to reform a field is a tall order. Hart succeeds in articulating a new view of the relationships between technology policy and conflicting ideologies about the role of government in the economy, and he illustrates how these normative conflicts relate to policy choice in an important domain of government activity. This accomplishment is enough to make the book worth reading.

Representing Popular Sovereignty: The Constitution in American Political Culture. By Daniel Lessard Levin. Albany: State University of New York Press, 1999. 283p. \$59.50 cloth, \$19.95 paper.

John Brigham, University of Massachusetts, Amherst

Daniel Levin examines the Constitution in various public manifestations, but his sustained focus is on the Bicentennial Celebration in 1987. This effort joins law scholarship in the social sciences that has broadened the field by incorporating interpretation and cultural studies into jurisprudence. Levin is critically self-conscious about the social dynamics of constitutionalism, and his book develops new ways to think about the Constitution.

In the section on mass-market tie-ins, Chief Justice Warren Burger comments on how tasteful the placemats produced by McDonalds will be, and Levin notes, in a parody of the pretentious authoritizing of law review editors: "The placemats are on file with the author" (chap. 2, n 96). With reference to the problem of commodification, he reminds us of Bisontennial Ben, a bison character wearing federal period costume offered by the Disney Corporation but rejected by the Bicentennial Commission.

The book operates at a number of basic intellectual levels. Levin offers a fresh and thoughtful response to the unstated question: "What is a Constitution?" He sees limits in the "traditional understanding," with its focus on what the founding fathers thought of the Constitution. The founders likely would have been incapable of explaining the importance of the Constitution that we protect in the National Archives in "a bullet-proof canister [that] may be instantly lowered into a subterranean vault." Levin provides a sustained investigation of current cultural representations, including the dramatic display of the original parchment.

The chapters are laid out with a clear sense of the terrain and what we need to be thinking about. "The Problem of an Abstract Constitution" suggests that the Constitution lacks "sex appeal" because it is difficult to understand. "The Conscious Creation of Constitutional Culture" gives attention to efforts by Safeway and Winn-Dixie to make Constitutions available to their shoppers and the Bicentennial Commission's special zip code so mail could simply be addressed as "Constitution." "Representing Popular Sovereignty" ties the whole enterprise to culture studies more generally.

The notes are full of citations to contemporary scholarship. We sometimes fail to see ourselves as a shared enterprise, but Levin provides evidence that constitutional study is emerging as a field that takes its practitioners beyond Supreme Court doctrine. Some work is in critical theory, some is ethnographic, and there is even some political science. I did think that pioneering work published in the APSR should have been given more attention. Articles by William Harris ("Binding Word and Polity," American Political Science Review 76 [March 1982]: 34-45), on how we bind ourselves through the words in the Constitution and by Timothy O'Neill ("The Language of Equality," American Political Science Review 75 [September 1981]: 626-35), on the language of constitutional equality appeared in the early 1980s. More recently, published debates between Michael McCann (Rights at Work: Pay Equity Reform and the Politics of Legal Mobilization, 1994) and Jerry Rosenberg (The Hollow Hope, 1993), turn on the meaning of the Constitution in the polity. Clearly, more historians, lawyers, and journalist are doing this work, but this is also a political science project with a

Although the cumbersome comprehensiveness of the Constitution makes it difficult to arouse the public about it, that is generally the subject of the book. Instead of the whole thing, which the Bicentennial was admittedly celebrating, perhaps we should pay more attention to how we represent its parts. We have had compelling debates about which evidence was to be excluded in the O.J. Simpson murder trial, conflicts over the meaning of affirmative action, and differing perceptions about whether the trial of a sitting president is much of a distraction to work at the office. I teach about these things, and sometimes I am amazed at the appeal of such constitutional topics before the students even get to my lectures.

Levin helps us get beyond a persistent behavioralism and recalcitrant formalism. His scholarship also reminds us that popular constitutionalism has had programs such as the Jefferson Foundation meetings that predated the Bicentennial. The Jefferson approach could use some of Levin's insight about the limits of simulation, and the earnest practitioners of this form of Constitution study deserve a little critical attention of the sort Levin provides. Much of the book is pathbreaking analysis, but the debates on new institutionalism and constitutive theory show that we can handle even more expansive critical inquiry.

The book is also framed by a sort of gap or impact logic expressed as wonder at the failure of the Constitution to operate in the public sphere in the same way that the Declaration of Independence has operated. The caution appears in the limited nature of the frame. There is so much that is useful in the book that the old framework which seeks to register the effect of decisions is a bit of a straight jacket. For instance, the author might have explored the corporate nature of contemporary constitutionalism as a constitutive dimension of the Constitution's cultural life but notes instead that corporate rather than widespread popular support gives us a false notion of the private.

In this regard, some themes could have been developed more carefully. Levin states: "Sacred texts rarely enter the profane realm of popular culture" (p. 3). Although this may have been true of the Constitution, it does not seem to have been true of the Bible, the Torah, or any number of Zen teachings. There is a tone that draws attention to the political scientist's Constitution, the one that the public does not know about and that many have treated as if it were too complex for them to understand.

The orientation of the book and its contribution to American constitutional scholarship is entirely in the opposite direction. As does Michael Kammen's awkwardly titled but seminal historical work, A Machine That Would Go of Itself (1986), and work by scholars such as Sanford Levinson, Wendy Brown, and Michael McCann, this book helps show us that the Constitution is not simply what the judges say it is (no matter how much the bulk of political science publishing in constitutional law has invested in this belief).

Levin has Garry Trudeau providing in the *Doonesbury* comic strip a copy of the Constitution to be attached to a cereal box for "mighty fine breakfast reading" (2/22/1987), but Levin's book gives us a Constitution that is not just living but in search of a celebration.

Environmental Cancer—A Political Disease? By S. Robert Lichter and Stanley Rothman. New Haven, CT: Yale University Press, 1999. 235p. \$35.00 cloth, \$17.00 paper.

Howard Margolis, University of Chicago

This is a good book on an important matter, but the material it addresses is so highly polarized that a different reviewer might have characterized it as a perverse book on an important matter. The deep issue is the persistent conflict of expert versus lay intuition that has marked public concern over subtle environmental risks: that is, not risks such as smoking, which no one any longer even pretends is anything but a serious risk, but risks such as those from saccharin or electromagnetic forces (EMF), whose actual effects (adverse or conceivably even favorable) are sufficiently subtle that it is very hard to be confident that in fact there is any net adverse effect at all

On such issues, lay concern is often so strong that expert concern comes to be marked mainly by a reaction about misallocation of public health resources. This frequency of expert/lay conflict on such issues then yields a sort of second-order polarization among people who write about this contrast. Some of us find the predominant expert views on these matters convincing, and others believe lay judgment deserves far more respect than the experts are inclined to give it. But there is one obvious sense in which lay views have to be taken very seriously indeed: There are vastly more lay voters than expert voters.

Lichter and Rothman focus, as the book's title states, on cancer. That is not the only risk for which expert/lay conflict is apparent, but it is certainly the most prominent. The main novel material in the book comes from survey data on expert versus lay differences and from content analysis of mass media coverage. For APSR readers, this will be of some interest simply as applications of method. Overall, however, the results do not differ much from the impression provided (for example) by Stephen Breyer's (now Mr. Justice Breyer's) Vicious Circle (1993) and any number of other studies. So the market for this book may be limited.

The central point, as suggested by the title, is that concern about subtle environmental cancer risks is largely to be understood in political and sociological terms, strongly influenced by how things are treated in the press and by how politicians and bureaucrats respond to public sentiment. The

press is, of course, strongly motivated to cover what readers want to know about, and politicians are motivated to make choices that voters find easy to believe are right. This easily aggravates the large disparities suggested earlier between that for which experts find evidence and that which the rest of us seem most ready to believe. In turn, that affects what the press is motivated to report and what politicians find comfortable positions to take. A study that finds a link between some chemical and cancer is more likely to be reported than a study that finds no link. In turn, the concerns of the public are reinforced, which reinforces the propensities of the press and the sensitivity of politicians. Causation goes all the way round (hence Breyer's discussion of a "vicious circle"). Among the results is a far greater public perception of a cancer epidemic than actual data could possibly warrant. Hence the concern of this book with cancer as a "political disease."

The Democratic Dilemma: Can Citizens Learn What They Need to Know? By Arthur Lupia and Mathew D. McCubbins. New York: Cambridge University Press, 1998. 282p. \$64.95 cloth, \$19.95 paper.

R. Michael Alvarez, California Institute of Technology

This book has generated excitement in political science for a number of reasons. First, Lupia and McCubbins have translated the rational choice signaling model into terms that most political scientists understand. Second, the authors discuss recent advances from the cognitive sciences in the context of a rational choice signaling model. Third, they test propositions taken from their formal model with a new form of laboratory experimentation and with a telephone survey. Given these three different dimensions, it is no wonder that this ambitious book has generated a great deal of interest.

Lupia and McCubbins structure their argument in two parts. The first is theoretical (chaps. 2–5), and the second is empirical (chaps. 6–9). Their use of the rational choice signaling model and their integration of cognitive science advances fall into the first major component, and their laboratory and telephone survey experimentation falls into the second major component. Generally, the book is exceptionally clear in structure and writing.

The general theme is that democratic citizens can make "reasoned choices" when they are poorly informed. By "reasoned choices" Lupia and McCubbins mean those "based upon an accurate prediction about a choice's consequence" (p. 18). Being able to make an accurate prediction, however, requires accurate information. Obtaining accurate information requires decision makers to be active information gatherers. If they are, then they will be attentive to information sources beneficial to their choice—information that prevents them from making a bad decision.

Thus, "reasoned choice" is based on obtaining information from others, which Lupia and McCubbins call "persuasion." They argue, from their rational choice model, that "persuasion occurs only if the principal is initially uncertain about which alternative is better for her, believes that the speaker may have the knowledge which she desires, and believes that the speaker has an incentive to reveal what he knows" (p. 55, emphasis in original).

This is an interesting and clear set of theoretical propositions, but their exact scope is not clear. Lupia and McCubbins want to apply their theoretical model to virtually every level of political decision making, from voters to jurors to legislators. In the end, the assumptions underlying their theoretical model seem much more appropriate for some political decisions than others.

Most important, it is unclear whether this theoretical model applies easily to the decision problem facing voters in large elections. For example, the assertion that voters in contexts such as presidential elections are active seekers of information is a difficult assumption to maintain. In fact, one of the most important advances in this literature is the finding that there are vast differences across the American public in the degree of political awareness (John Zaller, *The Nature and Origins of Mass Opinion*, 1992). Segments of the mass public are essentially uninformed, other segments are well informed, and yet other segments fall somewhere in between. Clearly, it is incorrect to assume that the citizens in the mass public are all seekers of information.

Instead, this model of decision making seems best suited for contexts in which the stakes are greater—such as legislative decision making or juries. In these political settings the assumption of active information gathering makes sense; because higher benefits derive from making decisions of better quality, individuals in these contexts should be more willing to bear the costs of information acquisition than the mass public in the context of a presidential election. To borrow the assumption of active information seeking from cognitive science is interesting, but this assumption does not lead to a model with general applicability to all realms of political decision making.

The other assumption that Lupia and McCubbins borrow from cognitive science is "connectionism," which "is the way that people systematically connect current observations of their physical world to physical or emotional feedback from experience" (p. 19, emphasis in original). Exactly how this assumption fits into their rational choice model is not clear, and it does not seem to lead directly to any new inferences about political choices.

Chapters 7 through 9 focus on the empirical testing of propositions from the "reasoned choice" model. Readers unfamiliar with the "experimental economics" movement will find chapters 7 and 8 very useful. The types of experiments discussed in these two chapters are excellent examples of this exciting method for testing rational choice theories. These laboratory experiments are useful for demonstrating whether subjects play the experimental game in ways consistent with the mathematical predictions of the rational choice model, thus testing the internal validity of the model that Lupia and McCubbins use.

But these laboratory experiments lack external validity. That is, they do not tell us whether decision makers in real political contexts behave consistently with the predictions of the model. In chapter 9, Lupia and McCubbins use telephone survey experimentation to test propositions about persuasion. They posed questions about prison spending to survey respondents; to a randomly selected group the question was framed as either being supported or opposed by Rush Limbaugh or Phil Donahue. In relatively simple statistical tests, Lupia and McCubbins report that respondents followed the cues presented in this framing experiment, as they made statements about prison spending consistent with the position of the source of the cue, conditional on whether the respondent considered the source knowledgeable and trustworthy.

It is difficult to write an ambitious book that satisfies every reader, and I would have appreciated more empirical testing of the theoretical propositions, especially more testing with strong external validity. Even so, Lupia and McCubbins have produced an extremely useful work that makes theoretical and empirical contributions to the literature. Most important, the clear and thorough exposition of the rational choice

signaling model and the elaboration of the experimental laboratory method make this a book that should be required reading for political science researchers and students alike.

Getting Beyond Race: The Changing American Culture. By Richard J. Payne. Boulder, CO: Westview Press, 1998. 240p. \$25.00.

Robert C. Smith, San Francisco State University

Race is the most profound and enduring cleavage in American society and politics. It always has been. Therefore, the social science disciplines have always devoted an enormous amount of attention to the subject. No less is true today. Indeed, in the last several years the number of scholarly books has likely outpaced the capacity to keep up of all but the most indefatigable.

In this vast outpouring of studies, Richard J. Payne's Getting Beyond Race is different. Rather than focus on the historical, structural, systemic, or contextual bases of the problem, Payne, a political scientist, focuses on the individual. He takes this approach because he believes "race relations are ultimately about ordinary people interacting with each other on an individual basis" (p. xii). Getting Beyond Race takes us back to the pre civil rights era (circa 1950) of scholarship on race, an era when "race relations" studies sought to point the way out of America's racial dilemma by formulating the problem as psychological traits of individuals that translated into social and economic cleavages. Payne labels this a "bottoms-up approach," writing in the concluding chapter that this method of "improving race relations places greater responsibility on individuals to do what they can to get beyond race. General concern about how races interact must be supported by specific actions on the part of ordinary Americans toward each other, not as monolithic groups but as unique individuals. Ultimately, race relations are about individuals getting along with each other" (p. 194).

Payne argues that there is growing evidence that Americans are embracing this individual approach to race. Among other things, he cites the example of race relations in the military (a great success he thinks); the decline of racist attitudes among whites; the effect that large-scale immigration from Asia and Latin America is having on racial classifications and boundaries; generational replacement; and increasing interpersonal contacts across racial boundaries, including more interracial marriages and transracial adoptions. These changes Payne believes are resulting in a decline of racism and increasingly are making the very concept of race irrelevant.

Payne's argument is alluring but not persuasive. Americans are not divided by color because blacks and whites as individuals cannot get along. Rather, the race cleavage in the United States persists because of the persistence of a historically constructed set of systemic, structural, and institutional arrangements that systematically result in negative outcomes for blacks while advantaging whites. These systems of disadvantage are sustainable without the active or self-conscious participation of most whites. Until these arrangements are changed, it is not likely that any amount of interaction between individual blacks and whites, no matter how well intentioned, or even intimate, will fundamentally bridge America's racial divide. Indeed, a fair amount of research shows that whites with egalitarian racial attitudes and close black friends can nevertheless support structural and institutional arrangements that preserve their group's position of dominance and privilege.

Funding Science in America: Congress, Universities, and the Politics of the Academic Pork Barrel. By James D. Savage. New York: Cambridge University Press, 1999. 219p. \$49.95.

David M. Hart, Harvard University

Perhaps you have wondered, striding across your campus, why the new center for quantum computing is named for Senator Blowhard, who you know has never touched a keyboard in his life. Posing the question, of course, is tantamount to answering it: The good senator got your university president the money to build the center. Just like loyal alumni and friendly corporations, members of Congress routinely earn such gratitude these days. Yet, perhaps (being a card-carrying political scientist) you took the next step and wondered whether the local benefits of this center and all the others similarly named might impose a general cost on the nation, since the judgments of Senator Blowhard and his colleagues appear to have been substituted for those of experts who surely know their way around a keyboard better than their elected representatives. These questions, both obvious and subtle, animate James D. Savage's short, solid, and readable new monograph.

Savage skillfully narrates a series of episodes in which university presidents and members of Congress advocate and obtain (or object to and block) "academic earmarks." This history, which covers roughly the past two decades, is exhumed from the obscurity of private meetings and massive appropriations bills with the assistance of the public record, selected internal documents of the Association of American Universities (AAU) and a couple of its member schools, and about twenty interviews conducted while Savage served as a consultant on the issue to several congressional bodies in the early 1990s. The most illuminating chapter for this reader describes the emergence of lobbying firms that specialize in academic earmarking and their diffusion of this innovation from client to client. The ability of these operatives to identify legislative vehicles and construct networks of support for particular projects may not surprise readers of this journal, but Savage provides detailed examples that you will enjoy and from which your students will learn. Other chapters cover the deliberations of the AAU, the history of federal legislation to fund academic research facilities as an alternative to earmarking, congressional debates, and the role of the White House.

Savage does not flinch from naming names and making judgments. Table 3, for example, provides the amounts that AAU members received as a result of earmarks between fiscal 1980 and fiscal 1996 (Iowa State and Pittsburgh lead the list with more than \$100 million); tables 5 and 6 list the higher education clients of Cassidy and Associates and other lobbying firms. Although he lets its advocates make their case, often in their own words, Savage thinks academic earmarking is a bad practice. He calls the AAU to account for its failure to sanction members who ignored its resolutions to refrain from the practice, and he amply demonstrates hypocrisy among the membership.

The analysis backing up Savage's overall assessment of academic earmarking is convincing in some respects but not in others. Savage shows that earmarks are highly concentrated in the districts of legislators on the appropriations committees and so cannot generally redress the geographical inequities that conventional research funding allocation processes produce (although West Virginia benefits handsomely). He also shows that many institutions which already receive substantial federal research funding gain from earmarks as well; the practice does not appear to target institu-

tional inequities very effectively. On the key question of research quality, Savage's data reveal that some recipients of earmarks have risen substantially in the rankings of total federal research funding since the practice became widespread.

Earmarks that build capacity in growing fields of research, such as biomedical science, may permit institutions to go on to obtain peer reviewed grants. Savage stops short of drawing this conclusion, which is unpleasant to him, because he claims to be unable to produce a figure for peer reviewed research alone. (He might at least have subtracted the earmarks from the institutional total to yield a guess or, more ambitiously, done some statistical analysis or sought alternative output indicators, such as papers produced.) If this conclusion is warranted, however, academic earmarking is not necessarily as bad as Savage thinks. The history of academic medical centers since the 1950s bears out the point that talent sometimes follows money. I do not mean to defend the practice in general but to suggest that a more nuanced assessment is in order. This portion of the analysis also suffers from the dichotomy that Savage draws between earmarking and peer reviewed research funding programs. Other modes of allocation beside these two, including formula funding and relatively autonomous program management, deserve to be taken into account.

More generally, Savage's figures show that academic earmarking is simply not big enough in the larger scheme of federal research funding to make the difference that he seeks to find at the level of states or regions. Academic earmarks have never amounted to more than 7% of federal academic research funds, and half of these earmarks were made by the agriculture and defense appropriations subcommittees. (One discovers this fact very late [p. 126], long after Savage criticizes others for failing to make their definitions of academic earmarking clear.) The National Science Foundation and National Institutes of Health appropriations have remained remarkably clear of earmarks. The book, then, is haunted by the problem of the dog that did not bark, as Savage, to his credit, admits. I was persuaded that academic earmarking has the potential to do serious damage but not that it has yet done so. Savage is better at elucidating the motives of those who pursue academic earmarks than those who fail to seek them or those who oppose them, despite his ambition to explain all these motives (p. 3).

This weakness may stem from Savage's failure to engage effectively with the theoretical literature. He makes a promising stab at using an Olsonian framework to analyze the AAU, but he does not carry it all the way through. Other potentially useful connections (even the most logical one, to Linda Cohen and Roger Noll, Technology Pork Barrel, 1991) are simply passed over. The framework offered in chapter 1, using the work of Thomas Kuhn and Donald Searing, is but a gesture and virtually disappears in the rest of the work. My conclusion is that this book will be of intense interest to practitioners and analysts of academic research policy, will make worthwhile reading for specialists in science policy, and will appeal modestly to students of public policy and American politics in general.

The Sometime Connection: Public Opinion and Social Policy. By Elaine B. Sharp. Albany: State University of New York Press, 1999. 289p. \$17.95 paper.

Ted G. Jelen, University of Nevada, Las Vegas

The simple yet profound purpose of *The Sometime Connection* is to investigate, in broad strokes, the correspondence

between public opinion and public policy in American politics. In an ostensible democracy such as the United States, policy should, in principle, be guided by the preferences of mass publics. Using highly accessible materials, such as historical accounts of the policymaking process and descriptions of aggregate public opinion, Elaine Sharp investigates the opinion-policy connection over the past thirty years (longer in some cases) for six issues: crime and punishment, abortion, affirmative action, pornography, welfare, and social security.

Sharp is quite attentive to several sources of complication in her analysis. She is well versed in the literature concerning the quality of public opinion in mass publics, and she takes care to distinguish genuine attitudes from nonattitudes; she also is generally alert to possible changes in political context that can alter the interpretation of poll results. At first glance, one would think that nonattitudes would pose a nonproblem in these six issue domains, since all seem to correspond to the notion of "easy issues" (Edward G. Carmines and James A. Stimson, Issue Evolution: Race and the Transformation of American Politics, 1989) on which meaningful public opinion is cognitively nondemanding. Yet, Sharp demonstrates plausibly, if not entirely convincingly, that the quality of mass attitudes varies across the domain of these six social issues.

Even if there exists a relationship between mass attitudes on a particular issue and public policy toward that same subject, it does not follow that political elites are responsive to changes in public opinion. Indeed, it is possible that the causal arrow runs in the opposite direction, that mass preferences are the result of elite leadership (phrased positively) or manipulation (understood more pejoratively). Sharp argues quite convincingly that precisely such a sequence accounts for the dynamics (or, more accurately, the statics) of public attitudes toward social security.

Sharp does an excellent job of accounting for the complexity of various policy areas. For example, she suggests there are at least three components to attitudes and policy toward welfare: the amount of money expended on public assistance to the poor, work requirements for recipients of public assistance, and the "moral regulation" of welfare recipients. These components are, to a large extent, independent of one another, and Sharp provides a very clear and compelling analysis of each.

The model of opinion-policy interaction that Sharp regards as normatively preferable is one she terms the "thermostat" model, in which mass attitudes and government policies exert reciprocal influences. That is, government policy ideally should change in response to public opinion, and the demands of certain aspects of public attitudes (e.g., the suggestion that "too little" is being spent on a particular problem) should change as the result of government actions. Sharp's analysis reveals, however, that only in two of the six policy areas considered here—abortion and welfare—does the thermostat model seem to apply.

In the areas of crime and social security, Sharp argues that the model of a "broken thermostat" is most descriptively accurate. That is, policymakers appear responsive to public demands to "do more" to punish criminals or to "protect" Social Security, but aggregate public opinion does not appear responsive to changes in government policy. In both cases, Sharp suggests that this nonrelationship can be attributed to the actions of certain policymakers. In the case of imprisonment, Sharp argues that strategic political candidates often can make use of the "crime issue" (perhaps using anecdotal evidence) despite real declines in the crime rate. Since it is unlikely that crime will ever be completely eliminated, the public's fears can easily be exploited by seekers of elective

office. With respect to Social Security, Sharp argues that the program was initially oversold to the public in order to protect Social Security from the stigma of "welfare." Proponents have emphasized the insurance and universality features of the program to ensure broad public support. In an important sense, such leaders have done their work too well, as even the appearance of benefit reduction now occasions intense public reaction (although Sharp notes some recent, and quite understandable, generational differences on this issue).

In a bit of terminology I found confusing, Sharp characterizes the opinion-policy relationship on affirmative action as a "Downsian sequence" (a term borrowed from a 1993 book by Frank Baumgartner and Byran Jones). In a Downsian sequence, public policy is initially responsive to public opinion, but responsiveness decreases as entrenched elites assume responsibility for policy implementation. Thus, the widespread support for civil rights for African Americans that existed before 1964 did not include the possibility of "preferential treatment" for disadvantaged minorities, but aggressive (generally bureaucratic) efforts to equalize the economic status of the races continued in the face of widespread public opposition. Sharp attributes this sequence to the need for government bureaucracies (in this case, the Equal Employment Opportunity Commission) to produce tangible evidence of their effectiveness.

Finally, Sharp shows that public attitudes toward the regulation of pornography, despite apparent stability, exhibit certain characteristics of nonattitudes, that is, public opinion appears neither to cause changes in public policy nor to be affected by such changes. Similarly, public attitudes toward pornography appear serenely stable, despite large-scale shifts in activist rhetoric (from pornography as a moral issue to an issue essentially involving women's rights).

One of the most frustrating but intriguing aspects of The Sometime Connection is Sharp's inability to provide a general explanation for the patterns described. Why, after all, should we enjoy (or endure) democratic policymaking in some issue areas but not in others? Sharp shows that the structure of public opinion, the tangible and symbolic stakes posed by an issue, or the institutions that are charged with making public policy on different issues do not allow us to distinguish the 'thermostatic" issues of abortion and welfare from the others. For example, abortion appears to be an area in which public policy is quite responsive to public opinion (and vice versa), despite the fact that the federal courts have largely determined the limits within which federal and state governments may regulate the practice. Yet, the lack of a general explanation for the success of democratic policymaking in some areas, and its apparent failure in others, is no objection to Sharp's excellent book. The Sometime Connection seems likely to be an agenda-setting work that will provide the basis for innumerable articles and dissertations in the years to come. It is the first component of a continuing conversation, well begun but far from completed.

Choosing Justice: The Recruitment of State and Federal Judges. By Charles H. Sheldon and Linda S. Maule. Pullman, WA: Washington State University Press, 1997. 305p. \$35.00.

Susan B. Haire, University of Georgia

The role of the courts in the American political system places judges in a precarious position. Those sitting on the bench are expected to decide cases without being influenced by the political context, yet they may pay a heavy price if their

decisions depart substantially from the preferences of the majority. The extent to which judges' decision making reconciles these competing demands is often linked by scholars to judicial selection. Recruitment processes that are more democratic in nature, such as elections, are believed to contribute to accountability in the judiciary, whereas selection methods that are insulated from the voting public should maximize the potential for judicial independence. Yet, accountability seems tenuous in most elective systems because voters know very little about judicial candidates. Likewise, judicial independence may not be attainable in those appointive systems in which executives select close political allies to fill vacancies on the bench. Rather than abandon a scholarly pursuit of these sketchy connections between recruitment and decision making, Charles Sheldon and Linda Maule bravely sort out these relationships in Choosing Justice. As a result of their efforts, scholars now have available a conceptual framework with the potential to advance our understanding of selection processes and judicial roles.

Sheldon and Maule emphasize that it is not the method of selection per se that determines the connection to judicial accountability and independence. Instead, their central contention is that "judges who survive a high articulation (recruitment) process will tend to view their judicial role from a more balanced viewpoint where both accountability and independence shape that role. In contrast, a low articulation process will elevate ... [those who] ... assume roles nearer the extremes" (p. 30). To model this perspective, Sheldon and Maule begin by identifying the three-step sequence common to all recruitment systems: initiation of candidates, screening by formal and informal participants, and affirmation of those selected. Within each step, the authors suggest there is variation in the range, number, and effectiveness of participants. Furthermore, they argue that this variation ultimately determines whether the level of articulation for a selection method may be characterized as high or low. High articulation recruitment is marked by contested, competitive seats and high participation by a wide variety of actors throughout the process. The absence of these characteristics signals a low articulation recruitment. They find that some types of selection systems, such as nonpartisan elections, lead to a high articulation recruitment when formal and informal mechanisms encourage effective participation at each step. Sheldon and Maule also suggest that variation underlies each selection method type. For example, in states in which judges are appointed by the governor, a low articulation process may result if the governor relies on recommendations from a few close advisers. In contrast, a high articulation process will characterize recruitment if the governor consults with a variety of groups and individuals before making the appointment. Both types of states may share a similar judicial selection method, but the level of articulation varies as a result of the participants involved in recruitment.

As these examples illustrate, the appeal of the Sheldon and Maule approach lies in the effort to conceptualize judicial selection as a recruitment process rather than a classification that focuses on the means of affirmation for those selected. By adopting a broader view and using an articulation model, the authors recast our traditional understanding of judicial selection. For example, conventional wisdom suggests that judges will be responsive to the voters in contested partisan elections. Yet, Sheldon and Maule's analysis of the Texas recruitment process suggests the level of articulation is low, even in contested elections, because political parties dominate the initiation, screening, and affirmation stages. These judges may be accountable, but their responsiveness will be focused on the party faithful who contributed to their suc-

cessful election. Sheldon and Maule also broaden the scope of traditional approaches to the study of selection systems by emphasizing the importance of both formal and informal institutions in the recruitment process, particularly during the screening phase. It is not surprising that they find the organized legal profession to be an increasingly effective group of participants. Their analysis makes it clear that bar associations have worked to establish a screening role through their evaluations of potential candidates for vacancies in most judicial selection systems.

Sheldon and Maule also bring new insight into this line of research by including federal judicial systems in their analysis. At the Supreme Court level, they analyze the nomination of Ruth Bader Ginsburg to illustrate the applicability of the articulation model. They suggest a high articulation process characterized her nomination due to increased attention to Supreme Court nominations in recent years, the sensitivity of the Clinton administration to wide-ranging interests, and the concern for a confirmable candidate. The authors find that these dynamics contributed to the selection of a justice with a balanced perspective. Their analysis of federal judicial recruitment also uses the articulation model to illustrate differences across court levels. For example, participants involved in the recruitment of district court candidates are limited to a few actors, most notably home-state senators. In contrast, many more actors are involved in the selection of Court of Appeals candidates. The authors note the consequence of this difference: "Most district court candidates will not experience high articulation recruitment processes that will socialize them to appreciate the need to counterbalance the expectations of impartiality and independence against the demands of political and public accountability. Instead, they will go through low articulation experiences, where the few effective actors involved will either emphasize political accountability . . . or judicial impartiality" (p. 203).

Although the authors clearly establish the logic underlying a relationship between judicial recruitment and orientation, their work does not provide substantial empirical support for the premise. They rely heavily on the scholarly literature from the past three decades, particularly works published in the 1970s, that are suggestive but do not directly tackle their central thesis. Sheldon and Maule also provide case studies of individual states or, in the case of federal recruitment, individual jurists. These case studies are helpful in illustrating the concepts suggested by this approach. Perhaps the strongest support for their perspective can be found in the thorough analysis of Washington's nonpartisan election system (much of which is located in the appendix). After documenting the extensive involvement of a variety of groups in the screening process for Washington's judicial candidates, the authors gauge the effectiveness of these participants by using survey data on voters' sources of information in judicial races. In addition, they test for the critical link to judicial roles by surveying candidates' views toward accountability and independence, and they find some support for a relationship between candidates' perceptions of articulation levels and judicial roles.

Overall, Choosing Justice is an invaluable resource for scholars interested in judicial selection. In addition to the rich descriptive accounts of recruitment processes, this work provides an ambitious blueprint for future studies on selection systems, particularly those that will examine more directly the connections between levels of articulation and judicial roles. The opportunities for this line of empirical research should be plentiful, as there is variation across state and federal court systems as well as change over time in the

number and effectiveness of participants involved in recruitment processes for all courts.

Uncertain Guardians: The News Media as a Political Institution. By Bartholomew H. Sparrow. Baltimore: Johns Hopkins University Press, 1999. 277p. \$48.00 cloth, \$17.95 paper.

Kathleen Hall Jamieson, University of Pennsylvania

Bartholomew Sparrow rejects the "fourth estate" model for an open systems approach (p. 4) to the news media. This approach is predicated on the assumption that in the uncertain environment in which they operate the news media find stability in links to established actors, including advertisers, audiences, politicians, and governmental officials. In the process the large news organizations standardize the practices by which they cover national politics. They come to function, as the title suggests, as a political institution. All this is, of course, standard fare.

The value of *Uncertain Guardians* comes not in its originality but in the intelligence with which Sparrow has woven together his case and the insight inherent in the metaphors he marshals to organize his arguments. For example: "Institutions are often nested—existing on different, mutually compatible levels, like the Russian wooden dolls" (p. 132). Or: "The news media are an aggregate actor in the way that six aircraft flying in formation may be considered a distinct entity" (p. 133). Occasionally the impulse to employ the vivid metaphor translates into hyperbole, however. For example, Sparrow writes that "the media are not merely umpires but hanging judges" (p. 49).

The book's weakness resides in its tendency to overstep the evidence, its inattention to small but important details, its uncritical treatment of such topics as civic journalism, and its failure to integrate the Internet as a news source into its theoretical framework. The tendency to overstate is evident in such claims as "the news media often . . . violate their own ethical standards and, indeed, the norms of democratic government" (p. 5). The book does note the standard instances in which the news media violate their own ethical standards (e.g., the "accidental" gas tank explosion on Dateline [p. 128]) and, some would argue, the norms of democratic government (e.g., buying into the Bush administration's view of the Gulf War [pp. 141-4] and uncritically embracing the U.S. government's official account of the downing of KAL 007 [pp. 144-52]), but Sparrow fails to establish that such behaviors occur "often" or are typical or widespread.

There is a similar problem with the claim that "many network correspondents and news anchors—such as Brit Hume, Dan Rather, Sam Donaldson, and Ted Koppelroutinely engage in editorializing in ostensibly straight, on-air news stories" (p. 51). The footnoted documentation does not provide evidence to support "routinely." It also does not justify the additional comment that "Tom Brokaw, Bernard Shaw, Christine Amanpour, and others, to be sure, are much less ironic or sarcastic in their reporting" (p. 224). Although he calls for a more critical stance by reporters and citizens alike, Sparrow is guilty of a one-sided examination of civic journalism (pp. 190-6). He criticizes the critics of civic journalism for failing to explore the "serious issues" (p. 195) underlying its advocacy, but he fails to explore the serious issues raised by the critics themselves. Those who see it as a worrisome alternative deserve to have their reasons represented.

Also, the section titled "How the Business of News Affects Political Communication" should not inform readers that "it is clearly in the constitutional rights of publishers and broadcasters to choose only the ads they wish to carry" (p. 78). The courts have held that candidates for federal elective office have a right to access. "It is the purpose of the First Amendment," wrote the Court in the famous Red Lion case, "to preserve an uninhibited marketplace of ideas in which truth will ultimately prevail, rather than to countenance monopolization of that market, whether it be by the Government itself or a private licensee." Section 312(a)(7) added by Congress to FCC regulations in 1972 states: "The Commission may revoke any station license or construction permit for willful or repeated failure to allow reasonable access to or permit the purchase of reasonable amounts of time for the use of a broadcasting station by a legally qualified candidate for Federal elective office on behalf of his candidacy."

At times the claims of the book simply seem strange. Included in the fabrications for which the American news media are indicted is Bush's statement that the fact Clarence Thomas "is black and a minority has nothing to do with this sense that he is the best qualified at this time" (p. 193). Sparrow surely does not mean to imply that the press should have ignored a statement by the president about a nominee for the Supreme Court? Perhaps he does not believe that the press treated Bush's claim with sufficient skepticism? Yet, it is from press reports that the public learned about Thomas's limited experience, the reservations expressed by fellow lawyers about the appointment, and that Thomas had been the beneficiary of the sorts of affirmative action of which he disapproved.

At other points Sparrow finds fault with news decisions that I consider exemplary. In the chapter "The Watchdogs that Didn't Bark," he criticizes the news media for their "failure to report on fundamental issues of public health in their coverage of a Canadian biologist's . . . development of a cure for cancer" (p. 166). Although he recognizes that "Nacssens has not yet conducted the randomized, doubleblind, controlled experiments that would confirm his research to the scientific community" and has not "contributed to scholarly journals" (p. 168), Sparrow objects that "Naessens has not been mentioned in most of the leading U.S. media" (p. 169). As a result, he concludes that "the general welfare of the American public and the advancement of biological science and medicine have been neglected" (p. 174). The person who is neglecting the advancement of biological science and medicine is Naessens. If he has a cure, then he should demonstrate that fact in controlled experiments and publish the results in refereed medical journals. Until that is done, it would be irresponsible for the press to attach the word "cure" to his hypothesis or needlessly to raise the hopes of those with cancer.

With its focus on print, cable, and broadcast news, this is largely a book about the past not the future. By covering the Internet in a few concluding pages, Sparrow minimizes his ability to integrate this news source into a coherent theoretical framework. He notes that General Electric owns NBC but does not explore the extent to which its media practices are changed by its cable and Internet news joint venture with Microsoft, MSNBC, or by its role as controlling shareholder in the search site Snap.com, which is operated by NBCi. G.E. also holds a large stake in Value Vision International, a cable shopping channel. Given these entanglements, how fair and extensive was NBC's coverage of the government's recent actions against Microsoft or its reporting on the dispute between privacy advocates and business concerns about disclosures of individuals' banking transactions to other businesses? Have NBC's ties influenced its reporting on financial news? The questions extend beyond conflicts of

interest. Does NBC's control over auxiliary channels of communication increase the likelihood that such important democratic rituals as party conventions will be carried not on broadcast but on channels requiring subscription fees?

In sum, Uncertain Guardians wraps a synthesis of a wide body of research from political science and mass communication in a number of tantalizing metaphors in service of an argument that few will find controversial. What makes it a less than ideal text is its tendency to overstep the evidence, its lack of attention to small but significant details, its uncritical advocacy of a controversial alternative to traditional journalism, and its failure to integrate the Internet tightly into its analysis. Finally, the utility of the book for scholars would have been increased by the inclusion of a bibliography.

Campaign Finance in State Legislative Elections. Edited by Joel A. Thompson and Gary F. Moncrief. Washington, DC: Congressional Quarterly Books, 1998. 249p. \$47.95 cloth, \$28.95 paper.

Frank J. Sorauf, University of Minnesota

No serious scholar of American campaign finance or of American legislative elections should even consider ignoring this book. It reports a pathbreaking first study of the funding of legislative campaigns—for the lower houses only—in eighteen American states in the 1980s and 1990s. It is, in short, required reading.

A project of such scope has many parents. Thompson and Moncrief, in addition to overall responsibilities, write individually or jointly in five of the eleven chapters. Six additional authors write in various combinations in ten of the chapters. One of these, Malcolm Jewell, was the graduate teacher of Thompson and Moncrief as well as three other authors. The book is, therefore, testimony to his enormous influence, as scholar and teacher, on the study of American state politics.

The plan of attack is straightforward, touching all aspects of legislative campaign finance: the sources of the money, expenditure levels and trend lines, incumbent advantage and challenger poverty, the roles of PACs and party committees, the effect of money on election outcomes, and the financing success of women and minority candidates. The study does not, however, extend to attempts to relate campaign funding to outcomes in the legislatures themselves. The eighteen states are, not surprisingly, states with available data. Despite that constraint, they are reasonably representative with one exception: The South and Southwest, where many states lack full reporting requirements or agencies that collect and publish usable data, are underrepresented.

Clear, no-nonsense, monographic prose prevails throughout. There are few rhetorical flourishes, but there are no dense thickets to fight one's way through. Methods, procedures, data, and conclusions are lucidly set out, and readers are spared methodological diversions and self-indulgence. The focus is on the substance of the inquiry. The result is a book that will be accessible to serious undergraduates, all political scientists, and interested citizens—no matter how methodologically challenged.

Inevitably in a work of such complex collaboration there is some repetition and a few inconsistencies, but much less than one might expect from a book written by eight authors. The only substantial fall from grace is an index beset by style inconsistencies (first name or initial of authors?), some errors (e.g., missing page notations), and, most serious, the decision not to index author references within parentheses. That latter omission reduces the book's usefulness to scholars and, as

one wise editor once told me, surrenders a proven marketing device!

And what of the substance—the findings, the explanations, the theoretical contributions? Not unexpectedly, the study confirms much of what we have learned from the analysis of congressional campaign finance. State legislative incumbents greatly outspend their challengers; party committees are the only sources of money consistently committed to helping those challengers; and competitive elections, especially those with no incumbents running, boost the aggregate cost of the campaigns.

Yet, there are significant differences among the states in these and many other matters. Even leaving California aside—with 400,000 people in each of its Assembly districts, its campaigns are almost in the U.S. congressional league—there are substantial variations in sheer sums raised and spent, in the funding roles of party committees, and in the level of PAC activity, for example. Significantly, these and other differences appear to be related to the degree of legislative professionalism. The state legislatures that most approach the elaborate campaigning we associate with congressional campaigns are those with longer sessions, larger staffs, more perks, and longer incumbency. In them, presumably, the electoral prize to be won is greater, and the office itself nourishes the electoral support, group contacts, and skills for large-scale campaigns.

Beyond that pervasive finding, there are also bodies of fresh, new data. The chapter on the funding of campaigns in primaries provides one of the first comparative looks at a subject long impeded by lack of interest and severe data problems. Likewise, the chapter on the funding of minority candidates in the states—limited to California, Illinois, New Jersey, and Pennsylvania—advances work on that topic by confirming the funding problems minority candidates face in majority white, non-Hispanic districts.

If I have any reservations about the analysis, they center on the relative absence of institutional and legal variables. For example, regulatory provisions (e.g., contribution limits) do not appear as independent variables or controls. The state laws that permit direct corporate or labor contributions—without the necessity of forming PACs—do not modify conclusions about PAC contributions. Moreover, although there are explanatory references to public funding in Wisconsin and Minnesota, there is no systematic analysis of the effect of that funding. Granted that an N of two is daunting, there are important differences between the two programs, and they affect candidate financing in each state differently.

The concluding chapter, a summing up with implications for reform options, does what it sets out to do very well. But some of the summary is repetitive, and the observations on reform surprisingly ignore this decade's spate of campaign finance reform in the states. My own preference would have been for a conclusion more willing to get at some of the theoretical issues that emerge from the analysis, such as the implications of the importance of legislative professionalism. Do public offices create or shape the political cultures that determine their campaign finance, especially their level of spending? Anthony Gierzynski has in fact nicely presaged such a theoretical exploration with his second chapter.

Going first is always hard. The first extensive study of any aspect of the American states cannot tackle everything—what constitutes "everything" may not even be clear until after that first study. In this instance the authors have made a superb start at unraveling one of the great unknowns in state politics. To be sure, they have left projects for themselves and their successors to pursue, but much of the status quo and its explanations are now apparent. One hopes they

will, individually or collectively, press on. Others should not hesitate to join them.

American Virtues: Thomas Jefferson on the Character of a Free People. By Jean M. Yarbrough. Lawrence: University of Kansas Press, 1998. 256p. \$35.00.

Ralph Ketcham, Syracuse University

The mind of Thomas Jefferson, recorded in his voluminous writings and papers, is and has been, ever since he began taking notes on his studies and writing public and private papers before the American Revolution, the source of intense and stimulating interest to students of American thought and, indeed, for more than two centuries, to theorists and public figures around the world interested in democracy and self-government. Jefferson's eminent biographer, Dumas Malone, once remarked that what distinguished Jefferson and some other notable founders from most contemporary political leaders was that "they knew more about the past, and thought more about the future." It is the depth and richness of this thought and Yarbrough's learned, analytical, and incisive handling of it that make this book, even among the dozen or more volumes on Jefferson's ideas published in the last decade or so alone, a welcome addition to a crowded field.

The special merit of this book is its thorough, traditional search for and analytical treatment of Jefferson's philosophy. Yarbrough takes Jefferson's intellectual millieu straight: She uses terms much as he did, giving reality to such words as virtue, character, moral sense, republican, and even God. Thus, when Yarbrough discusses Jefferson's connection of civic virtue with the idea of self-government, she accepts that there can be some real meaning to the concept of civic virtue, and that one can discuss the questions of good government and self-government in ways intelligible and constructive to modern (and postmodern) students and political leaders alike. The reader does not have to push through presentminded political or legal critiques, or through repudiation of Jefferson's "metanarratives" or universals, or through twentieth-century psychologizing in order to find the eighteenthcentury thinker. His words, and a learned explanation of what his ideas were, are there, as first hand as is possible two centuries later. When Yarbrough explains that Jefferson "understands that the preservation of republicanism does not depend primarily on laws or mere institutional arrangements but on the spirit of a people nourished in the habits of self-government" (p. 135), that he "believes that republican families are essential to the formation of republican citizens and that women are the pillars of the republican family" (p. 132), and that he "anticipates Tocqueville's observation that local self-government helps mitigate the corrosive effects of commerce and radical individualism" (p. 138), we can engage directly with what Jefferson took to be "the character of a free people." The result is a searching, unapologetic exposition of Jefferson's views on human nature, on justice and the good society, and on the purpose of government, in the conventional language of political inquiry.

Yarbrough is able to perform this useful service, of course, because of her own deep understanding of 2,500 years of Western political thought and her skill in handling traditional concepts. She makes use of the commentaries of students of political ideas from Montesquieu and Tocqueville to Hannah Arendt and Paul Rahe, but even more, she is well-versed in the ancient, medieval, and modern thinkers themselves who constituted Jefferson's intellectual world. Yarbrough places Jefferson firmly in the school of Lord Kames and other

Scottish philosophers who emphasized, as Jefferson himself put it, that since "man was destined for society," he was given a "moral sense, or conscience..., a sense of right and wrong... as much a part of his nature as the sense of hearing, seeing, feeling" (to Peter Carr, August 10, 1787; quoted on pp. 34–5) Yarbrough sees that this more elaborates Jefferson's standing with his "trinity of immortals"—Francis Bacon, Isaac Newton, and John Locke—than separates him from them.

Jefferson was always the Enlightenment intellectual, confident in the sway of order, morality, and reason in the world and ready to apply these precepts to every aspect of life, character, and culture in the new nation whose independence he had declared in 1776. He believed, Yarbrough explains, that "the moral sense, acting together with unassisted human reason, can discover . . . moral sentiments [that] are natural and universal...All human beings, in every society, are motivated by feelings of sympathy, justice, gratitude, benevolence, and friendship" (p. 193). Jefferson did not believe, of course, that all people always displayed these qualities, or that a political majority could create them; rather, "they are inscribed in human nature by a Benevolent Creator so that human beings everywhere can develop the moral and intellectual capacity for self-government (p. 193). Whatever modern or postmodern critical analysis might make of such sentiments, Jefferson expressed them cogently and plausibly (and idealistically) for the first generation of American leaders and citizens. As Yarbrough points out, however, Jefferson's is a "moral vision" of the "shared principles of the political community,...completely foreign...to the 'expressive individualism' so characteristic of Thoreau, Emerson, and Whitman in America and of J.S. Mill in England" (p. 193).

In a volume that stands as the best exposition we have of Jefferson's thought, especially his political thought, one finds nonetheless some things to complain about. In acknowledging and explaining carefully Jefferson's deep classicism, especially his debt to Epicurius and the Roman moralists, for example, Yarbrough sees too much separation between the disinterested, self-sacrificial notion of citizenship of the ancients and Jefferson's own powerful, private-oriented inclination to avoid public office. This overlooks both the proforma, politically coy nature of Jefferson's professions of preference for farm, family, and friends (he never for long refused to return to public office and, indeed, often sought it for reasons as patriotic as those of Pericles himself) as well as his ready blending of private and public friendship. When he pointed out to James Madison "the friendship which has subsisted between us now half a century, and the harmony of our political principles and pursuits" (February 17, 1826; quoted on p. 172), he was asserting the merging in his own mind of the loyal, devoted, and unselfish qualities necessary to both private virtue (friendship) and public virtue (citizenship).

Yarbrough sees the rather formal, generally "public" nature of the huge correspondence between Jefferson and Madison, with its lack of "the warmth and affection so evident in the late correspondence with [John] Adams" (p. 173), as proof that the political ("partisan") nature of the connection between the two Virginians was separated in Jefferson's mind from the personal virtues of private friendship. Rather, the difference in tone between the two correspondences could easily be explained by the fact that after Jefferson returned from France he often saw Madison daily—when they were in public office in the same city or traveling together—and probably otherwise semiannually in their exchange of visits to country houses only twenty-five miles apart; there were lots of opportunities for intimate, private talk. The elderly Adams

and Jefferson, in contrast, knew they would never meet again and thus had to express everything in letters. On important

matters, however, Yarbrough generally gets Jefferson just right in this profound, integrating, and important book.

Comparative Politics

Taking the Wheel: Auto Parts Firms and the Political Economy of Industrialization in Brazil. By Caren Addis. University Park: Pennsylvania State University Press, 1999. 257p. \$47.50 cloth, \$18.95 paper.

Peter R. Kingstone, University of Connecticut

The third chapter of Taking the Wheel begins with an anecdote related to the author by a Brazilian auto parts producer. A circus ringmaster desperately tries to make an elephant cry, telling tale after tale of woe. The elephant remains dry-eyed until a man in the audience whispers something in its ear. As the elephant finally yields a tear, the stunned ringmaster asks the man what he said, to which he replies: "I told him that I was a supplier for Ford" (p. 91). The anecdote pertains to the breakdown of the initial cooperative relationship between multinational assemblers and largely Brazilian suppliers. It also stands as a metaphor for the whole relationship between the two sectors since at least the 1960s. Over the sectors' first four decades, supply relations suffered from frequent betrayals and recriminations, intense lobbying and trickery, as well as continuous adjustments, particularly within the parts sector. In fact, Addis's account makes it easy for students of the recent neoliberal reform process to understand the current Brazilian context of brutal supplier-assembler relations and rapid and profound rationalization of the parts sector.

Nevertheless, Addis also shows how domestic auto parts procedures were able to put their stamp on the evolution of auto production in Brazil. The key to Addis's argument is that Brazilian auto parts producers, whose existence predates the initiation of assembly in Brazil, were able to forge ties to policymakers and assemblers and thereby shape the path of development in Brazil. Initially, policymakers had little interest in inviting assemblers to locate in Brazil and had little confidence that local conditions were adequate for assembly. Multinational assemblers were equally skeptical of the possibility of effective production in Brazil's backward economy. Yet, parts producers persisted and, combined with the efforts of key bureaucrats, finally convinced President Juscelino Kubitschek to make a priority Brazil's "modernization" through domestic production of the automobile.

As a consequence, small, technologically unsophisticated parts producers were able to shape the assembly process according to their preferences. These preferences led to an initial period in which a "horizontal image" of production was implanted, one that entailed mentoring of the parts producers by assemblers and long-term stable contracts for production of relatively small volumes that were well below standard beliefs about minimum economies of scale. Yet, over time, that initial cooperative, horizontal image unraveled in the face of several constantly changing pressures. Policymakers' preferences shifted, especially with the installation of military rule in 1964. Key bureaucratic agencies, notably the Executive Group on the Automotive Industry, were weakened, which weakened the input of parts producers. Assemblers' preferences shifted as production took off, luring new players and intensifying the participation of existing assemblers. Finally, economic uncertainty and volatile macroeconomic conditions led to continuous shifts in both the bargaining positions and strength of all the players involved. This pattern of shifting preferences and economic circumstances led to a continuous adjustment in the parts sector.

Ultimately, the horizontal vision gave way to a different pattern, including substantial rationalization in the parts sector and the emergence of two tiers of producers. The top tier, composed of multinational parts producers and large Brazilian producers (typically in some kind of association with multinationals), produced for assemblers and accounted for most exports. The bottom tier, typically smaller and technologically unsophisticated firms, produced largely for the domestic after-market. Meanwhile, the assemblers increasingly were able to assert their strength and impose their preferences on the market. Long-term stable contracts grew scarce as assemblers played suppliers against one another. Assemblers used promises of export promotion to undermine rules that protected the domestic parts producers. Mentoring gave way to both increasing levels of vertical integration and increasing numbers of multinationals who produced parts locally. The horizontal image, however, remained alive to the extent that assemblers continued to produce limited numbers of multiple models and platforms. This forced them to rely on flexible production practices more typical of Japanese methods than of American Fordist principles.

Addis's purpose in tracing this history is to debunk the notions of large-scale industrialization advanced by such theorists as A.O. Hirschman ("A Generalized Linkage Approach to Development, with Special Reference to Staples," Economic Development and Cultural Change 25 [1977 Supplement]) and common in typical accounts of the development of the auto industry, such as Helen Shapiro, Engines of Growth (1994). Addis also notes that these typical conceptions of large-scale industrialization are common as well among Brazilian policymakers. Thus, she situates her account within a new, revisionist current that has identified critical roles for small players in shaping Brazilian history.

Addis's account is very thorough, original, and persuasive; it is hard to argue with either the quality of the evidence or the care of her argument. It is possible, however, to raise some objections, two of which stand out. First, the author's interest in demonstrating the role of small actors leads her perhaps to downplay the role of major players. It is true that the outcome in Brazil deviates from large-scale Fordist principles. Yet, it is also true that assemblers very rapidly seized the upper hand in bargaining over the shape of the sector and seemed repeatedly to impose their preferences on the production and policy process (examples of which abound in Addis's analysis, such as Ford's role in the creation of BEFIEX, pp. 122-3). Similarly, by the 1970s the key players in the auto parts sector are no longer "small" per se. As Addis notes, in terms of political, organizational, and economic influence, the key players were the largest domestic producers and the large number of multinational producers in the sector (pp. 163-8). Granted, Metal Leve was not General Motors, but neither Metal Leve nor Robert Bosch were "small" actors.

Second, the book perhaps undersells itself theoretically. Addis is very concerned with critiquing large-scale notions of

industrialization and providing a corrective to emphases on the assemblers and the belief in the backward linkages that derive from them. This is certainly interesting, but Addis does not tell us much about the larger implications of this for development or for the study of political economy. That is unfortunate, because the book demonstrates much more than simply the inadequacy of large-scale industrialization as a framework for understanding Brazilian development. This incredibly rich account demonstrates concretely how market outcomes emerge from a process of constant negotiation both among market actors as well as between market actors and the state. Thus, the logic driving the specific character of industrialization is not technologically determined but instead reflects an interaction between markets and politics and the historical choices that shape production as well as the subsequent political conflicts. One reason this is important is that it teaches a crucial lesson about the political economy of economic reform in this era of neoliberalism. Addis's work is a crucial reminder that the current process of adjustment at the micro level reflects past conflicts, and future outcomes depend on how conflicts are resolved today. Among the many riches inside her book, this lesson alone makes Taking the Wheel valuable to students of development and political economy.

Women on the Defensive: Living through Conservative Times. By Sylvia Bashevkin. Chicago: University of Chicago Press, 1998. 318p. \$47.50 cloth, \$18.00 paper.

Janet K. Boles. Marquette University

Although contemporary feminism has been strongest in the Western industrial democracies, with their tradition of liberal civil rights, virtually every nation now has some type of women's movement. The literature suggests that U.S. feminism, with its professional interest groups at the national level and more activist local social movement groups, has greater adaptability to pursue multiple goals and shift priorities as needed. By this reasoning, U.S. feminists should have been insulated by a congressional system and a strategy of political independence from the challenge posed by the Reagan-Bush administrations.

Bashevkin successfully challenges this assumption of the greater success of U.S. feminism by presenting a comparative analysis of the women's movements of Canada, Great Britain, and the United States under Brian Mulroney, Margaret Thatcher, John Major, Ronald Reagan, and George Bush. Drawing upon interviews with more than 100 feminist activists and experts in the three countries (and their primary materials), she systematically traces the outcomes of national legislative and judicial decisions in five policy areas of concern to all movements: equal rights, family law, reproduction, violence, and employment. In each case she compares the success rate of women before the first conservative government took office with that during conservative rule. Although all three movements suffered setbacks, Canadian women best surmounted the conservative challenge; U.S. women took the harshest hits.

This study fills a significant gap in the social movement literature that is primarily focused on the origins of movements. Here is a look at three mature movements at a time when progress is typically slower and opposition greater and within the context of remarkably similar political opportunity structures. All three conservative governing regimes embraced a common idealogy of reduced government, antiwelfare, individual initiative, personal responsibility, family values, social conservatism, free trade, and devolution. For the

three movements, this required a paradigm shift from their premise on a positive role for government and reliance on bureaucracy, regulation, and intervention.

Women in all three nations lost economic ground in wages, jobs, and welfare; feminists were marginalized, isolated from their former allies, and stripped of their government advocacy appropriations and clientele agencies. But there were successes as well, and these are attributed to the intersection of ideas and institutions. U.S. feminists were disadvantaged by two presidents (with strong vetos) who deeply believed in conservative social policy that permeated all branches of government and the Washington, D.C., community, by a strong antifeminist movement, by the lack of constitutional equal rights protection, and by a more politicized judiciary. British feminists could draw upon supportive European Court of Justice rulings on employment and an autonomous judiciary. Likewise, Canadian feminists could rely upon a Mulroney government committed to free trade and indifferent to social policy, constitutional equal rights and affirmative action provisions, employment policy dominated by the provinces, and judicial holdovers from the Trudeau era.

Those who use either the political opportunity structure or resource mobilization perspectives on social movements are provided with a convincing refutation of the ascendancy of U.S. feminism, but Bashevkin provides no encompassing theory of women's movements and their life-cycles or predicted successes as a replacement. Instead, her many findings and observations will resonate with scholars as highly suggestive for middle-range theory. For example, the multiple-crack access points of federalism (and a state-focused organizational structure) appear to have served U.S. and Canadian feminists well in maintaining past victories. The U.S. tradition of private giving insulated women by filling their coffers to unprecedented levels. It is also possible that an equal rights feminism is better situated to withstand economic conservatism. In all three nations, membership in feminist groups rose, public opinion support grew, and more women entered elected office and the cabinet.

The study further contributes to the current dialogue on globalization. Bashevkin suggests that we may be witnessing the globalization of economics, politics, and feminism as Democrats, Liberals, and Labourites campaign from the Left, govern from the Right, converge toward the opposition party and the center, and assume the "permanent campaign" mode of U.S. parties. As a result, through 1997, few real policy achievements for women have occurred with the defeat of conservatism beyond a softer, gentler political rhetoric. In turn, British feminists have converged toward the pragmatic reformist U.S. model by lobbying more, contesting elective office, and courting media coverage. Canadian feminists have added protest to their range of strategies. All recognize that neither protest nor officeholding is sufficient for women's political incorporation.

In summary, this pathbreaking book is certain to stimulate debate and further research by scholars in several fields: social movements, comparative politics, and women's studies.

The Gorbachev Factor. By Archie Brown. Oxford: Oxford University Press, 1997. 406p. \$16.95 paper.

John P. Willerton, University of Arizona

Nearly ten years after the collapse of the USSR, judgments about both Mikhail Gorbachev's intentions and the consequences of his six-year tenure as Soviet leader are extremely varied. The previous enthusiasm and generally positive readings of his policy efforts have given way to mixed—and

occasionally quite negative—assessments of his reform legacy. Rejected by most in his native Russia, Gorbachev has become a symbol for an era of unfulfilled dreams and widespread regrets. Today, as Russia is mired in the complex and often tedious challenges of post-Soviet economic transformation, Archie Brown provides a needed reminder of the tensions, excitement, and high drama of the fast-paced perestroika years. Exhaustive and carefully grounded in an impressive diversity of sources (with more than 70 pages of detailed notes and supporting documentation), this volume provides an analysis that will prove to be among the most definitive when future students of politics turn to the final years of the Soviet system.

Reactions to Brown's volume are certain to vary considerably among interested observers. The core thesis—that Gorbachev was seriously interested in political change and economic reform but came to understand that reform was not enough and that the political system would have to be transformed—will provoke a range of reactions. Brown, however, has long studied Gorbachev, and the case he makes that Gorbachev was an important positive factor in the transformation of Soviet Russia is compelling. The author may well be sympathetic to Gorbachev's intentions and efforts, but his analysis is straightforward and must be considered by any serious student of the late Soviet period in Russian politics.

Brown effectively places Gorbachev in the context of a complex constellation of Soviet institutions, rules, and politicians. The policy dilemmas increasingly evident in the Brezhnev era greatly influenced Gorbachev's perspectives as he ascended into the upper echelons of the Soviet leadership. Brown demonstrates that these serious reformist inclinations were already apparent by 1984, before Gorbachev became general secretary. Yet, Brown presents a Gorbachev who was savvy in his own career building, who was effective in navigating the tricky waters of building alliances while expanding his own power base. He also presents a Gorbachev who was very effective in manipulating the official ideology to legitimate profound change in all major policy domains. The resultant analysis is thus multifaceted. Brown ties Gorbachevian reformism to Gorbachev's past and his own preferences, but that reformism is also tied to the team of allies and advisers who were cultivated by Gorbachev and to ideological developments that were contoured to legitimate his reformist agenda. Thus, the "Gorbachev factor" far transcends a single individual, as it reflects the influence of an evolving cohort of elites and a dynamic and changing institutional and policy environment.

Brown appropriately organizes the 1985-91 reform efforts into four major policy domains: the political system, the economy, foreign policy, and nationalities. He is especially detailed and effective in describing the domestic political reforms and changes in foreign policy ("new thinking") at the heart of Gorbachev's positive legacy. Brown's study makes clear that the most profound step taken by Gorbachev in transforming the USSR was to introduce the concept of contested secret ballot elections to a new legislature. Publicly proposed in June 1998 and applied less than a year later, this and related political changes fundamentally altered the functioning of the party-state system, made political elites and the Communist Party more accountable, and permitted the already more open public debate to widen and increasingly structure decision making. Meanwhile, foreign policy innovations permitted a fundamental change in Soviet Russia's international position, and the resultant diminishing of external obligations facilitated a more serious consideration of profound domestic economic reform.

Brown is less detailed in dealing with the economic and nationalities dilemmas, but he acknowledges both the system constraints and mistakes in Gorbachev's calculations that precluded policy success. Gorbachev's underestimation of the nationalities conundrum, his retreat from the so-called 500 days economic reform program, and his turn to the conservative right in 1990–91 are among the major miscalculations of the Soviet leader discussed in the volume. Brown emphasizes both the intractability of some policy areas (e.g., the nationalities question) and the effect of confounding bureaucratic obstacles in other areas (e.g., implementation of rootand-branch economic reform).

In assessing the intent, direction, and pace of Gorbachevian policies, different observers can offer alternative interpretations, and reactions to Brown's analysis and conclusions will vary. Some could emphasize limitations in Gorbachev's thinking or a lack of will on his part to apply more fundamental policy changes. I am inclined to agree with Brown. Similar to Brown, I would emphasize the powerful institutional realities within which Gorbachev was operating as he struggled to retain power while promoting his reform agenda. But this is largely a matter of interpretation, and one can appreciate that others would weight the array of explanatory factors—including the Gorbachev factor—differently and draw alternative conclusions.

Our understanding of the developments that led to the collapse of the USSR and Soviet bloc and the transformation of Russia will continue to evolve, and scholarly interpretations are likely to remain very divided. All observers, however, will need to address the role and influence of Mikhail Gorbachev as this profound set of events unfolded, and in doing so a reading of Brown's important study will be a necessary requirement.

Subversive Institutions: The Design and the Destruction of Socialism and the State. By Valerie Bunce. Cambridge: Cambridge University Press, 1999. 206p. \$54.95 cloth, \$19.95 paper.

Postsocialist Pathways: Transforming Politics and Property in East Central Europe. By David Stark and László Bruszt. Cambridge: Cambridge University Press, 1998. 284p. \$55.95 cloth, \$22.95 paper.

Michael Bernhard, Pennsylvania State University

These are two of the most important books on developments in Europe east of the Elbe since 1989. Both are based on deep knowledge of the region and are grounded solidly in contemporary social science theory. Most significantly, both present interesting new theories about what has transpired or is transpiring in postcommunist Europe.

Subversive Institutions explains the destruction of communist regimes, the unraveling of the Soviet block, and disintegration of the multinational states in the region as a product of the pathological design of communist institutions. What is positively pathbreaking about the book is the way in which it weaves together two heretofore disparate strands of the literature on postcommunist political development. Almost all other theories of the communist collapse have seen the disintegration of regime and state as different processes with different causes. Bunce's great innovation is to explain both with reference to the same independent variable, the design of Soviet-type institutions.

Bunce's argument is structured around a series of interrelated questions. First, why did Soviet-type socialism collapse? Why did the Soviet bloc as a whole collapse? Why did three multinational socialist states—the USSR, Yugoslavia, and

Czecho-Slovakia—also disintegrate as states? And why was the disintegration in Yugoslavia so violent in contrast to the other two cases? Bunce explains all this with reference to two independent variables: Soviet-type institutions and changes in the political opportunity structures within socialist societies, or what she provocatively describes as "subversive institutions interacting with subversive opportunities" (p. 88). The method she uses is historical institutionalism, with a series of small-n comparisons providing the basis for logical inference.

Bunce begins the discussion of institutions with an assessment of socialism in its Stalinist incarnation. In this phase, the state was strong, society was weak, and the mobilization economy generated robust growth. Over time, these relationships changed—the state weakened, society strengthened, and the economy began to perform poorly. In brief, although I do not do justice to the subtlety of Bunce's argument, the major reason for this was the replacement of coercion as the primary means of control with economic incentives. This transferred resources away from the center, divided the elite along functional lines, and both homogenized and empowered society. Communist leaders faced an increasingly discomforting critical choice: Reform the system (potentially undermining socialism) or attempt to placate constituencies in society by economic means while the economy was in decline.

In addition to these institutional changes, a confluence of developments in the 1980s brought about a radical change in the political opportunity structure throughout the bloc. There were (1) succession struggles in the Soviet Union and several other states; (2) the embarkation of the USSR on a program of great reforms under Gorbachev (by no means an inevitable outcome) and the political struggles that accompanied this in all European socialist states; and (3) changes in the international environment (specifically, the changed climate on human rights caused by the Helsinki process and the opening of East European economics to the world economy as a substitute for economic reform). All these worked further to empower society and fragment the regime.

Bunce also uses institutional analysis to understand why certain multinational states in the region disintegrated but others did not. She considers a number of theories and finds that comparison of the three cases leaves all of them wanting as comprehensive explanations. This discussion leaves her with one necessary condition for disintegration (national minorities located in concentrated geographic areas), but the answer to this question is also institutional. What these three states shared and no other in the region had was a federal structure.

Federalism contributed to the disintegration of states in three ways. First, it promoted national consciousness by encouraging the construction of nations (e.g., certain forms of national consciousness were encouraged under socialism—Moldovan, Macedonian, Belarusan), while other more established nationalisms were reinforced (Slovakian, Baltic, Croatian, Serbian). Second, the existence of republican institutions provided the basis for nascent states as well as resources for nationalist challengers to the center. Third, the state itself was highly dependent on the regime and its central structures. The party was the glue that held the federations together; when the party disintegrated, so did the state itself.

The final question Bunce addresses is why Yugoslavia disintegrated so violently. She compares the institutions of federalism there with those in Czecho-Slovakia and the USSR. First, Yugoslavia was exceptional in that it was the most decentralized of the federations, and this promoted an unusually contentious relationship among the republics even

under socialism. Second, whereas the Czecho-Slovak and Soviet militaries remained quiescent and subject to civilian control despite regime and state collapse, the Yugoslav National Army had long been an important actor in domestic politics and saw itself as "the guardian of socialism and the state" (p. 121). Third, in Yugoslavia, the dominant republic, Serbia, was in a politically weak position within the federation yet had powerful institutional resources at its disposal. Thus, it had grievances against the other republics and the means to pursue them. In the USSR and Czecho-Slovakia, the dominant nationalities, the Russians and Czechs, held the preponderant political position in the federation, yet they did not have independent institutional resources (neither Russia nor the Czech lands had republic-level institutions like Slovakia or the fourteen other Soviet republics).

Postsocialist Pathways, a collaboration of American economic sociologist David Stark and Hungarian political scientist László Bruszt, presents a highly original theory of the economic and political transformation of East Central Europe. The book is most strongly focused on Hungary, which Stark and Bruszt know best, but uses Poland, the Czech Republic, Slovakia, and East Germany as foils for comparative analysis. The first part of the book is a brief pathdependent comparison of the extrication from communism of Poland and Hungary. Along with the work of Rudolf Tokés, Hungary's Negotiated Revolution (1996), and Patrick O'Neil, Revolution from Within (1998), this section is one of the rare theoretically informed and empirically accurate accounts of the transition in Hungary. Despite the strength of this section, it is the second and third sections of the book, on the economic transformation, that represent the most unique contribution.

This study establishes Stark and Bruszt as two of the most trenchant critics of both neoliberal market-utopian theories of economic transformation and neostatist critics who advocate a strong, sometimes even authoritarian, developmental state. They reject the notion that there are ready-made recipes for moving from Soviet-type economic systems to capitalist market systems either on the basis of a neat neoclassical model or by copying the practices of the East Asian NICs. Instead, they argue forcefully that successful transformations must be based on the reconfiguration of economic elements already present, rather than on "building from scratch." These assumptions allow them to develop a much more path-dependent account of economic reform.

Their account also rejects zero-sum thinking on questions of markets versus states (either strong markets or a strong state, but not both). Instead, the authors argue that successful transformation requires both strong markets and a strong state. They also make the case that capitalist transformation should proceed institutionally from a point of strength. The most significant asset they identify in this regard are networks of interdependence (ownership, debt, supply), which distribute the uncertainties of transformation across firms. On the basis of some very persistent and imaginative fieldwork, they present several examples of this sort of networking in Hungary and the Czech Republic.

Proceeding from networks, however, will not necessarily guarantee success. Building on Peter Evans's work, Embedded Autonomy (1995), Stark and Bruszt maintain that network restructuring needs to be embedded in wideranging processes of social deliberation, so that reformers have access to the sort of information necessary for a successful transformation. They demonstrate the painful consequences of acting according to fixed notions of reform without consultation by

a comparison of the Hungarian, East German, and Czech processes of reform. They show how the doctrinaire implementation of reform in Hungary (the bankruptcy law) and East Germany (rapid opening of the economy) created severe, and unnecessary, economic shocks. In the Hungarian case this led to a rapid see-sawing back toward state intervention with concomitant strains put on the state budget. In East Germany, the state bureaucrats responsible for privatization, in concert with local politicians, came to recognize the network properties of the economy and avoided outright disaster.

The most curious of the cases is the Czech. Stark and Bruszt show that, despite Vaclav Klaus's free-market rhetoric, the Czechs have moved most cautiously, recognizing the interdependence of firms in their economy. They highlight the lack of a strong executive as an essential part of the more deliberative nature of Czech practice. This stands in strong contrast to the many observers who identify concentrated executive power as central to successful reform. Critics of their approach will, undoubtedly, point out the Polish case as one in which more cookbook-type approaches to transformation have worked. In this sense, the absence of an accounting of the Polish case is unfortunate, because Stark and Bruszt probably have a different view.

One weakness of Postsocialist Pathways is no consideration of the broader ramifications of transformation in property relations. Clearly, these processes have critical implications for such issues as the distribution of economic resources and political power in the new democracies of Central Europe. Furthermore, in all countries the issue of nomenklatura privatization is embedded in issues of social justice, coming to terms with the past, and even the legitimacy of the transformation of the last ten years. Because of this, the last chapter, "Extended Accountability," can sound a bit utopian at points.

Subversive Institutions and Postsocialist Pathways both demonstrate that a detailed knowledge of place is an excellent starting point for creating powerful social science theory. Both works go about it in different ways. Bunce proceeds from a broad knowledge of a moderate number of cases, compares them, and derives a causal theory of an historical event (the collapse of communism) to enhance our understanding of several contentious issues in social science, such as revolution, nationalism, the role of institutions, and the place of area in the practice of comparative politics. Stark and Bruszt take a much more microlevel approach to deflate widely held but substantially inaccurate impressions of what economic reform in East Central Europe has entailed. In doing so, they create or elaborate social science concepts that allow us to better understand this process (e.g., transformation, networks, deliberative association).

Comparative Public Policy: Patterns of Post-War Transformation. By Francis G. Castles. Cheltenham, UK, and Northampton, MA: Edward Elgar, 1998. 352p. \$53.00 cloth, \$22.95 paper.

Martin Rhodes, European University Institute

This is a magisterial study by a leading international specialist in public policy research. The approach is simultaneously comparative, interdisciplinary, and wide-ranging. Castles provides a "comprehensive account of post-war public policy development in the OECD region based on a systematic analysis of data covering more than 30 years for 21 nations and 12 policy areas." Moreover, three different audiences are targeted: those with no statistical training who are uninterested in numerical analysis, those with no statistical training

but who want to understand the techniques employed, and those with considerable background. Castles seeks to make the analysis accessible to the first two groups while not disappointing the third. He also wants to dispel some of the myths that loom large in both politics and academia, particularly the globalization bogey of the Left and the "big state" demon of the Right. Altogether this is a tall order. How well does he succeed?

Overall, Castles does extremely well. With regard to accessibility, much of chapter 1 is devoted to explaining the approach taken in the book, and it provides a useful map for exploring subsequent chapters and a basic introduction to data analysis. Set out with admirable clarity is the author's justification of his comparative and interdisciplinary approach; the rationale for his presentation of data; and the utility of invoking four "families of nations"—English-speaking, Nordic, continental Western, and southern European. Castles then explains that the statistical analysis is only an entry point for understanding the many factors shaping policy outcomes and postwar public policy development.

Chapters 2 and 3 review the major factors identified as significant for postwar public policy development, presenting the author's interpretation of the sociological ("modernization") and "politics matters" approaches. Castles begins by noting the blindness of both approaches to history and then guides the reader through the effects of postwar economic growth, covering changes in occupational structure, urbanization, and aging. Trade and religion are introduced as potentially important variables neglected by modernization theory. Chapter 3 departs from the latter completely by stressing the significance of ideology and institutions for cross-national patterns of public policy outcomes.

These two chapters provide a thorough account of the intellectual context and introduce the author's own brand of the "politics matters" school. At the end of chapter 3 is a useful summary of his "fractured modernity" thesis. In essence, this argues that there are fracture lines between "alternative routes to and through modernity." The most radical fractures of all, Castles maintains, are the result of partisan differences. These not only show no signs of convergence, he argues (indirectly countering "end of ideology" assumptions), but also are unconnected to the main socioeconomic cleavages of the postwar era (p. 95).

Chapters 4 to 8 are the meat of the book and provide the bulk of analysis on which this judgement is based. Each follows the same format: description of the data, development of key hypotheses, presentation of findings, and the final analysis, backed up by bivariate test tables that illustrate the size of correlations for each variable over time. The causes of big government, the welfare state, the state and the labor market, public policy, and "the personal" (i.e., home ownership, fertility, and divorce) are all analyzed in this way. The conclusions clarify our understanding of postwar developments. High government outlays and receipts are linked to minimal veto points, weak right-wing parties, and population aging; high levels of public consumption spending are associated strongly with Left incumbency. The effect of international trade on both is minimal. Social security spending in the early postwar period was determined by citizen needs, cultural patterns (particularly Catholicism), and the availability of economic resources; in the more recent period, politics—both partisan and constitutional—"matters" most. Public health coverage presents a mirror image: Left incumbency mattered most early on, but resource availability had become an overriding influence by the 1990s. In education, political and social variables combine: Spending grows least in countries with Catholic cultures and Right incumbency.

As for the labor market, Catholicism has a strong influence on male and female labor force participation (as does home ownership on male withdrawal), while corporatism and strong manufacturing sectors were until recently guarantors of low unemployment rates. Contrary to right-wing assumptions, there appear to be no direct links between benefit generosity or the size of the state and unemployment, although the latter is positively linked to the rate of real earnings growth. Finally, in the analysis of domestic and family life, Castles shows that corporatism and Christian Democratic incumbency are major factors in the emergence of comprehensive state housing policies; that far from undermining the family, state benefits and high rates of female labor market participation are positively linked with fertility; and that lack of public support for families and the constraints on working women of Catholic culture conspire to defer family formation. High rates of divorce occur in non-Catholic countries, especially the English-speaking world, where services employment and female labor force participation combine with relatively recent legal reform. Contrary to popular opinion, the "nanny state" influence is negligible in these low welfare-spending countries.

The concluding chapter explores more fully the author's fractured modernity thesis. The broad picture is one of variable routes toward a threshold of affluence, prior to which resources and needs consideration heavily determine policy patterns, but after which partisan politics and ideology have freer reign. Convergence in outcomes over the whole postwar period is now culminating in a plateauing of OECD public expenditure development. According to Castles, the best explanation for this is neither the modernization nor the

globalization thesis but program maturation.

This is a compelling and convincing analysis. In its combination of scope, ambition, and rigor it is currently unchallenged. It is succinct, concise, and undeviating from its central explanatory thesis. The book also is extremely dense, especially in the central chapters. This is its one weakness, the consequence of packing in such a large amount of data and analysis, and may deter all but the most assiduous advanced students. Yet, given the penchant of many academics for ungrounded generalization, it should be compulsory reading for their professors.

Saudi Arabia and the Politics of Dissent. By Mamoun Fandy. New York: St. Martin's, 1999. 272p. \$45.00.

Gawdat Bahgat, Indiana University of Pennsylvania

This work deals with political stability in one of the most important countries in the world. The focus is on Islamic opposition to the Saudi political system. The book seeks to discuss the social roots and political ideas of the leading opposition figures in the kingdom since the early 1990s. In the first chapter Fandy presents the main concepts and the framework of his study. The following two chapters examine the ideas of two prominent Sunni Muslin clerics—Sheikh Safar al-Hawali and Sheikh Salman al-Auda. In their sermons the two preachers strongly criticized public corruption and nepotism as well as the American military presence in Saudi Arabia. In chapter 4, the analysis shifts to a more organized form of opposition—the Committee for the Defense of Legitimate Rights (CDLR) founded by Muhammed al-Mas'ari. The CDLR, headquartered in London, depends on fax machines, e-mails, web sites, and other means of modern communication to mobilize public opinion inside

and outside Saudi Arabia against the royal family. In the mid-1990s, the leadership of the CDLR split, and a new organization, the Movement for the Islamic Reform in Arabia (MIRA), emerged. The leader of MIRA is Sa'd al-Faqih, a surgeon by profession.

Usama bin Laden, the most outspoken Saudi dissident, is the focus of chapter 6. The main differences between bin Laden and the other opposition leaders discussed in this work are the final objective and the means applied to achieve it. With the exception of bin Laden, all the opposition figures and organizations mentioned by Fandy can be described as a "loyal opposition." Their goal is reform not revolution. They do not call for the overthrow of the royal family. Bin Laden, in contrast, has crossed this line and seeks to change the government in Riyadh. In order to achieve this goal he declared a holy war against the Saudi political system and the foreign power behind it (i.e., the United States). The last opposition organization discussed in the book is the Shi'a Reform Movement. Unlike all the other groups, this one is Shi'a. Its goal is to fight discrimination and prejudice alleged to be practiced by the Sunni establishment against the Shi'a minority. The leader of this movement is Sheikh Hassan al-Saffar.

In its efforts to contain the opposition the Saudi authority has adopted a stick and carrot approach. Both Sheikh Hawali and Sheikh Auda were imprisoned in 1994 and released in June 1999. Bin Laden was stripped of Saudi citizenship. Meanwhile, economic and social conditions have improved in the Eastern Province, where the majority of Shi'is reside. Another way for the government to contain the opposition has been an attempt to control the mass media both inside and outside the kingdom. Aside from government control over local radio, television, and newspapers, Saudi princes and their business allies have purchased major international media outlets. Furthermore, there is severe lack of coordination among opposition groups, who do not trust one another. Finally, oil revenues serve as safety valves for the royal family. Fandy concludes that the opposition groups do not pose a serious threat, and the political system in Riyadh is stable now and in the foreseeable future.

The major strength of this book is its reliance on primary sources, including tapes, books, and personal interviews. Collecting information on dissidents in Saudi Arabia is not easy. Still, the book has several flaws. First, to some extent the discussion tends to be more descriptive and less analytical. Each case study (i.e., opposition movement) stands on its own without a serious attempt to compare them or find similarities and differences. Second, the author does not adequately discuss previous work done on political opposition in Saudi Arabia. Several studies have dealt with this important subject. Some of them were cited, but no real efforts are made to benefit from their analysis and build on their findings. Third, Fandy claims that the Saudi opposition movement rose in reaction to external developments, particularly in Egypt, Syria, Iraq, and Iran. There is no doubt that regional and international changes have affected the socioeconomic and political environment in Riyadh, but internal problems within the system are also responsible for the rise of indigenous opposition. Fourth, the author suggests that when Crown Prince Abdullah succeeds King Fahd the Saudi political system will be more stable. It is true that physically the crown prince is in better shape than his brother and has strong ties with powerful constituenices within Saudi society, but given his age (about 75 years), it is hard to imagine that he will live long enough to have a long-term effect on the political system.

Finally, an important theme of this book is the rising role

of information technology in shaping the Saudi political environment. In 1999 the kingdom was connected to the Internet. In order to filter out material that the authorities consider undesirable, a government-appointed body became the node through which all web sites are accessed. Using technology imported from abroad, the King Abdulaziz Institute for Science and Technology in Riyadh has installed a system that prevents end-users from viewing pornographic, politically sensitive, or fanatically religious material. The ultimate arbiter on what is permissible is the Ministry of Interior. How this new opening to the global information system will affect the dynamics of Saudi policy is an open question.

Shrinking the State: The Political Underpinnings of Privatization. By Harvey Feigenbaum, Jeffrey Henig, and Chris Hamnett. Cambridge: Cambridge University Press, 1998. 182p. \$54.95 cloth, \$18.95 paper.

William Gormley, Jr., Georgetown University

To policy analysts, what is most interesting about privatization is its consequences; to political scientists, the political origins of privatization are of greater interest. Although Shrinking the State offers little new material to resolve the pressing questions posed by policy analysts (does it work? when does it work? for whom does it work?), it contributes significantly to our understanding of privatization as a political phenomenon.

The central thesis is that privatization is rooted not in economics but in politics, particularly the motives and designs of leading politicians. The authors develop this thesis by examining the evolution of privatization in three settings, the United Kingdom, France, and the United States. In each country, national politicians link the rhetoric of privatization to ideological or partisan goals. Their underlying motivation is not the quest for economic efficiency but the quest for a smaller welfare state or a larger victory in the next election.

The authors substantiate their argument by immersing themselves in the nuances of each case. For example, they note that France's candidates for privatization were already profitable and efficient and that the United Kingdom sometimes substituted a private monopoly for a public monopoly. If economic efficiency were the real motivation, the authors argue, then these countries' conservative leaders would have privatized fewer assets and moved at a more deliberate pace.

To understand both cross-national and longitudinal variations, the authors introduce a typology rooted in a political understanding of privatization. Systemic privatization is an attempt to shrink the welfare state by privatizing large assets as rapidly as possible. Tactical privatization is an attempt to gain an electoral advantage by privatizing at an opportune time. Pragmatic privatization is an attempt to solve a particular problem through a tailor-made solution, such as contracting out. The first two forms are intensely political, and the third is not.

The story line in each country is strikingly different. In Britain, pragmatic privatization led to systemic privatization, which eventually triggered a backlash. In France, pragmatic privatization yielded to tactical privatization, which later mutated into a different version of pragmatic privatization. In the United States, pragmatic privatization never quite evolved into something else. Instead, pragmatic initiatives were coopted by advocates of systemic change, who ultimately settled for more modest reforms.

Each story is well told and well documented, with insights gleaned from the scholarly literature and the popular press.

The U.S. case is more difficult to characterize than the other two, perhaps because it involves a federal system, with more of a "bottom-up" approach to policy innovation. The authors concede an apolitical cast to numerous local government decisions to privatize through contracting out, but they argue that the broader concept of privatization was politicized by the Reagan administration, with important consequences for the future of privatization debates in the United States. They attribute Reagan's privatization failures to the ill-fated evolution of his plan to privatize public lands, a proposal that tainted other efforts.

There is much to admire in this book. It is theoretically informed, and it systematically applies a useful and original analytic framework. It provides rich, textured stories of privatization in three important countries and uses these to test four hypotheses. For example, the authors demonstrate that systemic privatization is favored by conservative governments and that certain forms of privatization generate backlash. It is easy to imagine this book being used to good advantage in advanced courses in comparative politics or public policy.

Still, several questions remain. First, is privatization a trend or a fad? The authors assert, convincingly, that it is not inevitable and that each nation's domestic politics will determine the issue. But are conditions generally favorable for privatization? In John Kingdon's words (Agendas, Alternatives, and Public Policy, 1984), have the problem, politics, and policy streams converged? If so, how long will a window of opportunity remain open for policy entrepreneurs?

Second, is privatization good or bad? The authors do not squarely address this question, although their asides suggest a jaundiced view. Numerous empirical studies have assessed the consequences of privatization for economic efficiency, but few examine the consequences for service quality or benefit redistribution. With privatization advancing in new policy areas (e.g., child support enforcement), we need more and better studies of the effects to inform and guide policymakers.

Third, is pragmatic privatization the wave of the future? Although the systemic and tactical forms have generated backlash, pragmatic privatization marches on. That is perhaps because it is rooted in nonpartisan policy analysis and decisions made by state and local bureaucrats rather than federal politicians. Such rationales are likely to prove more durable than ideological or partisan perspectives.

Finally, if pragmatic privatization is most likely to triumph, then where does this leave the authors' argument concerning the primacy of politics? By their own admission, pragmatic privatization is largely apolitical. If the perspective of political science helps us understand the choices national politicians made during the late twentieth century, then the perspective of policy analysis may help us understand the choices that officials at all levels of government will make in years to come.

Pornography and Democratization: Legislating Obscenity in Post-Communist Russia. By Paul W. Goldschmidt. Boulder, CO: Westview, 1999. 274p. \$55.00.

Peter Rutland, Wesleyan University

One of the first signs that censorship was easing in the former Soviet Union was the appearance in 1987–88 of young men selling home-made pornography at the entrance to metro stations, usually photocopies of crude erotic drawings. Printed color magazines and bootleg videos soon followed, and the rest, as it were, is history. The Soviet state simply lost its ability to control society. A vivid illustration was the fact

that the censor (Glavlıt) was still complaining in 1990 that its budget was insufficient to provide it with VCRs to monitor videos, which by then were showing in video salons in every Russian high street (p. 146).

Goldschmidt starts from the premise that a state's attitude toward pornography is a key indicator of its degree of tolerance, which in turn is an essential prerequisite for liberal democracy. He argues that the open circulation of pornography in Russia (on a scale far greater than in most other countries) does not mean that the Russian state, or society in general, has reached the level of tolerance required for a civil society. On the contrary, the dissemination of pornography is often regarded with disgust, as a sign of social breakdown and moral decay.

Despite the social upheavals that accompanied the rise and collapse of communism, traditional attitudes toward gender roles did not undergo radical change, Goldschmidt argues. Women are the losers in this situation: The state dominates men, and men dominate women (p. 240). Hence, the laissez-faire pornotopia that has blossomed since 1991 has led to a potentially explosive combination of traditional values of female subordination and postmodernist pornographic experimentation (p. 237). Russian liberals are divided over the issue, with their distaste for censorship overriding their differing opinions on the merits of pornography. Given this situation, Goldschmidt fears that conservatives, who oppose pornography on traditional moral grounds, are likely to win the argument and shape the future course of state policy.

One of Goldschmidt's central themes is that Russian intellectuals are partly to blame for this state of affairs. Rather than line up on the side of tolerance and pluralism, they tend to agree with the state that people need to be protected from their own worst instincts. Even Westernoriented thinkers, the bearers of liberal values, share the Russian intelligentsia's traditional suspicion of the people. Hence, one sees the extraordinarily prominent role allocated to "expert" commissions by Russian courts in deciding whether a publication is obscene. These experts rely on whatever discipline takes their fancy—from cinematography to psychology—as the basis for their judgment (p. 186). One of Goldschmidt's key informants is Vladimir Borev, an art historian who wrote the expert guidelines later adopted by the Procurator's Office (p. 175). We are told that Borev made his archive at the Video-ASS magazine available to the author (p. vii). (ASS is a Russian acronym, not derived from its Western synonym.) Borev has since given up reviewing pornography and is working as an "image maker" for former general Aleksander Lebed (p. 182).

A second central theme of the book is that of historical continuity. In fact, despite the title, only about one-third of the text is devoted to Russia since 1991, and at least one-third is given over to the tsarist era. Goldschmidt argues that the Soviet state, like its tsarist predecessor, was primarily interested in control over the dissemination of potentially damaging material and was not at all interested in trying to define exactly what qualifies as pornography. The wording of the legislation dealing with obscenity was lifted almost verbatim from an international convention devoted to the subject in Paris in 1910—and the wording was only introduced into Soviet law in 1935, as a condition of the Soviet Union's entry into the USSR League of Nations (p. 169). The clause in question (Article 242 of the current Criminal Code) criminalized the distribution but not the possession of pornographic material.

The book, like its subject, promises somewhat more than it can deliver. It contains some fascinating insights, but the structure of argument is rather disjointed, weaving back and

forth between present and past. It does not provide a systematic account of the spread of pornography since 1991 (this story is mostly compressed into pp. 64-72). Important aspects, such as the role of the mafia, are only mentioned in passing (p. 195). Goldschmidt relies on a handful of informants (albeit influential ones) to represent intellectual opinion. He devotes relatively little space to analyzing the legislative process since 1991 (pp. 146-54), and he glides quickly over the broader issue of press freedom in Yeltsın's Russia. The adoption of new pornography legislation was still in process (in 1998) when the book went to press (p. 153). The author takes as a case study the 1996 civil suit against Playboy for printing obscene drawings of Catherine the Great and other luminaries (chap. 7). The suit failed, illustrating (to me) that it is premature to adopt a law-based approach to the phenomenon of pornography in Russia.

The book is primarily structured around philosophical and legal concepts, in particular the dialectic between conservative and critical thinkers on pornography (chap. 1). This distinction comes from the West and does not really fit what is happening in Russia, a point that Goldschmidt himself is at pains to underline. He is sympathetic to the anticensorship feminist wing of the critical school, and he advocates sex education as a way to deal with the problem (chap. 8), although not with any great conviction that this will work. Perhaps a conceptual framework more rooted in Russian experience—based on an analysis of gender roles, the nature (and absence) of privacy, or the curious way in which the public/private dichotomy plays out in Russian society—could have been more fruitful.

All in the Family: Absolutism, Revolution, and Democracy in the Middle Eastern Monarchies. By Michael Herb. Albany: State University of New York Press, 1999. 352p. \$49.50.

Pete W. Moore, Concordia University

An observer of the Gulf states once quipped that studying the region's monarchical politics is akin to watching television without sound. Michael Herb pursues this difficult issue by launching a multicase, comparative project with a clear theoretical target. His aim is to place the political institution of the monarchy as the key variable that explains the resilience or overthrow of monarchical regimes, not only among the Gulf states but also across the Middle East. Herb's foil is rentier theory, a fiscal sociology of the state that argues the character of a state's revenue determines a country's basic politics. This is an impressive, thought-provoking book of comparative scholarship that advances debate about the rentier state to fruitful grounds. Yet, those who believe rentier theory explains more than Herb is willing to grant (this reviewer being one) still have grounds to challenge some of the book's claims.

Herb argues that the Arab monarchies of the Gulf have succeeded in fashioning enduring political rule through intrafamily cohesion, which he terms "dynastic monarchy." The primary cases that serve as examples for the other Gulf monarchies are Kuwait and Saudi Arabia. In each case, ruling families established their predominance in the period immediately before and after decolonization. Keys to the endurance of this domination were the ruling family's control of the most important government ministries and intrafamily resolution of succession issues. The distribution of government posts among family members ensured that political rivals could not gain a bureaucratic foothold, and the internal resolution of disputes limited the exploitation of family

divisions. To varying degrees, the monarchies in Bahrain, Oman, Qatar, and the United Arab Emirates have emulated this design.

Variation in the dependent variable comes with a discussion of the failed monarchies in Libya and Afghanistan. According to Herb, the failure to establish dynastic control led to their overthrow. The key comparative leverage for the Libyan case is that the 1969 overthrow occurred despite the presence of high oil rents. Demonstrating impressive breadth for a first book, Herb goes on to discuss the fate of other nondynastic regimes in the Middle East (Egypt, Iran, Iraq, Jordan, and Morocco). Herb's handling of these deviant cases settles on two claims. First, for the overthrown monarchies in the first three cases, the lack of a dynastic character made monarchical rule more precarious, and thus challenges eventually overcame the poor decisions of rulers. Second, Herb attempts to explain the resilience of the nondynastic Jordanian and Moroccan monarchies by highlighting contingent and idiosyncratic factors that have meant regime survival but not endurance. These are hard cases to square with the book's thesis, but by not shying away from the contrary evidence, Herb enriches the theoretical debate.

All in the Family joins the work of scholars (Eva Bellin, Steven Heydemann, Miriam Lowi, and Gwynn Okruhlik) who challenge the assumptions and predictions of rentier theory. An important component under frequent attack is the distinction made between externally and internally derived revenue and the political implications that are expected. In the latter, political representation follows taxation. In the former, the state is aloof and unconstrained by representation demands. Herb's counter is that the arrival or decline of rents only signals change, but to understand variation in political outcomes, one needs to consider domestic political institutions. This is clearly an agency claim against the structural determinism of rentier theory. Monarchies survive, liberalize, or fall because of the decisions of monarchs, not because of the abundance or lack of exogenous rent. To this point, the argument does not stray far from recent revisions to the basic theory that explore the importance of a number of nonrent variables to postrentier outcomes. Yet, Herb flies a bit close to the sun in asserting that oil by itself explains nothing and is best understood as "an intervening variable" (p. 241)

In the linchpin Kuwaiti case, Herb argues the dynastic pattern was set in the wake of the proparliament Majlis movement in 1938. It was not until the 1950s, however, that a "dynastic proto-cabinet" (p. 76) was formed, with al-Sabah family members dominating state agencies. Timing is crucial, for by the 1950s Kuwait was flooded with oil monies, and it is precisely the presence of those external funds that allowed dynastic formation. Jill Crystal's influential treatment (Oil and Politics in the Gulf, 1995) of this period notes that oil rents allowed the emir first to establish regular revenue distribution among family members and then to coopt merchant elites. The 1961 dispute between merchants and the emir over Kuwait's first cabinet (a crucial event only briefly mentioned in the book) clearly demonstrated merchant abandonment of cabinet positions in exchange for distribution. The particular form of dispute resolution and bandwagoning Herb documents can be understood once one recognizes that the prospect of ever-increasing rent makes royals risk avoident; therefore, sharing the pie ensures intrafamily compromise. In the contrasting case of Libya, timing was also important. No doubt, from 1963 to 1969 the Sanusi monarchy made bad political decisions, but Dirk Vandewalle (Libya since Independence: Oil and State-Building, 1998) argues, in part, that these bad decisions were amplified by institutional destruction wrought by a massive influx of capital. Similarly, Jordan was born a rentier state (albeit in the form of foreign aid) but has a surviving nondynastic monarchy. Apart from idiosyncratic reasons for survival, the presence of those rents allowed the Hashemite state to employ half the country's workforce and coopt much of the business elite. These incentives played an important role in the Hashemite victory in the 1970 civil war. In all this, however, Herb is right that there is plenty of room for agency mistakes, but nevertheless the structural constraints of a rentier state are quite evident.

All in the Family imparts very useful lessons that further the debate about the legacies of rent and postrentier politics. This debate is not abstract theorizing. The Gulf states are currently crawling out of their own oil bust, and over the next decade, more rentier states will be created as some of the Caspian States receive their own oil windfalls. Oil revenue and its effects will be with us for some time.

The Individual and the State in China. Edited by Brian Hook. Oxford and New York: Clarendon, Oxford University Press, 1996. 231p. \$23.00 paper.

Mass Politics in the People's Republic: State and Society in Contemporary China. By Alan P. L. Liu. Boulder, CO: Westview, 1996. 251p. \$65.00 cloth, \$19.95 paper.

Stanley Rosen, University of Southern California

Although both of these volumes appeared in 1996, the influence of the events of the late 1980s, which culminated in the military crackdown of June 4, 1989, continues to weigh heavily on the authors. This is not surprising in the case of the book edited by Brian Hook, since most of the chapters originally appeared as a special issue of China Quarterly, published in September 1991. There appears to have been relatively little updating, although two of the authors provide brief postscripts that essentially reaffirm their earlier conclusions. What is new for the book is the Introduction (by Brian Hook) and, of particular value, a typically thoughtful concluding chapter on the dynamics of civil society in post-Mao China by the late Gordon White. Since both books were published before the death of paramount leader Deng Xiaoping in February 1997, a sense of uncertainty with regard to China's future leadership also permeates the two volumes.

There is a good deal of similarity between these books, at least in terms of the questions addressed. The Hook volume brings together leading specialists to analyze the relationship between various social groups and the state: Andrew Walder on workers and managers, Robert Ash on peasants, David Shambaugh on soldiers, Michel Bonnin and Yves Chevrier on intellectuals, and Thomas Gold on youth. Lucian Pye provides the overview interpretation. Alan Liu's book also examines the relationship between social groups and the state, and it contains separate chapters on peasants, workers, students, and ethnic minorities. These groups constitute Chinese "public opinion," and Liu attempts to demonstrate that public opinion, as the concept is generally understood in the West, has existed in the People's Republic since its inception in 1949.

The books are most closely linked, however, by their concluding chapters, both of which assess the prospects for the development of civil society in China. This became a common theme in works by Western academics after the unexpected events of 1989. Writing several years on, both Alan Liu and Gordon White recognize the importance of the Chinese leadership in any political transition. Influenced by the consequences of 1989, Liu suggests that China is far more likely to follow the economy-centered, nonconfrontational,

elite-centered Hungarian model, also followed in Taiwan, than the bottom-up Polish model. White, noting the fears provoked by and the conflicts inherent in China's incipient civil society, sees a growing consensus in China for a gradual, managed process of political liberalization and democratization. If such a transition can be achieved at all, it will have to be sponsored and organized by reformist elements within the current political elite.

Each volume is a serious contribution to an understanding of state-society relations in China and offers rewards to the careful reader. Liu not only is cognizant of the primary and secondary source literature on China but also is well versed in the theoretical literature on public opinion and civil society. Scholars engaged in comparative analysis that incorporates nondemocratic systems should find persuasive his reasons for rejecting the more restrictive definitions of public opinion provided by Hans Speier and Jürgen Habermas in favor of the broader usage employed by such writers as Bernard Berelson and Ithiel de Sola Pool.

Although of necessity oversimplified, Liu's key argument can be summed up as follows. There has been uninstitutionalized public opinion in the PRC since the 1950s. The total effect of the Communist Party's political revolution since 1949 has been a sharpening of the state-society division. The Chinese masses still harbored a degree of positive, wishful thinking toward the party until the anti-rightist movement of 1958. From then on they adopted various modes of amorphous social actions to oppose the party-state. As a result of dealings with the party-state, a mass political culture emerged among Chinese peasants, workers, students, and ethnic minorities. "Occasionalism" and "sectionalism" mark this political culture, so that every group or person will try to maximize its (or his/her) short-term interest when a specific occasion allows it, and every group will deal with the state independently of other groups (p. 224).

This approach yields some useful insights, such as Liu's analysis of how the social isolation of students and intellectuals has enabled the state to dispose of them as it saw fit. Liu is also well aware of the ironies of government policy toward the various groups, and he offers an interesting contrast between the treatment and response of workers and peasants. The peasants, despite official rhetoric, were treated by the state as conservative and reactionary, and therefore they were excluded from the "construction of socialism." In response to their "intolerable" treatment, they became the leading force for change and modernization in the post-Mao period. The workers, as the "true proletariat," were expected to spearhead the socialist transformation of China, but their "privileged" status under Mao turned them into a conservative force that resisted the Dengist state's modernizing programs (p. 130).

Liu's volume is marked by a broad historical sweep, covering the PRC since its founding in 1949. This allows him to make thoughtful and provocative comparisons of Maoist and Dengist China in each chapter. In the end, however, he does not see any "fundamental difference" in the state's conception of the individual between the Mao and Deng regimes (p. 230). Hook's collection, written by individual specialists, is more nuanced in this regard. Liu offers a coherent theoretical framework to link his chapters rather than the set of essentially discrete chapters one finds in Hook, but the detailed analyses provided in the latter volume reveal important, if perhaps not fundamental, changes as one moves from Maoist to Dengist China.

As with Liu, none of Hook's specialists appear to be overly optimistic about the success of the Chinese leadership's reform program in solving various problems. Lucian Pye sees

a post-Tiananmen state seeking to mask a paralysis created by factional differences. The result is a movement away from its Leninist traditions and a reversion to a traditional and highly ritualized Confucian state that seeks to uphold stability at all costs. Walder focuses most directly on the political crisis of 1989 and concludes that the participation of workers in these events signaled the unraveling of the Dengist strategy for reform (p. 68). He expects the political relevance of urban labor to grow. Ash offers a narrow focus, examining the changing nature and direction of key financial and real resource flows between the peasant and the state from 1979 to 1988. He concludes that tensions between the peasant and the state, exacerbated by the agricultural stagnation of the second half of the 1980s, will not be easily resolved. Shambaugh, who looks at 1989 in the context of earlier and later events, offers as his central argument that the People's Liberation Army and Communist Party elite are interlocking, symbiotic entities. Taking issue with those who argue that the army is a professional military that wishes to disengage from the political arena, Shambaugh expects that the military, if called upon once again to defend the party-state, will again obev.

Bonnin and Chevrier, who provide a detailed account of nascent civil society development, are the most optimistic of the authors. They examine the rise of limited "intellectual autonomy" in the 1980s and conclude that the Tiananmen crisis was a significant step in the emergence of civil society from the bleak landscape of totalitarianism, albeit just a step (p. 173). Gold, who uses the life course approach of sociology, examines the three birth cohorts that have passed through the youth stage since the founding of the PRC and reveals how each has had radically different relations with the party-state. He explores the very different views of these cohorts regarding the nature of China, socialism, the world, and themselves. This wide gap between generations has made it extremely difficult to integrate Chinese society and virtually impossible for the state to reestablish the type of control it exercised in the 1950s.

How well do these volumes hold up? Both are valuable, particularly for the light they shed on the first decade of reform. The chapters by Pye, Shambaugh, Gold, and White are less time bound and offer theoretical insights that can still be used to analyze current and future developments.

Militant Nationalism: Between Movement and Party in Ireland and the Basque Country. By Cynthia L. Irvin. Minneapolis: University of Minnesota Press, 1999. 281p. \$49.95 cloth, \$19.95 paper.

Andrea Grove, University of Vermont

In this ninth volume in the Social Movements, Protest, and Contention Series from the University of Minnesota Press, Cynthia Irvin takes on a question that has often puzzled researchers of both social movements and nationalist movements: How can we understand why groups traditionally mobilize to take violent action then change strategies and begin to engage in institutionalized politics? Because militant nationalism is built around the idea that the state and its political system are illegitimate, it is indeed a puzzle when paramilitary organizations such as the IRA in Ireland and ETA in Spain develop an electoral strategy that involves participation in the regime's parliamentary politics (which may or may not involve renouncing violence; Irvin refers to the latter as a "mixed" strategy). In addition to offering a plausible answer to this puzzle, Irvin begins to deal with a question that is crucially important today: How do external

actors affect the internal politics of nationalist groups? This review summarizes how the author addresses both issues and draws attention to several policy implications of her study.

The majority of studies on paramilitary groups focus on why they resort to violence, but Irvin recognizes in this comparison of the Irish and Basque cases that understanding the move from violence to "playing by the rules of the game" is indispensable. Such knowledge offers insight into the best way to get extremist groups to the negotiating table. Because her data are gathered from interviews (for both cases) and surveys (for the Basque case only), Irvin makes a significant empirical contributions to the study of revolutionary movements. For obvious reasons, it is rare for anyone to observe this particular kind of organization, but the perceptions of these actors are crucial to understanding their behavior in changing contexts.

As a launching point, Irvin conceptualizes revolutionary movements not in the traditional way, "as coherent systems of relations oriented to the pursuit of specific goals," but "as an opportunistic collection of divergent interest groups, temporarily banded together to pursue goals that are developed through internal political discourse" (p. 13). She argues that the strategies chosen by the organization are the result of the bargaining among three "interest groups": ideologues, radicals, and politicos. Ideologues predominantly advocate armed struggle, politicos argue for electoral participation, and radicals wish to pursue a mixed strategy. Irvin then explores the degree to which three factors drawn from social movements theory-regime responsiveness, competitive environment (absence or presence of competing political groups willing to cooperate with the government), and organizational resources (both domestic and international) affect the internal balance of power among these three

Specifically, Irvin hypothesizes that the three factors interact to (1) affect the size and influence of ideologues, radicals, and politicos and (2) set the context within which they choose among the following strategies: terrorism, armed struggle, mass nonviolent direct action, and/or institutional politics (p. 25). The reader is initially puzzled about the dependent variable. Is it the choice of strategy, as the introduction and literature review indicate, or is it the relative size of the three groups of activists? It would have been helpful to include a diagram of the many variables, which could show that group size and influence are intervening variables, and strategy choice is the phenomenon to be explained.

In chapter 2 Irvin elaborates on the three groups and their preferred types of activism, then derives three hypotheses about the effects of the independent variables. A fourth hypothesis, which seems unnecessary, states that these movements will choose strategies that maximize support for the military wing as well as the organization's goals. A minor problem is that these are not stated in hypothesis form-if, then statements—but instead are declarative statements. In discussing the hypothesized relationships, Irvin draws on theoretical work to make logical assumptions about the effects of structural variables (regime responsiveness, organizational resources, and competition from other parties) on the perceptions of activists. For example, hypothesis 1 states: "In semirepressive regimes characterized by intermediate levels of negative sanctions, nationalist movements will primarily attract militant activists, both ideologues and radicals, who will opt for a strategy of violent confrontation with the regime" (p. 38). The logic is that activists see repression as closing off any chance for their fair treatment in the institutions of the state and thus perceive violence as the best option. Because Irvin has data that allow her to assess the perceptions of activists, she is better able to study the relationship between repression and militant strategies than most other researchers who have tried. Indeed, she does a wonderful job of drawing on excerpts from the interviews and internal party documents to show the activists' perceptions about regime responsiveness and the other variables. Previous work has tended to rely on assumptions about how structure affects the perceptions of the actors.

Chapters 3 and 4 provide historical background for the decisions by the IRA and ETA to support Sinn Fein's and Herri Batasnuna's decisions to engage in electoral politics. Chapter 5 presents empirical information showing the "splits in the ranks," or the distribution among the three groups of activists in each case. Finally, chapter 6 contains the comparative analysis that links the three independent variables to the changes in composition and strategy of Sinn Fein and Herri Batasuna. Again, Irvin makes good use of the interviews she conducted in Ireland and the Basque country. Also, an engaging and informative narrative shows how shifts in regime repression, the competitive environment (such as the influence of more inclusive, nonviolent nationalist parties), and organizational support affected these movements. The research confirms her hypotheses.

As noted above, this study is significant because the unique data enable the author to get at the mechanisms operating between the behavior of actors external to the organization (such as more moderate parties, the state, and international actors) and the perceptions of activists who are trying to choose strategies in response to this environment. Several of the findings have implications for policy. For example, by showing how the responsiveness of the regime can affect whether more exclusive (ideologues) or more inclusive (politicos) activists enter the organization and have their preferred strategy "win," Irvin offers governments insight about the behavior that would provide the best chances for politicos to dominate. Another example stems from the fact that external actors can affect the parties' perceptions of the resources they bring to bear in the struggle. A comparison of an organization's resource "profile" before it is willing to enter electoral politics with the profile afterward suggests how external actors can try to manipulate the resources available to the organization in order to give the upper hand to the politicos.

On a theoretical point, Irvin's profiles of activists she shows socioeconomic data and pathways to activism of ideologues, radicals, and politicos—speak to recent research on identity and mobilization. Most work looks at "public opinion" support for different leaders as a whole. This study reveals that militants differ from other constituent groups in their society, and certain characteristics are associated with people who are more likely to take an exclusive view of the group's identity and mission. Therefore, scholars who agree that it is important to consider the conditions under which an extremist approach is likely to gather a public following need to pay careful attention to the profile of that public. Furthermore, because the independent variables in Irvin's model can be manipulated by actors outside the organization, lessons are offered on how to make that particularly "vulnerable" constituency less open to extremist appeals. For example, governments should think twice about strict security policies that target poor and working class areas because such repression tends to mobilize ideologues into revolutionary organizations and weaken the more moderate voice of politicos.

This book should be of interest to a broad cross-section of political scientists and social movement theorists, including those who are interested in the different kinds of mobilization

(toward more inclusive and more exclusive ends), conflict resolution, and organizational behavior. Indeed, this comparative analysis offers insight that is backed up by unique empirical work.

Contested Economic Institutions: The Politics of Macroeconomics and Wage Bargaining in Advanced Democracies. By Torben Iversen. Cambridge: Cambridge University Press, 1999. 221p. \$54.95 cloth, \$19.95 paper.

Herman M. Schwartz, University of Virginia

Torben Iversen generalizes, elaborates, and quantifies propositions about the sustainability of social democratic political economics that Fritz Scharpf first aired in Crisis and Choice in European Social Democracy (1991). Iversen's book challenges conventional new classical and neocorporatist literatures that look one-sidedly at the macroeconomic consequences of combining accommodating or nonaccommodating monetary policy with relative degrees of centralization in collective bargaining institutions, respectively, by observing what happens from different combinations of these institutional variables. Contrary to new classical models, which hold that monetary policy has no real long-term effects on unemployment and income levels, Iversen argues that monetary policy does have real effects. Contrary to neocorporatist arguments, particularly the widely accepted model of Lars Calmfors and John Driffell ("Centralization of Wage Bargaining," Economic Policy 6 [April 1988]: 14-61), which hold that only either highly centralized or decentralized collective bargaining systems produce superior macroeconomic outcomes, Iversen argues that in the current global economic environment a combination of nonaccommodating monetary policy and intermediate levels of centralization can produce superior outcomes. Iversen thus contests the notion that the only policy choice available today is between the disorganized and inegalitarian capitalism of the liberal market economies and the deficit-producing and egalitarian traditional social-democratic model.

Iversen tests these propositions by correlating institutional structures (understood as the combination of accommodating or nonaccommodating monetary regimes with high, low, and intermediately centralized collective bargaining systems) with macroeconomic outcomes across most of the rich OECD economies. He finds that, as the Calmfors-Driffell model suggests, the combination of accommodation and high centralization does produce low unemployment and wage inequality, but the combination of nonaccommodation and intermediate centralization produces all this and lower inflation, too. Iversen then models the underlying strategic behaviors by governments and unions that produce these two best-case outcomes. Qualitatively inclined readers need not fear Iversen's formal modeling and econometric exercises. He presents his findings clearly in each chapter and segregates the nuts and bolts of the models and formulas into appendices at the end of each chapter.

Iversen's findings matter for two reasons. First, the external environment that permitted the accommodation/high centralization combination to work no longer exists. Technological change has made wage compression difficult for unions and undesirable for the employers of skilled labor, but centralization almost always produces compression. The financial penalties for accommodation are also much higher in a world of relatively more mobile capital. Second, because actors are free to redesign imperfect institutions at the margins, and substantially freer to alter their strategic behavior within a given institutional structure, Iversen predicts that

actors will shift from the accommodation/centralization structure toward nonaccommodation/intermediate centralization. In practical terms, this implies a shift toward a policy regime such as Germany's. Iversen's case studies of Sweden and Denmark show this evolution. Case studies of Austria and Norway show how Austria's lack of wage compression permits the unusual combination of nonaccommodation and centralization and how Norway's oil boom created pressures for centralization that countervailed continued tendencies toward decentralization. Iversen thus believes that social democracy is sustainable, but only with a nonaccommodating/intermediately centralized institutional format. Practically speaking, this presents social democrats with the choice between employment and wage equality that Scharpf (1991) identified.

Iversen builds on Scharpf's game-theoretic analysis of similar dilemmas in post-oil shock Europe, but he makes a clear advance in two respects. First, the imperatives posed and incentives offered by the global economic environment after ten years of financial liberalization and the diffusion of information technologies to manufacturing are much clearer. Iversen thus can make a more reasoned exploration and model of the microfoundations for worker and thus union behavior with regard to wage compression, and how that in turn relates to employer strategies. Second, Iversen confirms statistically Scharpf's discursive findings.

In other respects Iversen's analysis is a move sideways. Other intermediately centralized economies neighboring Iversen's northern Europe cases also experienced decentralization from their intermediately centralized position during the 1980s and 1990s. It would have been interesting to explore the degree to which these economies were able to continue to balance among employment, inflation, and equality and whether the decentralization of collective bargaining was as much a strategic choice there as it was in the Scandinavian social democracies that shifted away from the accommodation/centralization configuration. As well, the pervasiveness of decentralization of collective bargaining suggests that the environment facing northern Europe is itself still in flux. Insofar as the relative optimality of a given institutional configuration is a function of its fit with its environment, continued decentralization elsewhere changes the environment facing countries that recently generated a nonaccommodating, intermediately centralized structure. The optimality of that structure may be transient. Iversen himself recognizes that the reorientation of bargaining around German wage gains in a more unified Europe may change the logic she presents, particularly as unions are not centralized at a European level and continue to decentralize outside Germany.

That aside, this is a good, clear book that accomplishes all it sets out to do. Dare I say that, like most Danish products, it is spare, intelligently designed, and compelling?

The Collective and the Individual in Russia: A Study of Practices. By Oleg Kharkhordin. Berkeley: University of California Press, 1999. 418p. \$50.00.

Michael Urban, University of California, Santa Cruz

This is one of the most important books ever written about Russian politics and society. Applying a Foucauldian frame, it aims to provide an archaeology and a genealogy of Soviet civilization, focusing in particular on the production of its corresponding modal individual. The argument is layered by learned forays into many literatures. Kharkhordin juxtaposes controversies in the early Christian Church with their twen-

tieth-century counterparts in the Communist Party, compares Russian medieval monastic practices with manuals on social technology published throughout the Soviet period, and delves into the associations between blue jeans and belles lettres. The book's immediate effect is to engender radical revision of our conventional understandings of the Soviet Union. Any number of on-going controversies—the progression from Lenin to Stalin, the Great Terror, the post-Stalin "liberalization," perestroika and the disintegration of the Soviet state—are illuminated in novel and provocative ways. The deeper contribution flows from the book's challenging implications concerning how we construct politics, especially comparative politics, as an object of investigation.

In the foreground of this study is what the author calls "background practices," a concept derived from linguistics and discourse analysis that has been refltted for the dissection of social phenomenon at the micro level. A correlate of Pierre Bourdieu's "habitus" (Outline of a Theory of Practices, 1977), background practices are those taken-for-granted aspects of human activity that are indispensable to the meaning assumed by that activity itself. They represent its "conditions of possibility" (p. 11), constituting actions in particular ways in correspondence with culturally shared understandings about them. For instance, I "naturally" write this book review in a particular style—and can even play with the conventions of that style-because of a host of background practices that inform the academic enterprise. Were I reviewing this book for, say, the secret police or the Holy Synod, a different constellation of background practices would constitute this as a very different endeavor. The author's aim is to isolate the (changing) background practices specific to Soviet civilization, trace their genealogy from prerevolutionary through Soviet times, and thus lay bare the content of those practices that represent Soviet civilization and the individuals who inhabited it.

In historical perspective, the narrative begins at Christendom's fork into Eastern and Western churches. Whereas Western Christianity would come to privilege the practice of private confession over public penance, brethren in the East would reverse that balance. Consequently, Orthodox Russian theological and catechistic discourses valorized the manifestation of saintliness via objectifying practices available to the public gaze much more so than was true in the West, where, along the lines of Michel Foucault's History of Sexuality (1978), an entire technology of the self eventually grew out of the confessional focus on individual interiors. In this respect, Kharkhordin demonstrates that the Bolshevik transformation, which was primarily a reconfiguration of the ensemble of extant social practices, owed incomparably more to background practices inherited from the Russian Church than it did to Marxism.

The key elements in the argument are these. (1) The modular form of Soviet social organization, the collective, represented a constellation of background practices mainly derived from Orthodox teachings and monastic life. (2) The authorities conceived of collectives as units invested with subjectivity, as reasoning bodies with their own goals, consciences, and wills. (3) In order to realize that subjectivity, their members were required to practice mutual surveillance, which combined in shifting measures the operations of revealing, admonishing, and excommunicating individuals. When the former two elements in this trinity (revealing and admonishing) were emphasized, a relatively benign totalitarian logic prevailed in public life. During the 1930s, however, when the ruling party altered the prescription by dropping the element of admonition and assigned to collectives the

task of revealing and excommunicating their members, the ensuing dynamics produced the bloodbath.

The changing constellation of these three seminal collective practices constitutes the stages of Soviet development. Concomitantly, out of that development was born the Soviet individual. Subtextually, the reader senses that individuation—both in the objective sense of regarding people as individuals responsible for their actions and the subjective sense of individuals capable of self-perfection—was little advanced in Russia until the Bolsheviks began processing the peasant masses with the machinery of collectives. It was there that public selves would be revealed. In opposition to that onerous process, private selves would cohere around that which individuals were able to deny to the collective—their inner lives, which increasingly supplied refuge from relentless mutual surveillance. This is perhaps the most original and provocative aspect of the book: the genesis of the Soviet individual. Central to the author's account is a reversal of Ken Jowitt's (New World Disorder, 1992) concept of dissimulation in communist societies, understood as the learned capacity of each individual to put on a public show of loyalty and enthusiasm about things that she regards with indifference, if not downright hostility. Whereas Jowitt characterizes dissimulation as a technique for protecting some already existing private self against the hazards of collective life, Kharkhordin sees its Russian variant as the very source of that private self. It is through dissimulation that individuals revealed to themselves that which they were not; having made that distinction, the next step would be to discover who they were.

That discovery follows an ironic path from the seamless oppression and falsity of public existence to the liberating and authentic experience of the self in close circles of friendship. Thus, the practices ubiquitously installed by the state in the official sphere-collectives engaged in mutual surveillance, revelation, admonition, and so forth-increasingly represented hollow institutions governed by the practice of dissimulation; simultaneously, those same practices were reproduced spontaneously in private life, which engendered a particular Soviet personality who submitted to them and who constructed selfhood on the basis of the intense interaction that they involved. Without rehearsing the various venues in which this process—always negatively determined by the state and its all-embracing collectives—occurred, the main point is perhaps already clear: Individuation in Russia has resulted from practices quite different from those experienced and performed by the Western individual. Although this distinction amounts to a matter of degree, it nonetheless would sustain the conclusion that individuals produced by means of different background practices cannot be meaningfully grouped under universal categories.

For comparative politics, the implications of that conclusion are profound indeed. Ignorance of the relevant background practices invites mischaracterization on the part of the observer and thus guarantees mistaken analysis. Consider, for example, the category "democracy," whose presence or absence has always loomed large in Western studies of Russian public life. Unawareness of the practices of Soviet collectives in the 1930s could easily have led the uninitiated student to regard as democratizing the party's 1937 decision to conduct its selection of officers on the basis of competitive elections with secret ballot. In fact, that decision served as a stimulus to redouble the savagery in public life.

Similarly, the democratization thought to flow from the reforms of perestroika takes on an entirely different character in light of Kharkhordin's discussion of the hollowing out of official collectives in mature Soviet society and the corresponding migration of their background practices to the private sphere. The space opened by the relaxation of repression meant that an alternative to social relations based on collectives was not imaginable. Members therefore withdrew from them even more, and activists who derived status from their police functions in collectives deserted, too, identifying a far better arrangement for meeting their aspirations in the formation of gangs that could exact sought-for things much more efficiently through the direct application of violence. From this vantage, perestroika did not so much mediate a transition from a communist state to a democratic one; rather, it marked the replacement of a society based on collectives to one governed by gangs.

Post-Communist Party Systems: Competition, Representation, and Inter-Party Cooperation. By Herbert Kitschelt, Zdenka Manfeldova, Radoslaw Markowski, and Gábor Tóka. Cambridge: Cambridge University Press, 1999. 457p. \$69.95 cloth, 24.95 paper.

John Nagle, Syracuse University

This volume represents an important progression in empirically grounded and theoretically sophisticated work on the party politics of the new postcommunist democracies in East and Central Europe. Based on interviews from early 1994 with 481 party leaders in Bulgaria, the Czech Republic, Hungary, and Poland as well as citizen surveys in each country, the authors examine and compare the systems of party competition and cooperation and party representation of voter preferences.

The authors offer a theoretically refined and consistent perspective of party system development based on the continuing interplay of legacies from the particular type of communist system (with major influences on the birth of parties and party competition) and the learning process for parties within the new institutional framework of democratic multiparty electoral competition. Each section contributes to fleshing out the divergent qualities of democratic politics, using both deductive reasoning and hypothesis building as well as hypothesis testing and inductive modification from the empirical data. This study extends Arend Lijphart's research project into new territory, an important sign of the extent to which East and Central Europe have joined the Western comparative research agenda.

The authors take seriously the social structures and developmental politics of the communist era, building a typology of bureaucratic authoritarian (Czechoslovakia), national-accommodative (Poland and Hungary), and patrimonial (Bulgaria) communist regime types. Diversity in communist politics creates initial path diversity in postcommunist politics. This important starting point of significant communist-era diversity, so at odds with theories of totalitarianism but increasingly recognized through the post-Stalin years, led such observers as Joseph Rothschild to focus especially on the "return to diversity" feature in East and Central Europe by the mid-1980s. Yet, with the surprisingly swift and largely peaceful collapse of European and Soviet communism in 1989–91, this hard-won recognition was swept away by notions of a tabula rasa or historical vacuum that Western norms and practices, economic and political, could quickly fill.

This book rejects the tabula rasa thesis and begins the overdue task of systematically relating the quality of multiparty democratic practices to the continuing effects of specific communist structures and practices. Yet, the authors do not give in to any blanket notion of historical legacy; rather, they

specify those aspects of communist society and its demise that have particular influence on the formation of the new democratic politics. In this context, political actors engage in a dynamic learning process, using resources, legacies, and new institutional rules to develop different patterns of democratic competition (p. 45). One concern, however, is that the three categories of this typology (bureaucratic-authoritarian, national-accommodative, and patrimonial) seem tailored so closely to the four countries that one inevitably reads in the country names and country specifics. How many other "types" of communism are there? Are other countries mixed cases? A typology too differentiated loses its comparative power and theoretical interest. Each regime then requires its own categorical value.

This is not a book for those unfamiliar with the politics of the region. The authors have done well to provide very brief sketches of the many parties that had by 1993-94 made their appearance in the new party systems. But their attention is on measuring variables such as issue salience, programmatic diffuseness, and polarization of issues across the party spectrum; little space is devoted to the "flesh and blood" of political life, the colorfulness of individual leaders, or the evolving discourse among elites. The uninitiated may well get lost in the welter of party names and the hypothesized relations among parties (both cooperative and competitive), especially since there has been very considerable turnover in parties and significant migration of party programs since the data for this study were collected. There is no practical way in one volume to provide rich narratives of the new democratic politics for all parties in all four countries as the setup for the rigorous quantification, hypothesis formation, and empirical testing that is the real core of the project, and the authors had to make a hard choice. But this does present a problem for the reader who is not already well versed in the subject.

The project's quantitative approach, although ground-breaking and intelligently executed, does have its limitations. For example, is the Bulgarian Union of Democratic Forces a single party or not? Probably not, although it is treated as such in the quantitative analysis. Similarly, the fate of the Solidarity Election Action Party (p. 211) deserves more discussion, since it has become a major question mark in the Polish party system. In Hungary, the Independent Smallholder Party (FKGP) is classified (p. 353) only as an agrarian party and the Hungarian Justice and Life Party (MIEP) as the nationalist party, but FKGP leaders (and not just Josef Torgyan) are markedly antiforeigner, anti-Semitic, antiliberal nationalists not so far from MIEP leader Istvan Csurka. The quantitative imperative to place each party in just one slot is constraining and loses some valuable information.

As the authors repeatedly acknowledge, their data represent a single point in time, and replication is needed to develop the longitudinal trajectories of each party system. They are meticulous not to overreach, perhaps in part because of the significant changes in individual parties (Fidesz in Hungary, the Christian Nationalists in Poland) and party system competition since 1994. Apart from the legacy of communism, there is now also a growing legacy of the transition itself, of marketization and privatization, of IMF and EU guidance, of renewed aggregate growth and new inequalities; what political reasoning for party elites and voters emerges from this now more extended transition? Was 1993-94 a "special" period, when voters had their first chance to judge the work of postcommunist governments, with high hopes of economic revival but growing awareness of the pain? Is 1999 (after ten years of experience) another special period, with lots of cynicism, disillusionment, and bitterness, even in the best-placed candidates for EU membership? It

will be important for comparativists to follow up on this important work. The authors have set a new and higher standard and have generated rigorous but tentative interpretations of the differentiation among postcommunist party systems and the quality of democracy achieved.

Regulating Finance: The Political Economy of Spanish Financial Policy from Franco to Democracy. By Arvid John Lukauskas. Ann Arbor: University of Michigan Press, 1998. 326p. \$52.50.

Peter McDonough, Arizona State University

A full quarter of a century has passed since Spain undertook its transition to democracy. That experience is now one of the most amply documented in the third wave of democratization, which began in Portugal in 1974. Arvid Lukauskas makes a welcome contribution to this crowded field.

Regulating Finance is, despite the title, a study of deregulation. The basic argument is that Spanish democracy, and in particular an electoral system that favored the development of a few large catch-all parties across a heterogeneous citizenry, created incentives for politicians to deregulate the country's financial structures. State managers opened up the financial system, making it relatively transparent in the face of opposition from interests favored under the old regime and in the midst of indifference on the part of the public.

The paradox suggested by the title comes from the fact that public figures—elected politicians and ministerial bureaucrats, rather than private bankers and financiers—initiated and carried through a program of deregulation. They gambled, correctly as it turned out, that deregulation would increase public revenues, which would promote the solvency of the state just as democratization was unleashing demands for governmental expenditures. Enhanced economic performance paid off in increased political support. Spanish politicians—notably, those associated with the Socialist Workers Party of Spain (PSOE), the socialist party that governed from 1982 through the early 1990s—did well by doing (the collective) good.

Regulating Finance is persuasive on several counts. Lukauskas captures the air of supreme pragmatism that enveloped Spanish elites and the public like an obsession during the years of the transition and their immediate aftermath. Most Spaniards yearned to overcome the radicalism that had sent the country into civil war when democracy had been tried in the 1930s. The once intransigent socialists were probably even more eager than their conservative rivals to embrace a political style—"model" would be misleadingly precise—that favored economic growth, one with a distributive cast that rendered it palatable to a democratic clientele.

Lukauskas takes us through a careful case study of the ins and outs of financial reform during the center-right and later the socialist governments that made and consolidated the transition. His concluding chapter extends the analysis by examining variations in financial liberalization in Brazil, South Korea, and Turkey, all of them similar to Spain in terms of the transition from developmental authoritarianism but different in their postauthoritarian policies and economic performance.

Most of the book is a resolutely rational choice account of the ways in which institutional incentives shunt self-interested politicians down vote-maximizing pathways. At the same time, a truly convincing facet of *Regulating Finance* lies in the acknowledgment of the limits of institutional constraints in accounting for political behavior. Lukauskas is alert to the fallacy common in some soft rational choice work, to the effect that once you have the incentives straight, understanding the course of political action is fairly straightforward. That approach tends to gloss over the strategic complexity, erratic learning curves, and multiple equilibria—in brief, indeterminate outcomes—generated by even short-term interactions among multiple players. The case study material presented in *Regulating Funance* goes a long way toward curbing this trivialization of rational choice theorizing, even if Lukauskas does not develop a formal model of the politics of deregulation.

In the exogenous zone of the causal equation, Lukauskas also recognizes that a variety of factors in addition to institutional rules—historical legacies, culture, cleavage structures, transnational influences, and so on—may condition political outcomes. One benefit of the cross-national foray in the concluding chapter is that it enables Lukauskas to deal with these outside factors in something like systematic, if summary, fashion. (The next step might be to extend the perspective used in *Regulating Finance* beyond the standard political economy framework to encompass not only different countries but also different and less self-evidently instrumental families of policy disputes.)

Yet, it is also in the more axiomatic treatment of causal elements that the book runs into trouble. Lukauskas claims, at length and repeatedly, that Spanish politicians are self-interested actors who seek to retain power and that this ultimately explains why they responded to changes in institutional incentives in the way they did. It is possible to agree with the first assumption (indeed, it seems impossible to disagree with it) and still wonder what exactly it has to do with the second assertion. Since the self-interest/power-retention maxim is not a variable at all, it is difficult to see what explanatory function it serves, except the peripheral one of a kind of mantra to assure the reader that, yes, even Spanish politicians are rational and that the author has not been gulled by rumors to the contrary among purveyors of the Hispanic mystique.

The difficulty here is not with the supposition of rationality or self-interest but with the doubtful utility of a construct so generically construed that it fails to clarify the specifics of motivation and behavior. Lukauskas lists the names of more than a dozen government officials and politicians he interviewed, but it seems he never asked them directly about the reasons for their decisions or about what incentives they believed they were responding to; if he did, he does not report this information. Not a single quotation, not one sound of any Spanish voice, appears in the book. An editorial case can be made for this mode of presentation, but the result is unduly schematic. It is hard to shake the feeling that this chaste realism derives from the economist lore that measuring anything so ethereal as an attitude or a perception, a preference or a taste, is nowhere near so important as deducing it. This assent to the higher squeamishness creates a forensic problem. Lukauskas is left to "contend" or to "claim" or to "argue" that Spanish elites must have been motivated in one way or another, when he might have told us something about how they themselves interpreted their actions at first hand.

The argument is consistent with the evidence presented. But other explanations and interpretations are also consistent with similar evidence. Sofia Pérez covers much the same ground in Banking on Privilege: The Politics of Spanish Financial Reform (1997) and in "From Labor to Finance: Understanding the Failure of Socialist Economic Policies in Spain," Comparative Political Studies 32 (September 1999): 659-89, and she arrives at rather different conclusions about the rationale and beneficence of economic liberalization. It is an indicator of how far we have come in studies of democ-

ratization, not to mention from disquisitions on what used to be called the relative autonomy of the state, that genuinely sophisticated works such as *Regulating Finance* can produce disagreement for methodological and substantive reasons without detracting from the valuable advance Lukauskas has made over previous research.

After the Propaganda State: Media, Politics, and "Thought Work" in Reformed China. By Daniel C. Lynch. Stanford, CA: Stanford University Press, 1999. 327p. \$55.00.

Yanqi Tong, University of Utah

The fact that the Chinese communist government has been losing control over the flow of communication is no news to the student of Chinese politics. Yet, Daniel Lynch is the first to describe this development in detail and analyze it systematically. Combing through a wealth of materials (official documents, newspapers, journals, and interviews), the author thoroughly traces the erosion of the propaganda state of the Maoist era and offers a colorful picture of what he calls "a praetorian public sphere" in China today.

The central argument is that a combination of administrative fragmentation, property rights reform, and technological advance has led to the breakdown of the party-state's ability to engage in "thought work," a Chinese communist term that refers to the party-state's struggle to control the communication flows that shape the perceptions, attitudes, and values of the population. Lynch suggests that these three trends have subjected China's mass media to the processes of commercialization, globalization, and pluralization. Commercialization refers to the increasing tendency of the mass media to cater to the tastes of its customers in order to boost profits. Globalization refers to the importation of foreign messages. And pluralization is the proliferation of sources of information.

Although these processes have seriously eroded the state's ability to engage in thought work, Lynch notes that they have not produced a liberal public sphere in which individuals and groups in civil society freely create and circulate thoughtful political messages in a well-structured and institutionalized manner. Instead, borrowing a term from Samuel Huntington, Lynch argues that China's public sphere is characterized by a form of "praetorianism." That is, neither the state nor any other organized political force can impose order on the mass media, and the construction of culture becomes excessively market oriented without being directed toward the achievement of public political goals.

This is a fascinating book for those interested in the evolution of Chinese media and communication markets (television, radio, newspaper, telephone, publishing, advertising, and journalism) over the past twenty years. The processes of commercialization, globalization, and pluralization in China are carefully documented and analyzed. The book is full of detailed information about the changing forms of mass media, such as paid news, soft advertising, and "edgeball" journalism (which pushes the limits of permissible discourse), that are characteristic of the Chinese political system in transition.

Lynch also pinpoints, although somewhat implicitly, an important theoretical issue. Recent advances in information technology have produced a revolution in communications, which in turn will very shortly change the means of political interaction. Specifically, the rapidly expanding public sphere produced by the Internet will challenge authoritarian regimes in ways that no established theories can fully predict. This

volume has no doubt laid a foundation for future research on this exciting subject.

I do, nevertheless, have two reservations. First, the author has not pushed his work to the theoretical level that he is capable of achieving. The breakdown of the state control over thought work in China and the emergence of a chaotic sphere of public discourse are integral parts of the broader transition from totalitarian and authoritarian rule. This could have provided the basis for the author to present some more theoretically informed generalizations about the process. Although Lynch does try (in chap. 7) to locate his conclusions in the wider literature on transition, this part of the book seems to be more an afterthought than an integral part of the analysis. The author correctly notes that the current literature on transition overlooks the role of the media and the revolution in communications, but he does not specify the extent to which his work may contribute to theory building in this regard.

Moreover, although Lynch identifies administrative fragmentation as one causal variable that explains the emergence of a "praetorian" public sphere, he gives the impression that the state is merely a single-minded actor trying desperately, but unsuccessfully, to regain the control over thought work that it unintentionally lost when it launched economic reform (pp. 12–5 and chap. 6). If the author could come up with an analytical framework that would reconcile these two seemingly contradictory themes, then the theoretical implications would be much broader. After all, the commercialization, globalization, and pluralization of the mass media that was once (and to a large extent still is) part of the state apparatus requires a more nuanced analysis of the state.

A final and minor reservation concerns the author's use of property rights reform as a major causal factor (p. 7, and pp. 41-4) in his analysis. Lynch's basic argument is that now that the mass media have become responsible for their own profits and losses, they seek to maximize their profits by way of commercialization. What is not clear is to what extent this responsibility for profits and losses (more precisely responsibility for profits only) is the result of property rights reform. Surely, all the media stations and most publishing houses are still owned by the state. The introduction of market mechanisms, and the resulting change in incentive structures, seems to be a more appropriate and straightforward causal factor than property rights reform in explaining the commercializing of the mass media.

In short, this book is the best available description of the transformation of the mass media in post-Mao China. The author would have made an even greater contribution if he had been able to show how this is part of the broader transition from totalitarianism, and how in turn it will challenge the state's effort to preserve authoritarian rule.

Ideology and Change: Transformation of the Caribbean Left. By Perry Mars. Detroit, MI: Wayne State University Press, 1998. 230p. \$65.00.

Horace G. Campbell, Syracuse University

The violent implosion of the Grenadian Revolution in October 1983 brought to a close a phase in Caribbean politics when groups calling themselves the Left sought to use their place in government, trade unions, and universities to put forward an alternative agenda. This form of political intervention can be compared to other forms of interventions in the Caribbean. The most enduring has been that of the Rastafarian movement, which placed the stamp of Babylon

on official society. This movement, despite its contradictions, calls for a rejection of Enlightenment philosophies that placed an artificial distinction between spirit and matter. In a profound sense, the Rastafarians have made a fundamental contribution to the Caribbean in exposing how the ideas of the Enlightenment with regard to linear determinism, mechanistic predictability, and property rights were based on Eurocentric conceptions of society. A second contribution has been made by feminists, who have critiqued leaderism and androcentrism in Caribbean politics. Rhoda Reddock, in Women, Labour and Politics in Trinadad and Tobago (1994), attempted to correct the preoccupation of political scientists with only half the population and the inadequacies of the theoretical frames through which the societies in the region were analyzed. By focusing on an individual, Reddock demonstrated the contradictions of the Caribbean scholars who were reared in the Anglo-American tradition of focusing on great men. The Grenadian, Rastafarian, and feminist contributions provide a context for the book by Perry Mars on the transformation of the Caribbean Left.

Ideology and Change takes the reader through the past forty years of Caribbean politics, with particular attention to those at the forefront of the "radical" organizations. The author defines leftist politics as "the study of the groups and individuals seeking to challenge or change the dominant ideology and political system by a variety of conventional or unconventional means" (p. 40). Apart from the New Jewel movement in Grenada, which came to power outside the Westminster model of politics, the main parties and organizations adopted conventional means in their quest for power.

Some of the political leadership in the Caribbean associated itself with the ideas of Fabianism, the Social Democracy of the Second International, and other conceptions of Left politics from Marxists in Europe, and Mars demonstrates the borrowings at the level of ideology as well as the ambivalence of the middle class. He takes the reader through the global context, the specific Caribbean conditions, the genesis of the Left, the agenda of the Left, the quest for power, leadershipmass contradictions, destabilization and disintegration, ideological effects, and the future of left-wing politics. The book's silence on the Cuban and Haitian revolutions is compounded by its equivocation at the theoretical level on the concept of revolution. One major limitation of intellectuals in the English-speaking Caribbean has been their failure to probe the depths of the Cuban political process and its effect on the Western hemisphere. Throughout the text the author essentializes the concept of the Left and takes the self-proclamations of various leaders at face value.

One point brought out is that thus far Caribbean political scientists have been unable to place their own stamp on an alternative conception of the political process. This task has been attempted by feminist scholars and by historians, but political scientists are still caught in the mold of organizational theory, modeling, opinion polls, structural functionalism, and the elements of politics that emanate from the systems analysis theories of American political scientists. The book examines the leftist ideology of leaders and organizations, but it is not clear how different these are from the conventional forms of state legitimization in former colonial territories. The manipulation of nationalist symbols and radical language has been the hallmark of neocolonial leadership throughout the underdeveloped world.

Ideology and Change is part of a wider discussion on the moral responsibilities of the Left. Recent calls for a truth and reconciliation commission in Jamaica and for an investigation

into the assassination of Walter Rodney in Guyana expose the unease with the record of sections of the Left. Former leftist leaders have published books, such as Trevor Munroe (The Cold War and the Jamaican Left, 1992). Others, such as Rupert Lewis, have investigated Rodney's intellectual and political thought. It is significant that Lewis, who was along with Munroe a member of the same leftist formation (the Workers Party of Jamaica [WPJ]), took a different path from Munroe after the fall of the Berlin Wall. There is now throughout the Caribbean the demand for self-criticism on the part of the Left for its role in Grenada. The place of the Peoples National Party under Manley and that of the WPJ under Munroe in the struggle for change is analyzed by Mars, and he was able to avoid the pitfall of placing too much emphasis on charisma and other formulas that have dogged the study of Caribbean politics since the time of Eric Williams.

The other important study that appeared at the same time as that of Mars is the book by Winston James, Holding Aloft the Banner of Ethiopia: Caribbean Radicalism in Early Twentieth-Century America (1998). This important historical work drew attention to the origins of the radical traditions in the Caribbean in the twentieth century and brought out the interconnections in social conditions throughout the region and the linkages among organizations irrespective of language. The James contribution stands out in exposing how the radical traditions of the Caribbean Left made fundamental contributions to radical intellectual and political traditions in the United States.

The contribution of Marcus Garvey and other Caribbean nationals—such as C. L. R. James, George Padmore, Elma François, and Walter Rodney—to the ideas and organizations of those struggling against oppression all over the world is now well known. Less well known are the antidemocratic conditions in the Caribbean that constrained their linkages to the toiling masses. Mars brings out the challenges faced by those who seek fundamental change, and from this book it becomes clearer why people like Rodney or Garvey cannot thrive in political structures of the Caribbean. What is needed are new structures, which is one conclusion that can be drawn from the book.

Another is the more explicit conclusion of the author that there is no future for leftist politics in the English-speaking Caribbean. This is not the view of Winston James, Rupert Lewis, and Rhoda Reddock. The difference stems from their different starting points for discussing the Left. The conclusion of Mars is flawed precisely because the theoretical base of the book is essentialist in defining the Left. It would be difficult to term as leftist the practices and ideas of such groups as the People's Progressive Party and the People's National Congress in Guyana, or the People's National Party of Jamaica, despite claims of the leadership. Mars himself introduces the novel concept of "democratic dictatorship" to characterize the government of Forbes Burnham in Guyana. One then wonders how the same author who documents the brutality of this government can label it "Left." More important, the manipulation of race and ethnicity on the part of both the Guyana parties left a tradition of division and insecurity that was incompatible with a society seeking the confidence for change and transformation.

The lack of a critical analysis of the transformation issue reinforces the theoretical flaws of the book. Another weakness is the absence of an analysis of the limitations of androcentrism in Caribbean politics. In theory and in practice, those seeking a new mode of politics in the Caribbean have made striking contributions in the past decade. In the case of Guyana, a formation such as the Red Thread seeks

consciously to rise above both the forms of electoral competition and the divisions of the working people. By seeking to develop an alternative space for political mobilization for women, this group is demonstrating that the transformation of politics is bound up with addressing the insecurities of the middle class and their Eurocentric preoccupations. The present conditions of politics in both Trinidad and Tobago and Guyana attest to the legacies of the middle class in dividing the working people and manipulating the symbols of progressive politics. In a profound sense the Mars book points to the necessity for turning away from the intellectual retrogression of the middle class.

Parliamentary Representation: The Case of the Norwegian Storting. By Donald R. Matthews and Henry Valen. Columbus: Ohio State University Press, 1999. 211p. \$49.95 cloth, \$23.95 paper.

Knut Heidar, University of Oslo

This is a fresh and insightful book by two major contributors to empirical political representation research for more than 40 years. Their general question springs from the concerned democratic tradition that asks how "the many" can be represented in political institutions by "the few." In this case the institution is the Norwegian parliament, the Storting, and the answer is that the Norwegian representative democracy seems to be doing well—particularly in comparative terms despite successive minority governments, multipartism, a consensual style in politics, and other factors that make for a somewhat hazy elite accountability. The authors are, however, reassured by their finding that "much of Norwegian politics consists of the active minority, including members of the Storting, trying to anticipate what the interests and preferences of the quiescent are, or might become in the future" (p. 182).

The theoretical problem raised in the book is "how legislatures and legislators represent the absent others" (p. 2). In discussing models and methods in representation research, the authors argue that U.S. dominance in the field is "not always helpful in comprehending representation in other legislatures" (p. 3). Their own approach distinguishes among three types of responsiveness—policy, service, and symbolic—and two levels of representation, the individual parliamentarian and the aggregate parliament. They also emphasize that "representation" is a process, and they identify three models in the literature that give different answers to how this process produces representative assemblies: the party mandate, corporate pluralism, and principal-agent models. These are not tested but are presented as heuristic devices to help understand representation processes, and Matthews and Valen criticize much contemporary research for being both too individualistic and too static and ahistorical.

The empirical basis of the research derives from several projects carried out in conjunction with the 1985 Storting election: a voter survey, a survey of delegates to the party nominating meetings, a survey of the elected Storting representatives, and a data file with biographical information on Norwegian MPs. The book provides historical perspective on the evolution of Norwegian politics since the nineteenth century and in practice gives a broad introduction to the Norwegian political system as it operates in the latter decades of the twentieth century. There are discussions about the

development of Norwegian consensual democracy and its cleavage structure, the roots of the electoral system and its current Proportional Representation formula, the basic institutional setting of the Storting, and basic facts about the party selection of candidates for the electoral lists. Then the focus shifts to voters, activists, and their opinions before we are given a tour of the parties—their members, activists, leaders, and manifestos. Finally, Matthews and Valen examine the "groups" represented in the Storting in terms of MPs' sociodemographic profiles before they turn to the organization and political processes inside the Storting itself. All is well structured and well produced.

This skeleton description of content cannot do justice to the richness of the empirical material. The litmus test of the book is whether its analytical focus generates general and comparative findings that advance scholarship. In my view the book passes well on that score. A small quibble is that the authors could have given more space to general reflections over "the case" and the typologies and models—to highlight, criticize, and suggest—toward the end. But these are rocksolid folks, not much prone to lofty pronouncements.

In the central part of the book-chapters 5-9, which address the relevance of the three models of representative processes—there is plenty of food for the general discourse on representative democracy. Only a tiny elite (5%) contributes to the selection of candidates for the Storting list, and this group is not very representative of voters in terms of age, gender, education, and occupation. The voters have a low level of name recollection when asked about the candidates in their constituency (chap. 5). Actually, the voters are not very interested in politics; they are not very knowledgeable and have, with the very significant exception of voting turnout, a low level of participation. A small, self-chosen elite of about 15% of the electorate is active, interested, and informed. The good news is that this elite actually is fairly representative of Norwegian voters in terms of sociodemographic composition (chap. 6). In the discussion of political parties, the mandate model is not found to fit very well, as the voters do not respond much to party positions on contemporary issues. Without sufficient knowledge, they vote more on the merit of past party performance than on the issues of the day. The MPs vary in terms of representative focus (district, party, particular groups) and experience a varied input of viewpoints from the outside through numerous group contacts. They also become policy specialists through their committee work, but the parliamentary party groups appear to be rather effective agents in shaping the policy profiles of the representatives.

Matthews and Valen conclude that the mandate theory is difficult to apply in Norway since the linkage is weak between electoral opinions on current issues and consensual party politics in the Storting. The merits of the corporate pluralist and principal-agent models are left more in the dark. This is probably so because the implications of the mandate theory are more clearly spelled out than those of the other two. This volume, as a good book should, leaves us with both new answers and more questions. How could these models be given a more cutting edge? That would give the Norwegian case (and others) potential for carrying even more fuel to the general and comparative debate on how to make the "absent others" present.

For teaching purposes I recommend the book for general courses in European and Nordic politics as well as for courses in political representation.

The Agony of the Russian Idea. By Tim McDaniel. Princeton, NJ: Princeton University Press, 1996. 201p. \$14.95 paper.

Arthur H. Miller, The University of Iowa

All post-Soviet societies faced the challenge of constructing a new identity that is both national and democratic. Yet, not long after the Soviet demise, initial public optimism about the future of a new Russia was replaced with growing concerns about corruption, crime, economic uncertainty, and the absence of a unifying national identity. In an attempt to address the latter issue Boris Yeltsin, after his reelection in 1996, set up a committee to produce a new "national idea" that would define the Russian essence and inspire its people. At the time of this writing, the committee is still searching.

A book entitled *The Agony of the Russian Idea* should have something worthwhile to say to the Yeltsin committee and to scholars interested in the historical development of Russian national identity. Both groups may be disappointed. The committee will find a very pessimistic interpretation of both the historical path that has produced the various elements comprising the "Russian idea" and what the future holds for contemporary Russians. Scholars will find that, while the author realizes there are two ways to deal with a historical presentation of the Russian idea, he adopts the one that promotes an argument rather than a careful analysis.

Early on McDaniel notes that the Russian idea may reflect that which is seen to be distinctive about Russian culture and institutions or it may represent an ideal model of society based on whatever elements the writer believes are the predominant elements of the culture. This is not a traditional intellectual history but McDaniel's interpretation of work produced by Russian intellectuals, who in many cases have presented an idealized version of the principles inherent to Russian life, culture, and institutions. This does not mean that scholars should dismiss the book as irrelevant, but they should realize what it does and does not do. It is a good example of how the evaluation of any study of Russian national identity has been changed by the fall of restrictive communism.

Today, we are able to examine both statements of what beliefs are prevalent among Russians and their idealized hopes for tomorrow by using surveys to probe these questions directly. Such evidence can then be compared with the statements of those who write about contemporary history. McDaniel's argument is most convincing when he focuses on earlier periods of Russian history, because we have no readily available corroborating evidence against which to test his statements. But when historians stray into contemporary times, their interpretations and outright claims for reality are open to challenge, now even in Russian. Thus, the standards for scholarship are raised and moved from the realm of pure opinion or journalism to the realm of science, with its demands for validity and replication.

The "Russian idea," McDaniel tells us, reflects "the conviction that Russia has its own independent, self-sufficient, and eminently worthy cultural and historical tradition that both sets it apart from the West and guarantees its future flourishing" (p. 11). He goes on to say that all the Russian thinkers who address the idea posit a higher form of modernity for Russia. Various principles and values of Russian culture are presented as the unique elements of the Russian idea. These are readily identifiable in the writings of the intellectuals cited by McDaniel, and chapter 1 is devoted to cataloging the positive and negative elements that comprise the Russian idea.

McDaniel presents three "negations" that contradict the presumed uniqueness of the Russian idea as well as introduce

the negative aspects of presumed Russian identity. The contradictions involve the realization that the Russian idea is not uniquely Russian but partly dependent on a reaction to the West, and there is not one Russian idea but competing visions of what was dominant in Russian culture during the nineteenth century as well as today. According to social psychological and anthropoligal theories of identity, all social and national identities have an "us" versus "them" quality. For the Russians, the West and its stereotypical features (capitalism, materialism, individualism, legalistism, rationalism, and immorality) epitomized "them."

Within the "four affirmations," McDaniel presents Russian identity or the image of "us" as based on ultimate goals or values: a higher moral order; nonrationality, a preference for communal interpersonal relations, such as occur in the peasant communes, as opposed to legal and formal interactions; an emphasis on equality; nonmaterialistic values; and a penchant for a paternalistic state. Such is the picture of traditional Russian culture as portrayed by the Slavophile intellectuals.

Having laid out the content of the Russian idea, McDaniel goes on in later chapters to argue that the end of tsarist and communist Russia occurred because of the contradiction inherent in a state that promoted modernization while trying to maintain the traditional Russian idea. Communism was doomed to fail because "instead of the old values of selflessness, modesty and compassion, Soviet Communism had ushered in a stage of crass Americanization of values. Young people especially seemed to be absorbed with technology, athletics, and practical results; they had no time for the moral dilemmas explored by classical Russian culture" (p. 92).

Today, according to McDaniel (chap. 5), the Yeltsin government is also sowing the seeds of potential future collapse because reforms are based only on economics, thereby ignoring the spirituality of the Russian idea. In order to succeed, current reforms must provide a bridge to a past Russian identity by promoting the ideals of equality, moral virtue, community, and the responsibility of government for social welfare. While I agree that post-Soviet reformers might be well advised to work toward those goals, one could hardly argue that these are uniquely Russian. All governments are well advised to pursue such goals. In short, this book does not provide the answer for the committee seeking a new Russian idea. That committee should be told, however, that identities arise out of long-term shared experience, not committee deliberations.

Certain aspects of the traditional Russian identity discussed by McDaniel can be found among the attitudes of contemporary Russians (particularly concerns about inequality), but the book is replete with statements that would have been well informed by a familiarity with current survey research. Perhaps the most egregious misstatement in the book is the following: "Communism is now widely regarded as a German plot, or the evil work of non-Russian nationalities, particularly Jews and Georgians, who imposed these non-Russian ideas upon the innocent Russian people" (p. 53). No doubt one can find some Russians who express this view, but to claim that this is a "widely" held belief is simply inaccurate. The reformed Communist Party is the single strongest party in Russia today, and slightly more than 60% of Russians still think of themselves as Soviets, so there is little evidence that the former regime is regarded as a hated foreign plot. Given the ideas expressed in this book, it should provide excellent grist for the critical thinking of any graduate seminar examining the concept of identity.

Inequality, Democracy, and Economic Development. Edited by Manus Midlarsky. Cambridge: Cambridge University Press, 1998. 281p. \$64.95 cloth, \$24.95 paper.

Jacek Kugler, Claremont Graduate University

This collection of essays provides a multifaceted perspective on the process of economic development and its connection to politics and inequality. The reader is exposed to a number of approaches, whose results challenge existing connections among these elements and open new vistas on the complexity of such interactions. A number of possible paths for future analysis are followed. This collection, however, does not provide a unified structure that could be used to build a theory of development; rather, it encourages readers to research and resolve the paradoxes of democratic development

A useful way to approach this volume is to consider many of the counterintuitive insights about democracy and development. Evaluations of politics in ancient times by Bollen and Paxton, for example, show that hunting and gathering societies achieved levels of participation and equality still unmatched in developed societies. Using the ethnographic and anthropological record, Ember, Ember, and Russett argue compellingly that in ancient societies social equality strengthens democracy. This result challenges modern findings that equality weakens participation. They also show that female participation increases in peacetime and declines during conflict. Such evaluations again challenge suggestions that war mobilization opens new opportunities for women. Likewise, Midlarsky and Midlarsky find that the availability of rainfall fosters democracy, while frequent exposure to conflict and permeable borders among landlocked nations diminish the likelihood of democracy. While these works do not reach a consensus regarding the source of democratic evolution, they demonstrate that the development and sustainability of democracy are contingent on long-term structural conditions.

A major section of the book is devoted to the proposition that economic development leads to inequality and democracy. Muller extends Lipset's finding that societies in transition from very low to high income levels may find their political system—whether democratic or authoritarian challenged frequently. Moreover, he shows that income inequality tempers efforts to develop democratic structures. Thus, in rapidly growing societies, with serious inequality, democratic or authoritarian rule is precarious. Muller's results are challenged by a number of other researchers. Simpson attributes democratization to improvements in communication and literacy rather than economic growth and equality. Coppadge demonstrates that economic growth by itself is insufficient to prompt democracy. Democracy will flourish in societies that achieve economic and social performance above the mean, but empowered citizens are the prerequisite for any such movements. Crenshaw, again, challenges the notion that democracy is directly connected to growth and inequality. Instead, he finds that the expansion of technology, which in turn creates interdependence, generates the conditions under which competing political elites have leverage to pursue democracy. Finally, Simpson demonstrates that economic development has little independent effect on democratization. He argues instead that literacy generates the preconditions for democracy.

These articles all show different flows in the main thesis of modernization that connects economic development, equality, and democracy. New proposals about the underlying democratization process are proposed, but although the original argument is seriously damaged, none of the alternatives is compelling enough to replace the modernization thesis. In the end we know the connections between democracy and various aspects of modernization are far more complex than anticipated, but we still do not know what these connections are.

Another major section explores the relation between inequality and democracy. A number of possible approaches are taken to explore this interaction. Gasiorowski examines the relation between democracy and wage growth. He demonstrates that wages and growth regularly decline before a transition to or from a democratic to an authoritarian regime, but authoritarian governments do not slow growth or wages. Thus, wage and growth effects are related to transitions and are not associated with specific systems in place. Challenging assumptions about a regular path of development, Greskovits shows that Eastern European transitions do not follow patterns established by the developed North and developing South. These experiences suggest that democratic transitions can have multiple and contradictory effects. Chan reverses the causal arrow and explores how democracy affects inequality. He shows that once democracy was established, East Asian nations used the welfare system to equalize access to education and social services. These results are consistent with the Aristotelian proposition that democratic leaders will redistribute and will equalize income, presumably to increase participation and generate electoral support. Finally, in an overarching essay, Di Palma argues that market economies are necessary for social society, but markets do not suffice to prompt democracy and do not assure its permanence. Successful democracies find a compromise between the requirement of reproducing capital and achieving political consent. Classic thinkers would be comfortable with such insights.

In sum, the book successfully presents a number of conflicting perspectives that detail aspects of the evolution of democracy. Most of the empirical chapters debunk long-held assumptions and proposed direct links among growth, inequality, and democracy. The contributors consistently hint that these relations are very complex, that indirect and nonlinear effects dominate the interactions. What the book fails to deliver is a plausible general proposition or a consensus about major findings. It does not provide a general approach to the subject or reject elements that are empirically shown to be irrelevant. The evidence presented is sufficient to challenge existing constructs but insufficient to lead to a new comprehensive understanding of economic development, democracy, and inequality. Perhaps this is the inevitable consequence of the state of the field at this time. The many questions posed now have a chance of obtaining consistent answers.

Who should read this book? It is a must for advanced students of political economy and development, who will find this volume a major source of both theoretical and empirical inspiration. The number of approaches, temporal structures, variable specifications, and alternative methods used to elucidate these complex relationships is impressive and inspiring. Graduate students in particular will find a gold mine of meights, data, and research designs and will be rewarded by the complex exploration of topics that are central to our understanding of the connection between economics and politics. This work will motivate a number of scholars to look in the right direction and answer the unresolved puzzles of democracy so effectively laid out by the editor in the concluding chapter.

Rural China Takes Off: Institutional Foundations of Economic Reform. By Jean C. Oi. Berkeley: University of California Press, 1999. 253p. \$35.00 cloth, \$17.95 paper.
Contesting Citizenship in Urban China: Peasant Migrants, the State, and the Logic of the Market. By Dorothy J. Solinger. Berkeley: University of California Press, 1999. 444p. \$50.00 cloth, \$19.95 paper.

Dali L. Yang, University of Chicago

For the past two decades, scholarly attention on China has grown steadily as that vast country has reformed its planned economy while sustaining a stellar growth record. Not surprisingly, the Chinese experience has become highly significant for generating and assessing models of economic reform.

Rural China seeks to explain the sharp rise in output produced by rural enterprises, primarily in the 1980s, when publicly owned township and village enterprises led the upsurge in rural industrialization. Building on her 1992 article in World Politics and other writings, Oi concludes: "The rapid takeoff of China's rural industry was the result primarily of local government entrepreneurship" (p. 2). Oi draws on agency theory to analyze "the more successful cases" of industrialization (p. 200). She notes in chapter 2 that at the turn of the 1980s the institutional environment for rural development was transformed. Decollectivization of agriculture sharply reduced the income that local cadres could generate from agriculture, but fiscal reforms in the form of revenue contracts not only tightened the budget constraints of local governments but also offered local authorities rights to the revenue stream and limited central government capriciousness. The conjunction of these two institutional changes provided powerful incentives for local officials at the county, township, and village levels to promote industrial development. Private sector expansion was restricted for political and ideological reasons, and local officials gave preference to collective enterprises, especially the township and village enterprises.

Oi recognizes that the revenue imperative was a necessary but not a sufficient condition for rural industrial growth. In chapter 4 she argues that it was the institutions inherited from the Maoist era that provided local governments with the political capacity and policy instruments to foster the rapid growth of rural industry (pp. 95-6). She believes that this legacy, refitted for the reform era, is the foundation for a distinctive form of state-led growth. She calls it local state corporatism and claims that it is a "new form of development that is committed to growth and the market, but it is led by a party-state with roots in a Leninist system and with the Communist Party still at the helm" (p. 97). Local governments and their enterprises are in a relationship of mutual dependence rather than predation. As a result, Oi concludes, "large extractions of revenue have not negatively affected growth" (p. 98). Thus, even though local authorities gained more power and resources vis-à-vis the central government, they have practiced "positive intervention" in the economy

Oi thus joins most China specialists in pointing out that regime change is not necessary for reform and growth in a Leninist political system. Indeed, the reforms in China have served to strengthen the power of officials who pursue development. In the process, China has become "a qualitatively new variety of developmental state and not merely a modified Leninist system" (p. 192).

Although carefully crafted, Oi's study leaves a variety of lingering questions, some of which the author mentions in the concluding section. The spirit of constructive criticism re-

quires me to contribute to this conversation. For example, Oi states that the overall environment had become so friendly to development that local officials had little reason to obstruct the reforms. Moreover, the "incentives presented a legitimate alternative to corruption" (p. 56). This seems to contradict popular reports of rampant corruption in China. Indeed, in light of the Asian economic crisis, her favorable comparison of government entrepreneurship in China with that in the developmental states of East Asia also raises questions about the quality of China's growth. Not only is the degree of official intervention in enterprises greater in China than in other East Asian states, but also it is well known that local officials in China have tended to exaggerate output figures from rural enterprises. In this sense, the Chinese variety of developmental state may share the weaknesses that have afflicted others in the region. In fact, Oi notes that local officials have started to readjust their relationship with businesses in the more stringent economic environment of the 1990s. Finally, although government entrepreneurship apparently contributed to developmental successes in some places, it is worth noting that in other places the more local officials set up township and village enterprises at great expense, the poorer farmers became (Fortnightly chat, no. 13 [1999]). The state that Oi finds doing good in some places can also turn predatory in others. Oi alludes to such variations here and there, but it is hoped that future studies will pay more attention to this issue in order to determine the causes.

Whereas Oi emphasizes the positive role of the local state in generating China's rapid economic growth, Solinger focuses on the government's uneasy relationship with the millions of rural migrants in the cities. For Oi, rural industrialization was at the core of China's "economic miracle" (p. 1). Yet, although some rural communities along the coast have become models of economic success and destinations for migrant workers, a great divide continues to separate rural and urban life in China. In much of rural China, particularly the hinterland, life is still very hard indeed. In consequence, tens of millions of rural residents have left for the uncertainties of urban life as migrants or transients.

By examining this "floating population," a category that straddles the historical segmentation between urban and rural, Solinger is able to illuminate the nuances, ambiguities, and complexities of China's economic reforms. There is little of the romantic musing that some scholars have used to discuss how unorganized individuals have trumped the state. Instead, Solinger highlights how, despite the advent of the market, state institutions continue to constrain and limit the populace's spatial mobility more than in most other nations.

Contesting Citizenship is composed of two parts, each with three chapters, all backed up with a wealth of information from extensive ethnographic fieldwork and Chinese publications. Part I focuses on the structure of policies and institutions—the urban bureaucracy and the rationing or entitlements regime—with which the migrants must contend and to some extent subvert. It shows that the market has expanded against the backdrop of powerful statism in China. Not only are the networks through which the migrants make their moves partly social and personal and partly bureaucratic, but also the migrants must live with various forms of stateimposed inequities. Unlike the United States, where those from outside a state can qualify for in-state benefits, such as reduced tuition rates, after a period of residency, Chinese transients who lack urban residency (hukou) are excluded from various goods and services, ranging from education to health care to welfare benefits, to which those who possess legal urban residency are entitled. Overall, Solinger concludes that this institutional environment severely undercut the migrants' "opportunities for acceptance and inclusion among the city folk as citizens on regular terms" (p. 149). The market has not transformed peasants into citizens (although it should be noted that some once rural communities in the Pearl River Delta are doing better than most cities).

The picture is not as stark as it appears. In Part II, Solinger finds an expanding range of agency for migrants. Their very presence in cities stirs the urban labor market and has contributed to a breakdown in the government's capacity to affect the disposition of the urban work force. As a result, some urban residents have experienced downward mobility, albeit cushioned by the rudimentary state welfare system. In consequence, there has been some convergence between rural and urban.

Most interesting is Solinger's emphasis on the variations among the migrants. Some, particularly the unskilled and less educated from the inland provinces, have found second-class citizenship by signing employment contracts with urban enterprises through official sanctions. In contrast, those with capital and skills, generally from the coast, have paradoxically been living on the fringe, often literally, in the cities. Denied the opportunities to assimilate, they have formed their own communities, such as the Zhejiang Village in Beijing, thus creating an alternative to the state-sanctioned citizenship. In the process, they have transformed "the meaning of life without formal citizenship in the city" and altered the "exclusivity of urban citizenship" (p. 241). Unfortunately, even these communities came into being because of state exclusion. Many of the transients would prefer official residency, which would allow them to send their children to good schools on the cheap and entitle them to other benefits.

Solinger concludes that China has so far seen no unilinear relationship between commercialization and democratic institutions. One wishes that she were more forceful in discussing the dynamics for change in the future. Also, she might have given more attention to how changing population dynamics, including negative growth rates in some cities, and the Chinese government's emphasis on technological development may affect the patterns of migration. Nevertheless, there is no better book than this for readers who want a single volume that underscores both the dynamics and ambiguities of Chinese reforms.

All in all, both books deal admirably with institutions and dynamics in the process of transformation in China. Readers from multiple disciplines will profit from reading them.

China's New Business Elite: The Political Consequences of Economic Reform. By Margaret M. Pearson. Berkeley: University of California Press, 1997. 217p. \$35.00.

Suzanne Ogden, Northeastern University

Margaret Pearson addresses a key question in the field of comparative developmental politics: the relationship between economic reform and political change. Her focus is on the role of business elites in bringing about political change. Although the book is in large part a case study, based on interviews in 1991 and 1995 with members of China's new business elite (notably, Chinese managers in the foreign sector and private entrepreneurs), its conclusions are based on Pearson's substantial knowledge of China and theories of developmental politics. She examines how these two key components of China's mushrooming business elite have reshaped state-society relations. Among her major conclusions are, first, that China's economic reform has in itself been insufficient to bring about political reform, specifically, a change in the relationship between state and society, and,

second, that rather than become part of a "civil society," China's business community is best conceptualized as working within a hybrid framework of "socialist corporatism" and "clientelism."

Pearson notes that China's new business elite does not necessarily perceive political reform, especially democratization, as in its interests. That is one reason this group has not forced political change, although it is in the best position to do so. Despite continuing economic liberalization, clientelism and corporatism seem to work better for this elite than the creation of a "civil society." Pearson acknowledges that the concept, which developed in reference to the sort of bourgeois society that emerged in Western Europe during the seventeenth and eighteenth century, is of questionable relevance to a reforming socialist state such as China in the twentieth century. She uses the term "civil society" to refer "to the emergence of a realm of activity outside and independent of the hegemonic communist party state" (p. 26).

According to Pearson, China's business elite has little interest in becoming an activist political community and building a civil society. Its members tend to be apolitical. They try to satisfy their own narrow business interests through clientelism: building personal relationships (guarva) with officials through gifts and favors (although Pearson rightly notes that most forms of clientelism in China do not involve illegality). Indeed, foreign firms operating in China realize the necessity to work with the state and develop "relationships" with officials in order to improve their business conditions. Participation by officials is not viewed as an obstruction to economic growth but as essential to its success. As a result, foreign firms often put key Chinese officials on the board of directors or high in management in order to use their connections and influence. For its part, the state does not try to eliminate clientelism, because it allows the state to satisfy the individual interests of members of the business community, thereby preempting any need for the latter to engage in activist, confrontational activities against the state.

The reliance upon clientelism and socialist corporatism results in large part from deeply embedded cultural attitudes and practices. Yet, the fact that culture continues to play such an important role has led to a cycle of cause and effect: Weak horizontal connections among the business elite reinforce traditional business practices. Furthermore, clientelism and socialist corporatism have been strengthened by the aversion to politics as a result of 30 years of campaigns, criticism, self-criticism sessions, and class struggle; a belief that the present communist party regime provides the stability, law, and order necessary for economic growth; and the view of businesspeople that lobbying is unlikely to lead to change in national economic policy. Because China's marketization is incomplete, moreover, businesspeople are still dependent on local officials, who retain some types of authority and control and do not hesitate to use "discretionary favoritism" in allocating resources and protection. So, rather than build business associations to lobby governmental agencies and offices, the business elite has strong incentives to cultivate personal ties with local officials in order to address their own sector's or enterprise's narrow needs and to affect the distribution of those resources still in the hands of the state. "Distributing goods in response to personal requests diffuses societal energy rather than concentrating it; it allows the state to buy off discontent short of a point where such discontent might foster organized lobbying or opposition" (p. 143).

Pearson notes that the new business elite has so far proven unable or unwilling to free itself from China's deeply embedded values and traditions concerning how the private should relate to the public and the society to the state. Indeed, it is

ironic that the business community, once economic decentralization and marketization gave it significant structural autonomy from the state, systematically went about reestablishing informal ties to the state by cultivating relationships with officials. Pearson finds the roots of the hybrid pattern of socialist corporatism combined with clientelism in Confucian thought, in the history of merchant-state relations, in prerevolutionary China, and in socialist China, but she never uses the term "political culture" to describe the source of this continuity between pre- and post-1949 China. She implicitly recognizes, but never states, that China's political culture has survived the demise of the imperial state, as well as the collapse of republican and socialist institutional forms, such as a centrally directed economy. China's traditional political culture is a key to understanding why the business community persists in its old habits of relating to the state instead of attempting to create a civil society. In short, Pearson could well have concluded that culture has as much, it not far more, to do with the pattern of society-state relations than does socialism.

Pearson may be correct that if the well-positioned new business elite does not bring about radical political reform, then no other economic group is likely to do so. But the entrepreneurial managers of town and village enterprises (TVEs), whom she dismisses as too conservative and not well positioned to force political reform, should not be overlooked; in local elections, the party is most likely to nominate, and villagers are most likely to elect, candidates who have successfully established or managed TVEs. Those who are elected gain greater power to challenge the basic society-state relationship, but this group is likewise bound by political culture to work with the state, not against it.

Pearson examines the state-created business associations, which represent the new elite but are dominated by retired officials, as part of the state's effort to coopt potentially autonomous social forces. Here, again, she notes that this is not a one-way relationship. Business benefits from the linkages of these officials to government bureaucracies because they give members access to "the vertical, informal clientelism embedded in these associations" (p. 135).

Pearson makes an important contribution to our understanding of the relationship between political culture and the development of civil society. Elements that in another cultural context might lead to the creation of a civil society tend to be quickly absorbed in China into a culturally rooted clientelism and socialist corporatism. As a result, the state's corporatist strategy is to encourage business to support economic development while restricting its political role. At the same time, the business elite has been allowed considerable financial success for its cooperation.

Pearson concludes by putting China into the East Asian context. Once she asserts that clientelism in China is far less functional for the purposes of development than elsewhere in East Asia, she is left with the difficult question of explaining China's ability to produce growth comparable to such East Asian states as Japan and Korea. It is beyond the scope of her book to address this question fully, but her partial conclusion may tell us much about the success of the "Chinese model." In her estimation, local officials in China may have acted as "corporate directors" for their localities, and the central state may not have interfered with "neophyte business entities in business, agriculture, or local government" (p. 158) as much as one might assume. In short, beneath a cloak of communism there beats a capitalist heart in many local Chinese officials, who have encouraged economic growth within the hybrid form of clientelism and socialist corporatism.

Pearson's book adds an important dimension to our un-

derstanding of how China's communist regime has been able to maintain broadly based support as it liberalizes the economy. Deeply entrenched cultural attitudes and practices predispose the new business elite to cooperate with the regime in a clientelistic pattern, a predisposition that is reinforced by socialist corporatism. It will take far more than the disappearance of communism or the creation of a multiparty system to eliminate this mutually beneficial hybrid pattern.

Islamic Political Culture, Democracy, and Human Rights. By Daniel E. Price. Westport, CT: Praeger, 1999. 221p. \$59.95.

Shahrough Akhavi, University of South Carolina

Does "Islam" cause authoritarianism? Samuel Huntington claims it is antidemocratic (*The Clash of Civilizations*, 1996). Daniel Price begs to differ and concludes that it neither undermines nor supports democracy and/or human rights. Actually, it was Ann Mayer who in 1991 argued that Islamic values can be marshaled on behalf of human rights schemes but that male Muslims have interpreted these values in a way adverse to such rights (*Islam and Human Rights*, 3d ed., 1999).

Price, who ignores Mayer—although he lists the first edition in his bibliography—uses a simple typology, following William Shepard ("Islam and Ideology," International Journal of Middle East Studies 19 [August 1987]: 307-36). The key concepts are "comprehensiveness," the degree to which the shari'ah (Islamic law) is applied, and "authenticity," the extent to which non-Islamic values are permitted to influence society. Operationalization of comprehensiveness is based on whether Islamic law "is used" (p. 145) in the following domains: (1) personal status; (2) economics; (3) social customs; (4) crime and punishment; and (5) governance. If shari'ah is not applied, then Price assigns a zero. If it is applied exclusively, then he assigns a three. Between these extremes are cases of modest and major use, which merit a two and three, respectively.

By contrast, the scale for authenticity ranges from 0-15: 0-2 represents a society in which non-Islamic ideas, institutions, and technologies [sic] are accepted without reference to Islam; 3-5 means they are accepted by rationalizing their compatibility with Islam; 6-8 is a society in which they are accepted but believed enhanced by Islamic ones; 9-11 means they are accepted, but Muslims make efforts to "trace their roots in Islam" (p. 146); 12-15 means they are rejected.

Leaving aside the asymmetry of the 12–15 category (the ordinal range is four as opposed to three in the other four categories), there is no clarification of the difference between rationalizing the compatibility of non-Islamic ideas and institutions, on the one hand, and "trac[ing] their roots in Islam," even though these two categories are on opposite sides of the intermediate position on Price's spectrum.

Price is aware of the difficulties encountered in assigning numerical scores to societies' performance on these dimensions, which in the final analysis are based on "guesstimates." Turkey gets a one on authenticity, and Senegal gets a "three or four" (p. 146). The decision as to what score to accord a society comes down to interpreting the evidence contained in leaders' statements and speeches; scholarship on the political systems involved; constitutions; media reports; information generated from these states' embassies in the United States; the author's personal contacts in these states; and survey questionnaire responses by members of the Middle East Studies Association of North America. Price then seeks to

collate and distill the data generated by all these sources into quantitative integers, which is a daunting task at the least.

Four descriptive chapters discuss Egypt and Jordan, Syria and Tunisia, Saudi Arabia and Morocco, and Algeria and Iran. Another fifteen Muslim states are included in the statistical analysis. The relationship between Islam and democracy is mediated by seven variables, four of which, because difficult to quantify, are excluded from the regression analysis. These four are historical influences, regime strength, regime strategy, and the strength and orientation of Islamic groups. The remaining three are quantifiable: modernization, presence of social cleavages in society, and the presence of a minority religious group. Among Price's hypotheses are: (1) an inverse correlation exists between the presence of politicized cleavages and democracy; (2) there is an inverse correlation between the existence of a minority religious group and democracy; (3) a curvilinear relationship exists between the degree of a society's "wealthiness" and democracy (beyond a certain threshold of wealth, the relationship becomes inverse); (4) the more rapid the economic change occurring in a society, the less democratic it will be; and (5) a positive relationship exists between the level of social mobility in a society and democracy.

After his statistical runs, Price concludes: "I have provided evidence in support of the argument that Islam is not a monolithic political force that is the primary cause of political outcomes in predominantly Muslim countries" (p. 177). When control variables are placed into the mix, the relationship between Islamic political culture and authoritarianism becomes insignificant.

To all this one can only say "amen." Should we be surprised? After all, a moment's reflection will reveal diversity in the social, economic, and political processes and institutions of contemporary Muslim states. Because of these variations, one may not logically use the constant of "Islam" to explain authoritarianism in these systems. If "Islam" were the culprit, then the polities of these systems would be characterized by uniformity. Price is to be commended for the industry he brought to the task of making sense of Islamic political culture, but such labors appear to have yielded a finding that is predictable on theoretical grounds.

The book contains a number of errors in grammar, syntax, orthography, and occasionally even substance. An example of this last is the statement that Islamic law "was finalized over eight hundred years ago and has not been significantly altered since" (p. 24). In fact, the *shari'ah* continues to evolve, in part because one of its sources is the opinion of jurists. In earlier periods, the juristic principle of *maslahah mursalah* (public interest) was scarcely known, but in the last two centuries it has been elaborated in a way as to be undoubtedly unrecognizeable by earlier jurists, including the Caliph 'Umar ibn al-Khattab (d. 644) or Malik ibn Anas (d. 795), al-Juwayni (d. 1085), al-Ghazali (d. 1111), or Najm al-Din al-Tawfi (d. 1316).

Other mistakes include Price's assertion that the Prophet Muhammad's second wife was a merchant (p. 25). Populace is rendered "populous" (pp. 27, 30). The transliteration of the word for a Christian or Jew (protected people in Islam) is given incorrectly as *dhummii* (it should be *dhimmi*); we get the expression "quranicaly justified" (as opposed to Qur'anically justified); and the two early dynasties in Islam are spelled Abasaid and Ummayid (the correct forms are 'Abbasid and Umayyad). (All these mistakes occur on p. 28, and *dhimmuni* is repeated on p. 148.) In comparing Islam in Morocco and Algeria, Price tells us that "political Islam has taken a much less virulent form in Algeria," but surely he means in Morocco (p. 34).

In the bibliography Price errs in claiming 1986 to be the

date for Shepard's article (it is 1987); reverses the dates of publication of the two works by Piscatori; misattributes Vatin's essay on puritanism and reform in Algerian Islam to Piscatori's 1986 work, Islam in a World of Nation States (Vatin's essay appeared in Piscatori's 1983 edited volume, Islam in the Political Process); and misspells a variety of author names and terms: "Phanatos" (correct: Thanatos), "Ahkavi" (it is Akhavi), "Mahkzen" (it is Makhzen), "Mattadeh" (it is Mottahedeh), "Tahari" (it is Taheri), "Tibbi" (it is Tibi), and so on.

Popular Movements and State Formation in Revolutionary Mexico: The Agraristas and Cristeros of Michoacán. By Jennie Purnell. Durham, NC, and London: Duke University Press, 1999. 271p. \$49.95 cloth, \$17.95 paper.

Neil Harvey, New Mexico State University

Jennie Purnell's detailed historical analysis of rural conflict in Mexico in the 1920s sheds new light on the formation of political identities in the context of revolutionary state formation. The book is well written and provides new material on the comparatively understudied cristero rebellion of 1926-29. Drawing on the experience of three regions of Michoacán state in Mexico's center-west, Purnell traces divergent responses to two main elements of postrevolutionary politics: radical agrarianism and anticlericalism. The author's concern with local histories is prompted by her dissatisfaction with structuralist analyses that tend to assume fixed interests and identities of social agents due to their economic position. Purnell also rejects new social movements theory as an inadequate framework for analysis. In explaining why some peasants supported the goals of agrarian reform and anticlericalism while others took up arms against such policies, Purnell focuses on how historical legacies of local conflict were politicized in new ways during the 1920s as the postrevolutionary elites sought to consolidate national government and reshape cultural identities.

For Purnell, the contours of state power emerged from myriad local and regional histories of negotiation, accommodation, and resistance. In this respect, Purnell clearly locates her work within the recent literature in Mexican studies on everyday forms of state formation (the title of a 1994 volume edited by Gil Joseph and Daniel Nugent, which include essays by James Scott and Derek Sayer). In this sense, Purnell takes aim against both orthodox and revisionist understandings of revolutionary state formation in Mexico. The former tend to view the state as representing the popular will, and the latter claim that the new state was able simply to impose its centralized authority on a passive society. For Purnell, state power is not seen in terms of the successful centralization of authority in the capital city (expressed by the notion of "Leviathan on the Zócalo"). Instead, it is the contingent result of conflictive processes of resistance and negotiation that take place in a great variety of local and regional contexts, where what is at stake is not national power but local understandings of political authority, religious practices, and property relations.

The book is divided into eight chapters. The first lays out Purnell's argument in favor of the nuances of local histories, promising a very descriptive but also analytically perceptive text. Chapters 2, 3, and 4 provide relevant historical contextualization of local resistance to nineteenth-century liberalism and (early) twentieth-century state formation. In this way, Purnell connects longer term processes of identity formation with the cristero rebellion. Her original, empirical research, carried out as part of a doctoral dissertation, is

concentrated in the following three chapters, which compare how rural communities engaged with the state in three subregions of Michoacán. The concluding chapter repeats the book's main arguments and places the study in the context of other antistate movements in early-twentiethcentury Mexico. She also provides an alternative explanation for the sustained support for the cristero rebellion in the center-west region of Mexico. Rejecting structuralist and other analyses, Purnell argues that cristero support was strongest when three elements were present: Communities had survived liberal reforms with a relatively intact land base; a dense network of Catholic organizations existed at the grassroots level; and there were particular experiences of state formation in the 1920s, especially the association of centralized power with state-managed agrarian reform and state-sponsored anticlericalism.

Purnell's study will be particularly welcomed by specialists in Mexican political history. The depth of archival research is impressive, and the book makes a coherent case for more microlevel studies that may challenge existing scholarly and political understandings of postrevolutionary Mexico, particularly the critical formative years of the new state in the 1920s. The book will have a harder time attracting other audiences in political science. The author makes only a limited attempt to locate her work in broader scholarship of interest to comparative politics or political science more generally. Theoretical discussion tends to be limited to debates among historians of the Mexican Revolution, with only passing reference to comparative study of peasant rebellions and state formation. Purnell is too quick to dismiss theories of identity formation that she associates with "new social movements" theory. I presume that she means poststructuralist theories, which can and have been applied to the study of political identities in many historical and geographical settings (e.g., Aletta Norval, Deconstructing Apartheid Discourse, 1996; Ernesto Laclau, ed., The Making of Political Identities, 1994). Postcolonial theories and subaltern studies are not mentioned, despite some close affinities. Even more mainstream scholarship in comparative politics receives little or no attention (e.g., the "state-in-society" approach developed in Joel Migdal et al., State Power and Social Forces, 1994).

The limited discussion of theoretical alternatives is also flawed. For example, Purnell claims that new social movements theory believes that identities are "almost completely divorced from economic structures and political institutions" (p. 182). This cannot be the case, since those movements that have been categorized as "new" (peace, feminist, ecological, ethnic, and so on) are, to my knowledge, conceptualized as responses to transformations in the economic, political, and cultural realms of contemporary societies. That they are not reducible to economic interests or political institutions is not so different from Purnell's own arguments for a more historically contingent and relational understanding of popular mobilization. Similarly, structuralist approaches are presented in their most extreme, deterministic form, which omits the development of more contextually specific approaches to the constraints and opportunities for collective action.

In sum, this book provides more support for the postrevisionist synthesis in the history of revolutionary Mexico. Yet, it does not address either the theoretical and empirical problems with this approach or the broader developments in the study of state formation and popular movements.

Nihil Obstat: Religion, Politics, and Social Changes in East-Central Europe and Russia. By Sabrina P. Ramet. Durham, NC: Duke University Press, 1998. 424p. \$69.95 cloth, \$23.95 paper.

James W. Warhola, University of Maine

Sabrina Ramet has supplied the scholarly community with yet another excellent, meticulously researched work dealing with politics, religion, and social change in the contemporary world. The book details the symbiosis of church-state relations in a remarkably broad scope of religious orientations across a daunting range of national contexts, including Albania, Bulgaria, the Czech Republic, the former East Germany, Hungary, Poland, Romania, Russia, Slovakia, Ukraine, and the former Yugoslavia. Eight chapters devoted more or less to individual country studies compose sections 2, 3, and 4. These are preceded by an opening section, "Cross-Regional Overview in Comparative Perspective," and followed by a concluding section, "Postcommunist Trends and Conclusion." Six of the 14 chapters appeared in print in earlier versions. These were expanded and developed to reflect changes in the countries under consideration and to provide coherence to the book; both objectives were successfully accomplished.

Substantial treatment of church-state relations before, during, and after each country's respective experience with a Marxist-Leninist regime is provided within the framework of the profoundly insightful chapter 2, "Phases in Communist Religious Policy." Much of the analysis is undertaken from the perspective of the "symbiosis of religion and politics" rather than a one-directional dynamic of either the state shaping and determining religious affairs or of one or another religious group affecting the policies and practices of the state. Thus, the work provides a highly useful and necessary addition to the massive body of scholarly literature on church-state relations, much of which either gives short shrift to this symbiotic dynamic or simply neglects it altogether. Also, chapter 12, "Mores Ecclesiae et Potestas Fidei: A Contrast of the Bulgarian Orthodox Church and the Polish Catholic Church," is a model of comparative scholarship on religion and politics at its best.

Documented evidence derived from numerous language sources is adroitly provided from nearly every major language of Eastern Europe as well as Italian and German. Errors of fact, or questionable assertions, are minimal and rather inconsequential. Former Ukrainian President Leonid Kravchuk's name, for example, is inconsistently transliterated (Kravchuk until the second half of p. 257, thereafter Krawchuk); referring to Abkhazia as one of "other countries" (p. 271) seems premature at best, given the status of this region of Georgia at the date of publication; and more than a few historians would surely dispute the claim that the "persistent tendency of Christian religions" to "anathematize homosexuality" has been occurring only "since the twelfth century" (p. 333).

Two characteristics of this work might be viewed as weaknesses but strike me more as implicit invitations to further research needed in the general area of religion, politics, and social change. First, although the work successfully and emphatically underscores the significance of the symbiosis of religion and politics in the governance of a society, the volume represents more a chronicle of the entire region's experience in this regard than a deep, comprehensive consideration of the phenomenon of religion and its place in sociopolitical transformation in the modern world. Perhaps limitations of space precluded such consideration. Yet, one does not need to subscribe fully to modernization theory, for example, to appreciate that the role and place of

religion in the symbiosis of church-state relations has undergone dramatic transformations in literate as opposed to preliterate societies, or in societies characterized by the very social infrastructures that both enabled political totalitarianism and ultimately undermined it (e.g., modern mass communications and transportation). One also might argue that the symbiosis of religion and politics characteristically undergoes profound transformation in societies in which the level of industrial development has dramatically altered patterns of livelihood and consumption, thereby casting such questions as property, wealth, status, and social responsibility of economic powerholders—all of which have clear and direct religious implications—into bold political-theoretical relief. Aside from largely coincidental treatment of this aspect of the symbiosis of state and religion in the otherwise excellent and incisive chapter 2 and in the all-too-brief chapter 14, the volume leaves to future researchers the further exploration of the connection between modernization and change in church-state relations since the collapse of the communist regimes in the region.

Second, the philosophical questions addressed by the author in the concluding chapter are useful and necessary. Given their gravity, however, the work would have benefited from a much more lengthy exploration than that offered. That which Dr. James Wood, founder of the Journal of Church and State, refers to simply as "the existential reality of religion" has the most far-reaching and profound social and political implications, and Ramet does well to address these in chapter 14 in terms of the irreducibly political questions of social identity, public morality, and human purpose. The scope and depth of treatment, however, does not match the sharp and deep insight into the regional chronicle of religion and politics offered in this volume. Despite the constraints of space in what is already a very substantial volume, one finishes this book with the sense that the final chapter deserved more than the profound but scant six pages allotted, particularly given the author's demonstrated acumen.

It has been suggested that the communist regimes in Eastern Europe and the former Soviet Union ultimately found themselves at war with all three transcendental values: truth, beauty, and goodness. As such, the title Nihil Obstat ("nothing stands in the way") might have been offered as testament to the social dimension of "the existential reality of religion," even in the face of explicit and powerful attempts to eradicate it, and not just to indicate that now, after the collapse of these regimes, "nothing stands in the way" of the vitality of religion and its role in public life (p. 3).

Nihil Obstat is a remarkable scholarly work of encyclopedic scope, from the former East Germany, to the curious case of Albanian society's religious tolerance (p. 220), which contrasts so sharply with the fanatically antireligious communist regime of 1945–89; to postsoviet Russia and Ukraine, whose highly complex and troubled configuration of religious forces is handled deftly and evenhandedly. This book is essential reading for those studying the interaction of politics and religion in the contemporary world, and it appears destined to shape the contours of subsequent research on politics and religion in this region

Deepening Democracy? The Modern Left and Social Movements in Chile and Peru. By Kenneth M. Roberts. Stanford, CA: Stanford University Press, 1998. 370p. \$55.00 cloth, \$19.95 paper.

Gerardo L. Munck, University of Illinois at Urbana-Champaign The Left in Latin America has changed in many ways from the days of Che Guevara and Salvador Allende. Its revolutionary aspirations were dashed preemptively by repressive military regimes that swept the region in the 1960s and 1970s and were short lived in Nicaragua. Even in Cuba, where Castro remains in power, the retreat from the ideals of the 1960s is remarkable. In short, today even the old choices between capitalism or socialism, dependency or revolution, have lost much of their force as points of reference in the definition and formation of the Left.

In the context of democracy and neoliberalism in the 1990s, the re-formation of the Left hinges on a much more complex choice. Defining the Left as those who seek an alternative to neoliberalism is easy enough. But, as Roberts argues with clarity and force, the problem is that those seeking an alternative to neoliberalism face a tough decision: whether to search for an alternative to neoliberalism within or outside the institutional framework of democratic regimes. which highlight the electoral process above all else and which have perpetuated the antipopular demobilization strategy of the previous military regimes. In other words, the current context presents the following dilemma for the Left: The support of democracy, which in the wake of repressive regimes has become more valued, may actually forestall the constitution and action of the very actors who would have a stake in an alternative to neoliberalism.

The effect of this choice on the evolution of the Left has been profound. Initially, different responses to it led to a split between moderate and orthodox tendencies in countries such as Brazil, Chile, El Salvador, Nicaragua, and Peru. Thereafter, the political successes of the moderate Left strengthened the reformist tendencies within the Left. Thus, by the late 1990s, the process of formation of a democratic Left was well under way. But inasmuch as the democracies the Left was now supporting had been made safe for elites, the Left had become more democratic than leftist. Ironically, it had not been fully committed to democracy in the 1960s, at a time when the existing democracies were susceptible to popular mobilization, but in the 1990s was a key supporter of democracies that were overwhelmingly conservative.

This problematique, of vast implications for the future of Latin America, is at the heart of this very insightful book. It starts by laying the groundwork for the empirical analysis. The Introduction and the first two chapters discuss the context within which the recent evolution of the Left in Latin America should be understood. These chapters then present a set of explanatory variables focused on two questions: why the Left responded to the choices it faced as it did and why the demobilization of the Left has been so pervasive. The next five chapters advance the analysis through a study of two cases: Chile and Peru. These chapters are structured both in terms of chronological time and the book's key questions.

The choice of cases has a solid rationale. These two cases not only are of great intrinsic interest but also, as Roberts shows, are useful in thinking about Latin America more broadly because they illustrate the two most clear-cut responses to the choice faced by the Left throughout the region. The Peruvian case demonstrates how the failure to adapt to the new institutions of democracy can lead to obsolescence. In this case, although the United Left looked like a formidable electoral force in the mid-1980s, the failure of this coalition to agree to give primacy to an electoral strategy, along with the Shinning Path's continued pursuit of change via armed action, led to the utter collapse of the Left as a viable force.

In contrast, Chile demonstrates the implications of the choice to accept and defend democracy. It shows how leftists willing to undergo a process of moderation—the socialists—

can grow in strength, take advantage of the growing marginalization of leftists who refuse to accept the institutional framework of democracy (the communists), and even accede to power, as did the socialists, who have been a main bloc of Concertation, the governing coalition since 1990. But, as Roberts argues, if the contrast between Peru and Chile shows that the path of moderation seems to be the only one that leads to the successful re-formation of the Left, then the case of Chile also shows how success via moderation has its own costs. Indeed, being part of a governing coalition has done little to allow the socialists to steer policies toward some alternative to neoliberalism. (At this time-July 1999-we can only wonder whether Lagos, the leader of socialists and presidential candidate of Concertation will win and, if so, whether a Lagos administration will lead to a different assessment of this question.) Thus, these two cases do much to capture the dilemma currently faced by the Left in Latin America.

Some critical points might be raised in assessing this book. For example, the analysis of the cases could have been more systematically driven by the explanatory factors presented at the outset. Along these lines, although Peru and Chile are compared sometimes, the separate presentation of the cases makes the analysis less explicitly comparative than it could be. Moreover, a discussion about how the analysis could be extended to other cases would have strengthened the argument. But these points should not detract from the book's achievements. Roberts not only brings to bear on his analysis a clearly formulated set of explanatory factors but also advances a very complex and compelling argument. Indeed, because he relies on data that are very detailed and hard to collect, in particular about actors and their strategies, he does a masterful job of articulating nuanced causal arguments that weave together structural, institutional, and choice-based factors.

In sum, this book makes an important contribution. It offers a broad picture of how Latin America is changing and, specifically, how masses and political parties not only are responding to but also shaping a new social and political order. It is a worthy addition to a growing body of literature on Latin American social movements and politics, and it should be read and studied not only by those who are concerned with the evolution of the Left but also more broadly by those who are interested in understanding the travails of current Latin American democracies.

Building Social Capital in Thailand: Fibers, Finance, and Infrastructure. By Danny Unger. New York: Cambridge University Press, 1998. 227p. \$59.95 cloth, \$19.95 paper.

Ansil Ramsay, St. Lawrence University

From the late 1950s until the late 1990s Thailand had one of the best records of economic growth among all less-developed countries. Danny Unger uses a social capital approach to explain why Thai political leaders adopted the market-oriented development strategy that initiated the period of growth, and why ethnic Chinese entrepreneurs were so effective in taking advantage of the strategy. His analysis begins with the assertion that social capital, defined as cooperative groups "within which individuals can reach compromises and foster shared understandings of common problems," is very unequally distributed between ethnic Thais and the ethnic Chinese minority (p. 2). Thais tend to have a low level of social capital, and Chinese tend to have a high level.

From this beginning Unger moves to an explanation of how low levels of social capital among Thais contributed in significant ways to the adoption of market-oriented development strategies in the late 1950s. They did so in part because lack of social capital weakened state officials' capacity to work effectively with groups outside the state to promote economic development. This made it easier to rely on market-oriented strategies that did not require close cooperation between state officials and representatives from the private sector (p. 10). In addition, the inability of Thai officials to cooperate led to intense competition among political leaders and the need to find resources to fund this competition. One major source was the Chinese business community, which was supportive of market-oriented policies. Finally, the new leadership that came to power in the late 1950s was able to adopt such policies because of the absence of distributional coalitions capable of blocking the shift.

The new development strategy brought quick results because of the vigorous response of ethnic Chinese entrepreneurs to market incentives. This response was more the result of their "capacities for cooperative undertakings" than any innate capacity for thrift, hard work, or patience (p. 51). The ability to cooperate enabled them to overcome market failures and collective action problems that stemmed, in part, from the ineptness of the Thai state. In short, Thailand's successful economic performance was the result of "a serendipitous match of Thai and Chinese social capital endowments" (p. 175). The first three chapters of the book are devoted to developing this argument and sketching out the main features of the Thai political economy.

That and Chinese social capital endowments help explain the overall pattern of That growth, but there has been considerable variation in patterns of interaction among state agencies and private firms in different economic sectors. The middle three chapters of the book present a sophisticated comparative analysis of patterns of interaction in three economic sectors: finance, textiles, and infrastructure for heavy industry and transport. Unger suggests that four structural variables account for the variation: "the number of firms and state agencies involved, the number of times and length of time over which firms and agencies meet in order to cooperate, the degree to which they can share among themselves the gains they achieve, and the number of times such gains recur" (p. 7).

Unger does not devote a great deal of space to the economic crisis of 1997 and its aftermath, but he does discuss the ways in which the lack of cooperation between public and private agencies contributed to growing economic and financial difficulties in the 1990s. The author finds evidence, however, that cooperation began to improve in the 1990s, "both in the private sector and in the broader civil society," and that Thai officials began to be more effective in cooperating with the private sector (pp. 177–9).

This is an important work that has the additional merit of being written with great clarity. Much has been written about rapid economic growth in Thailand as well as other Asian countries. This literature is dominated by political economy explanations. Unger offers an alternative. He does not claim to have discovered a Rosetta Stone that completely explains Thai success, and he draws on literature on collective action, state embeddedness, and institutions as well as on literature on social capital. He argues, however, that the social capital approach provides "new ways to understand the Thai economic record" (p. 5).

Unger makes a strong case for this claim, but skeptical readers are likely to challenge him in at least three ways. First, how much does the social capital approach add to our understanding of Thai development success and difficulties? One of the author's main claims is that the approach adds to our knowledge about why Thai leaders adopted a market-

oriented strategy in the late 1950s, but he points out that a number of factors help explain their choice. These include the failure of past state-led policies, the desire of Field Marshal Sant Thanarat to undermine opponents whose bases of power were in state enterprises, and pressures from the United States. Unger uses the social capital approach to suggest that, in addition to these reasons. Thai leaders were able to make the transition to a new market strategy because Thailand's legacy of social capital left it without organized groups capable of resisting the change. Yet, it is possible to explain the absence of such opposition without using the concept of social capital. There was little industry in Thailand in the 1950s, either private or state owned, and therefore no large or strong base among industrialists to oppose liberalizing policies. Trade unions were small and easily suppressed by military governments. The ethnic Chinese minority who dominated trade and finance were very vulnerable politically.

Skeptics will also find it possible to challenge the assertion that market-oriented policies worked so well because the rich accumulation of social capital among ethnic Chinese enabled them to overcome various market failures and collective action problems. To make this argument fully convincing would require not only a systematic account of the kinds of market failures and collective action problems that were the greatest threats to sustained economic development but also specific analyses of how the ethnic Chinese drew upon their social capital to overcome them. The book does not have such an account.

Finally, one can challenge such blunt assertions as "Thats did not readily participate in continuing cooperative groups beyond the family" (p. 168). There are clear exceptions to such assertions in both the state and private sector, where Thats have demonstrated high levels of cooperative behavior over long periods. The Bank of Thailand is an example from the state sector, and Siam Cement is an example from the private sector.

These challenges do not diminish the significance of the book or the author's accomplishment. They do point to areas in which future researchers, using the social capital approach to explain development in Thailand and elsewhere in Asia, can build upon Unger's achievements.

Institutions and Institutional Change in China: Premodernity and Modernization. By Fei-Ling Wang. New York: St. Martin's, 1998. 227p. \$65.00.

Peter R. Moody, Jr., University of Notre Dame

For the past generation, after the rejection of Maoism, the Chinese leadership directed its policy to the "four modernizations." Fei-ling Wang, however, comments on how "barren in terms of theoretical construction" (p. 5) this discussion of modernization has been, whether by the Chinese authorities themselves or by those who study and interpret them. Wang is dissatisfied with even the more theoretical political science studies of modernization and hopes his work will offer an improvement.

He has his own definition of modernization: "the institutional changes toward the optimization of the human institution with a particular form of the human institution—modernity—as its identifiable goal." This is less tautological than it might appear on quick reading. Modernization involves changes in institutions, and Wang, unlike most theorists, has an explicit picture of what modernity might mean as a goal. Modernization is the process of institutional change leading toward modernity. The puzzling term "the [the!] human institution" seems to refer to the ways human beings

live with one another, and modernity apparently is the best (optimal) form this human institution can take. The human institution is itself a compound of three institutions: the polity, the economy, and society. If this is supposed to embrace all of human life it may omit several important dimensions—the aesthetic, the moral, the spiritual, the realm of personal attachments and affections—but perhaps these are not particularly relevant to achieving modernity. Modernity entails democracy (an effective, stable, and participatory "nation-state"—this last term might be somewhat problematic), a market economy, and a civil society (pp. 7-20). Wang is not very interested in the modish notion of postmodernity. This is either a further development along the lines of modernity or a deterioration from it. Wang says that modernization is not the same thing as development and that modernity is compatible with any level of development. One might think, however, that the modern society Wang has in mind will turn out to require a rather advanced technological base.

Wang proposes to study modernization through changes in what he calls labor allocation patterns (LAPs). China is an instance of "the [the!] traditional LAP" (p. 62). (Wang's analysis is rather more nuanced than this trick of style implies, and his promiscuous use of the definite article does not mean that everything within a certain category is exactly like everything else.) China has been a "superstable" society with a family-based LAP and an authoritarian state dominant over society. Communism did not break this pattern but reinforced it. It is only recently, with the liberal economic reforms, that China has begun to modernize, particularly with the evolution of a market-rational labor system, at least at the local level, what Wang calls "community based labor markets" (CLMs) (p. 68). Those whose sense of humor ceased development in the seventh grade might suggest an alternatrve abbreviation, CLAPs, in homage to the prominent role of "sex workers" in China's emerging market economy.

Wang's characterization of China's society is probably not "wrong," in that everything in the world is in some sense like everything else, and there are significant continuities between "traditional" and Maoist China and differences between these and the period since 1978 or 1992. His model is particularly helpful in understanding the current transformations It probably cannot serve as an accurate description of Chinese society throughout the ages, not so much for what it asserts but for what it omits. It ignores the role of commerce and the market at many different periods of Chinese history. It does not acknowledge that many of the current developments were under way during the republican era. It contains no means for easy accommodation with contradictory, equally persuasive conceptualizations of Chinese reality. A generation ago, for example, it was a scholarly commonplace that communist China marked the definitive modernizing break from the past, and that those who saw continuities between Mao's China and old China were atheoretical smological windbags who probably should confine their attention to the classification of Ming vases. The results of much of the social change brought by reform look more like "traditional" China than China under Mao.

Another cut at the Chinese example may indicate that a family-based LAP may be compatible with modernity, and the focus on LAPs may leave important things unexplained. The old system of collective agriculture was rather explicitly designed, for example, to weaken the family-based LAP, in that workpoints were awarded to individuals, not to households. But the system seems to have done little to weaken the hold of the family over the individual (or, to phrase it another way, to damage family cohesion). Wang's introductory discussion of the CLM exaggerates the degree of market

rationality—although elsewhere he gives ample attention to the pathologies generated by the reform, including, perhaps surprisingly in a society ruled by a communist party, the evolution for the first time in China of a genuine proletariat on the nineteenth-century Marxist pattern.

The analytic model is itself interesting, even stimulating, although one can get the impression its logic has not been fully thought out. It seems to be a version of the old standard modernization theory, early David Apter, with a touch of Huntington. Wang says that institutions exist to satisfy human needs, with desires arising from needs. He also says, again, that modernity is a "goal." To call modernity a goal may imply there is nothing in the operation of social forces (or whatever) that spontaneously leads to modernity. But neither, it seems, should modernity as here conceived necessarily fit anyone's particular need. The Chinese rulers, presumably, would be completely happy with a prosperous economy, a closed political system, and a cowed society. Wang stresses (p. 33) that the modernization of the economy, polity, and society need not move in harmony with one another. He conjectures (without offering evidence) that America's demoralizing social problems come from excessive development of the market at the expense of political and social health, and he points to problems marketization may be causing Chinese society. This is not to say that Wang's notion of modernity does not have its appeal, but his theory is more normative than positive.

Wang purports to treat China as a natural social science laboratory, but there is evidence that he cares about the country for its own sake. His picture—communism as a continuation of a mostly dark tradition, with the hopeful and imperfect break coming only with the post-Mao reforms—is similar to that of the more radical reformers, with a hint of neoconservatism thrown in (Wang worries that democracy might weaken the capacity of the state). His thesis is attractive as a policy guide and generally compelling as an analysis of China's problems and prospects, but it works better as a contribution to political thought than to political science.

Unstately Power, Vol. I: Local Causes of China's Economic Reforms. By Lynn T. White III. Armonk, NY: M.E. Sharpe, 1998. 521p. \$69.00.

Unstately Power, Vol. II: Local Causes of China's Intellectual, Legal, and Governmental Reforms. By Lynn T. White III. Armonk, NY: M.E. Sharpe, 1998. 765p. \$45.00.

Joseph Fewsmith, Boston University

In two thick volumes, Lynn White explores the origins and implications of China's reforms across a wide spectrum of issues, encompassing everything from the economics of rural reform and the growth of local business; to the changing ideational complex of ideology, religion, the media, and the arts; through the changing role of the legal system and the selection of the political leadership. Unsatisfied with narrowly gauged explanations, White explicates the dynamic of reform through multiple overlapping and/or reinforcing arenas of social life. It is an extremely ambitious undertaking, reflecting prodigious scholarship and wide-ranging interests. White adopts such an ambitious research strategy because of his desire to overturn what he believes to be deeply engrained understandings about the hierarchical, top-down nature of the Chinese state and its relation to society.

The author sets out two broad but important research goals. First, he wants to break down the conventional (in China and the West) dating of reform from the Third Plenum in December 1978. For White, that was not an especially

important date; it was "less situational than normative" (vol. I, p. 12). It marked the rise of a new group of leaders at the center and hence a new historiography but not much else. White argues that China's reforms began when the Cultural Revolution ended, 1969 in his reckoning, or shortly thereafter. His emphasis on the emergence of reform in the early 1970s is closely related to his other major thesis, namely, that reforms have been a bottom-up process. Local networks—unstately power—have been the source of the reforms and of their continuation, and it is the rise of these local interests that is the real story of China's reforms.

White's emphasis on local networks and local interests builds on the same intellectual orientation that James Scott developed in Weapons of the Weak (1985) and Domination and the Arts of Resistance (1990). Indeed, White argues that "few" of China's reforms "served the long term interests of the central Party elite in Beijing," on the contrary, "the government over time tolerated and then legitimated such changes, because they could not be stopped" (vol. I, p. 15). In arguing this, White takes on what he perceives to be the dominant focus on Beijing and the leaders at the top.

This multitude of interests, which in the aggregate White believes have proved themselves more powerful than the Leninist party that rules them, makes up the local power in all the various dimensions that interest White. In calling these interests "unstately," White goes well beyond most students of China. There has been much interest in the field in recent years about the relations between central and local levels of government. White simply excludes the latter from the realm of the state ("The state is the central government elite whose glue holds" [vol. I, p. 19]), lumping, say, the City of Shanghai party and government organizations in with intellectuals, artists, business interests, and so forth. This tendency to conflate local political organizations, which do have responsibilities vis-à-vis higher levels of authority, with other and more informal interest networks captures one facet of political action (i.e., the way in which local political authority has to respond to, mollify, or control local interests), but it ignores the way they must respond to the central government as well.

Perhaps because White is fascinated by local networks and the interaction of many disparate variables to produce an outcome—growing local power—he never explores in systematic fashion the incentive structure that has increased the importance of local government or the differential effects of these incentives on different local interests. Although the author discusses briefly the 1979-80 tax reforms that gave localities incentives to develop their economies, and hence to support local interest, he does not exploit this insight in the systematic fashion of Jean Oi (Rural China Takes Off, 1999). Perhaps this is because White rejects the sort of rational choice approach Oi employs, but it seems that White's approach is driven more by his own ecumenical interests than by methods per se. Causality, in his opinion, is too complex to be reduced to incentive structures. That may be true, but one would hope for a little more guidance through the thicket of local interests explored here.

The relationship between central power and local interests in China has received much attention in recent years. White's emphasis on the importance of local interests puts him well within the mainstream of opinion. Indeed, it is difficult to understand the author's repeated criticism of those who look at central politics in Beijing. That fight has been won; the vast majority of scholarship today, as indicated by White's own bibliography, directs its attention away from elite politics (in this regard, the present reviewer is an exception). But in adopting the thesis that China's reforms have been driven

from the bottom up, White goes beyond what most people would accept and, I think, what the evidence will allow.

The classic example, which White discusses, is the origins of agricultural reforms (the so-called household responsibility system). White argues that this was entirely a bottom-up process. "Decollectivization," he argues, "was not an idea from Beijing; it came from local farmers" (vol. I, p. 96). This oversimplifies the reality. There was, of course, a basis of support for decollectivization among the rural population, just as there had been every year since collectivization was forced on them (see Edward Friedman, Paul Pickowicz, and Mark Selden, Chinese Village, Socialist State, 1991). But the change came in 1978 because some determined local leaders, most notably Wan Li, were courageous enough to push the change against both local and central resistance (which is not to say that Wan Li did not have significant local and central support; he was a close ally of Deng Xiaoping, and that relationship was critical to what he did in Anhui between 1977 and 1979).

This example suggests that central politics remains an important force in China, shaping (not determining) what happens at local levels. Indeed, in pushing the origin of the reforms back into the 1970s, White ignores the importance of what was going on at the center. One of the most important political changes in that period, to which White alludes in his references to the emergence of reforms in the early 1970s, was the death of Lin Biao after an apparent failed attempt to assassinate Mao Zedong. Lin's death, and the explanation provided to the Chinese people, set off shock waves across the nation. Mao brought Deng Xiaoping (purged in the Cultural Revolution as the second major capitalist proponent in the party after Liu Shaoqi) back into the leadership to stabilize the military and provide assistance to Premier Zhou Enlai, then suffering from cancer. Deng fought hard in those years to begin to restore rationality to the economic system, generating some of the reforms White discusses. Deng paid for his efforts by being purged again in 1976. The death of Mao and the arrest of the Gang of Four, which permitted policy adjustments under Hua Guofeng and paved the way for Deng's reemergence, were not trivial events.

More important than the origins of the reforms is their effect, particularly what they have meant for the central-local relationship. Despite the prodigious research and rich insights, White ends up asserting, rather than demonstrating, that the reforms have meant the decentralization of power. It is unfortunate that Yasheng Huang's powerful argument (Inflation and Investment Control in China, 1996)—that the central state has retained political power even as it has decentralized economic power—apparently came out too late to be taken into account. It goes right to the heart of the matter of how much power of what sort has in fact devolved to the localities. And Huang's answer is diametrically opposite that of White.

Moreover, White tends to focus so much on the local level that he ends up ignoring the central government. He misses an important, and still very much unexplored, story of how the reforms have reshaped both the localities and the central state through their mutual interaction and adjustment. In fact, the central state has acted to counter many of the trends White discusses, most notably in the tax reform of 1994. The erosion of state capacity in the 1980s has generated efforts to reverse those trends in the 1990s, and how successfully remains to be seen.

Many other topics are explored in these volumes, too many to discuss in a short review. Suffice it to say that White has engaged just about every significant debate in contemporary China studies. Such a work is bound to be controversial, as the comments above suggest, but by engaging such broad-ranging, important issues, White has done a real service to the field.

Peasant Revolution in Ethiopia: The Tigray People's Liberation Front, 1975–1991. By John Young. Cambridge: Cambridge University Press, 1997. 270p. \$59.95.

Ruth Iyob, University of Missouri-St. Louis

The author offers a much needed account of an ethnonational liberation movement that dislodged Ethiopia's Afro-Marxist regime after a seventeen-year guerrilla war (1975–91) and received international recognition and acclaim for its pragmatic and innovative leadership. John Young's focus on the Tigray People's Liberation Front (TPLF), which dominates the coalition government Ethiopian Peoples Revolutionary Democratic Front (EPRDF), provides the first scholarly attempt to analyze Ethiopia's second "revolution" of the twentieth century.

The author's thesis can be summed up as follows: The findings of major theorists of peasant revolutions minimize the role of the peasantry when the leaders are from the urban petit bourgeoisie. As a result, their theories do not accurately reflect the subtle ways in which peasantry's demands are addressed by the leadership and the myriad ways in which unaddressed or postponed demands permeate the postrevolutionary period. The book is divided into eight chapters. The research is based on field work and interviews collected over more than thirteen months. The title reflects the major difficulty of this work: a focus on critiquing theorists of peasant studies superimposed on Tigray's ethnonationalist movement led by the urban intelligentsia of the TPLF.

This cumbersome design forces the reader to maintain bifocal lenses, one aimed at the conditions of Tigray's peasants and the other on the origin, development, and policies of the TPLF. The author succeeds in bringing to light little known facts about the TPLF, but the not so well-defined peasantry recede to the background. Young glosses over the ethnographic grounding upon which the works of Eric Wolf, Joel Migdal, Jeffrey Paige, and James Scott rest, thus discounting the rich database that validates the inter- and intraclass definitions of categories. His reliance on the ideology-based categories in use since Ethiopia's first experiment with agrarian reform—subsequently adopted by the current regime—leads him to assert that extant theories of peasant revolution are "largely inapplicable to the agrarian social structure in Tigray" (p. 20). He supports this assertion by his patently erroneous assessment that "wealth was normally defined in terms of possession of capital, largely in the form of oxen and other animals, and not land" (p. 20). He then posits that regime stimulus for revolt and nationalism deserve greater attention as mobilizing factors in enlisting peasant support for objectives beyond local/provincial concerns.

The first two chapters provide the theoretical and historical background. The third chapter brings to life the conditions in rural and urban Tigray in the 1970s that led to armed resistance. The TPLF's leadership, "drawn disproportionately from the educated sons of the rich peasantry and the lower-middle local nobility," used real and perceived grievances of Amhara domination to unite the different ideological camps of monarchists, republicans, religious officials, and Marxist-Lennists to rebel against the Addis Ababa regime (p. 85). Chapter 4 is a political saga of the TPLF during 1975–78, replete with accounts of bank robberies in distant borderlands, bandits fighting for a nobility under siege, gruesome tales of betrayals and executions, and alliance-building with other armed political groups, at the end of

which the TPLF emerges as a winner. In the next chapter the author deals with the making and breaking of the TPLF and Eritrean People's Liberation Front alliance, the politics of famine, internal power struggles, and the emergence of a clandestine vanguard party known as the Marxist-Leninist League of Tigray (MLLT).

One is sympathetic to Young's difficulty in overcoming official "reticence" regarding dissidence. Tracing and documenting the shadowy existence of the MLLT and identifying town and regional grievances of discrimination are noteworthy achievements. It is to his credit that as an external analyst and a relative newcomer to the study of Ethiopian politics, Young was able to discern the underlying contradictions present in the discourse and praxis of the organization. Unfortunately, his persistent efforts to center the peasantry in this study of the TPLF diminish the theoretical illuminations that otherwise would logically emerge from this fascinating study of ethionationalism. Nevertheless, the author succeeds in documenting this crucial period during which the ideological fault lines of post-1991 Ethiopia first appeared.

In chapter 7, Young analyzes TPLF policies of recruitment and retention, which used cultural symbols and political education that embraced both traditional and radical elements of the society. The TPLF represented itself as a defender of the faith to adherents of both Christianity and Islam. Radical reforms, such as the much touted liberation of women, clashed with both religious and patriarchal institutions and resulted in the postponement of concrete reforms (pp. 178–81). The discordance between image and reality documented by the author is in stark contrast to the pervasive

fiction of gender equality and "open dissent" (p. 140). Young's account of the TPLF strategy of mobilization indicates a number of compromises and postponed promises. Key among these is the leadership's responsibility to kith and kin in the region, which, in the eyes of the peasantry, supercedes any responsibilities the TPLF leadership may bear to their multiethnic constituencies. The author questions the lack of transparency of the TPLF-EPRDF regime and its contradictory discourse(s), but he too readily succumbs to facile arguments that "Abyssinian culture typically endorses secretive, evasive, and distrustful behaviour, and sees openness as akin to innocence and simplicity" (p. 211).

Young has traveled fruitfully through very difficult terrain, but he is outmatched by a leadership well versed in the art of dissimulation. Overall, he must be commended for the completion of a pioneering work and for his effort to present a rigorous study of one of Africa's most ancient and closed societies. This work raises important questions about the prospects of Ethiopia's "cultural aristocrats," whose harshness is camouflaged by the Maoist-Marxist-Leninist rhetoric and whose ethnonationalist discourse has lost its moorings. The existence of a political culture of secrecy and the absence of requisite ethnographic skills in the extraction of verifiable knowledge present any outsider, however diligent, with lofty barriers. Notwithstanding all these difficulties, this book is a valuable reference. It is a fascinating study that points to the need for urgent attention to the problems of cultural translation as comparativists engage in the pursuit of knowledge about nationalism, revolutions, and the rapidly disappearing peasantry class.

International Relations

Institutional Designs for a Complex World: Bargaining, Linkages, and Nesting. Edited by Vinod K. Aggarwal. Ithaca, NY: Cornell University Press, 1998. 224p. \$65.00.

Oran R. Young, Dartmouth College

This collection of essays is a welcome addition to the small but growing literature on institutional interplay, that is, interactions between or among distinct institutions operating in international society. The analytic focus is on linkages that may be described as horizontal, in the sense that the interactions considered occur at the same social scale (i.e., within international society), and as political, in the sense that the cases examined involve conscious efforts to work out appropriate relationships among distinct institutions. The primary substantive focus is on arrangements dealing with economic activities, although one of the case studies extends the argument to the realm of security affairs. The book does not cover the full range of issues associated with the idea of institutional interplay, but the portion it does cover is obviously important and properly subject to careful scrutiny as a subset of the broader domain of institutional interplay.

The book directs attention to situations in which crises or mounting pressures call into question existing institutional arrangements. In such situations, institutional responses are likely to take the form of modifying existing arrangements to meet the new conditions. Those responsible for handling such institutional adjustments must reconcile the new institutional forms with the previous arrangements. The central argument is that reconciliation can take one of two forms: nesting, in the sense that the new arrangements are linked in

some hierarchical way with previous arrangements, or a division of labor, in which distinct institutions handle separate tasks. The choice of strategy and the consequences arising from such choices are determined by what Vinod Aggarwal, in his theoretical chapter, calls institutional bargaining games. Understanding institutional interplay, then, comes down to an effort to grasp the nature of institutional bargaining and the forces that determine outcomes arising from institutional bargaining games in specific situations.

The detailed case studies presented in the various substantive chapters offer a selection of empirical examples that is less than ideal for an analysis of institutional bargaining games. In a study of the 1992-93 crisis afflicting the European Monetary System (EMS) in the aftermath of German reunification, Steven Weber argues that the existing arrangement actually survived with relatively modest alterations due in considerable measure to the fact that the EMS is nested into the larger framework of the European Union (EU). Focusing on the Bosnian crisis, Beverly Crawford analyzes efforts to work out an appropriate division of labor among organizations—the WEU (Western European Union), OSCE (Organization for Security and Co-operation in Europe), NATO (North Atlantic Treaty Organization), and the UN (United Nations)—interested in keeping or making peace in southeastern Europe. She concludes that "a vague, institutional division of labor" (p. 112) was emerging by the time of the 1995 Dayton Agreement, although it is hard to see how this helped Bosnia or provided a basis for handling subsequent crises in the region, such as the 1999 confrontation over Kosovo. Cedric Dupont considers the effort to nest the EU and EFTA (European Free Trade Association) into a broader arrangement known as the European Economic Area (EEA), and Benjamin Cohen looks at the effort to nest the Financial Support Fund (FSF) into the Organization for Economic Cooperation and Development (OECD) in the wake of the 1973–74 oil shock. On the evidence presented in the case studies, both initiatives ended in failure.

Even so, the case studies provide a rich vein of evidence relating to institutional bargaining in situations in which it is important to reconcile new or substantially revised arrangements with existing institutions. Some important insights surface from a reading of the detailed accounts. It is essential to consider the various stages of the bargaining process. An arrangement like the FSF, which passes the negotiation stage with flying colors, may founder at the stage of ratification and implementation. Initiatives launched in one arena may be overtaken by events occurring in other settings. The decision of the key members of EFTA to apply for membership in the EU effectively destroyed the rationale underlying the EEA. Links to broader institutions may help a specific arrangement meet severe challenges without any basic restructuring. It probably would have been a mistake for key players to initiate any radical changes in the EMS as a response to the exchange rate crisis of 1992-93, even in the wake of such drastic steps as the withdrawal of the British pound and the Italian lira from the system.

This collection is not beyond criticism on methodological grounds. The "hypotheses" dealing with various facets of institutional bargaining games (e.g., the choice between the strategies of nesting and the pursuit of a division of labor, the selection of a multilateral or a bilateral approach) are not stated with sufficient specificity to make them easy to explore in the cases (pp. 23–7). As is often the case in works of this sort, the selection of cases for in-depth study seems to be based on ad hoc considerations. It is hard to compare and contrast the conclusions to be drawn from the four individual case studies regarding the "hypotheses" about institutional bargaining. There is no way to judge the extent to which the findings of this project are generalizable to other issue areas.

Nonetheless, these shortcomings should not be allowed to obscure the real contributions of this collection. Institutional interplay is an increasingly important but poorly understood phenomenon at the international level. Institutional bargaining games clearly loom large in any consideration of political linkages in international society. In thinking about the relative importance of power, interests, and knowledge as determinants of institutional bargaining, it is surely correct to conclude that "one size does not fit all with respect to explaining the process of institutional change" (p. 206).

From Motherhood to Citizenship: Women's Rights and International Organizations. By Nitza Berkovitch. Baltimore, MD: Johns Hopkins University Press, 1999. 224p. \$34.95.

Elisabeth J. Friedman, Barnard College

One of the more interesting puzzles in political science is why nations choose to undertake similar reforms—whether in legislation or institutional design—at roughly the same time. This becomes particularly intriguing when the reforms address historically controversial issues, such as the equal rights of women. Nitza Berkovitch takes on the challenge of explaining why it is that "in such different societies... women were granted the same kinds of rights, at approximately the same period of time" (p. 5). In a study that spans 1875 to 1985, she answers that the global cultural and political system

of international institutions and associations, particularly its discourses on women's rights, has been primarily responsible. She presents new information that refutes nation-centric explanations of systemic political change and adds a significant work to the literature on international women's rights. This book would be useful for analysts of international organizations, national legislative reform, and women's movements.

Berkovitch's argument is based in large part in the "world polity" literature from sociological institutional theory. This literature finds that concurrent state action on particular issues cannot be explained by examining the characteristics and processes of individual nations. Instead, it conceives of nations as subparts of a "cultural-political" global polity, a Westernized, rationalized, "highly institutionalized but stateless structure" (p. 9). Particularly in the post–World War II era, its central players have become international actors who produce global discourses that influence the models of organization used by individual nations.

Because of her focus on the effect of global discourses on issues of women's rights, Berkovitch also draws on feminist theories of citizenship. Countering the arguments of other institutionalists, she holds that the extension of citizenship to women cannot be seen as part of a general extension of the right to participate in public institutions to marginalized groups. Instead, women's incorporation has been fundamentally marked by their historic association with the private sphere of domestic responsibilities. Thus, women's citizenship has often been restricted in ways that reflect societal assumptions about their duties as homemakers, specifically as mothers. As these assumptions have broadened, however, equality, not just motherhood, has become a legitimate justification for women's rights.

Following chapter 1, which is an introduction to the theory and cases used, the book is organized chronologically to trace the effect of a consolidating world polity on women's rights. It focuses particularly on workplace rights and how these have changed with respect to changing discourses on womanhood. Chapter 2 analyzes international campaigns carried out by "transnational social reformers" from the mid-1800s to World War I: promoting suffrage, preventing prostitution and trafficking in women, and institutionalizing protective labor legislation. Relying on the more general reform vision of this historical period, the campaigns all conceived of women as needing protection or offering a distinctively "female" perspective on social change.

Chapter 3 examines the interwar period and covers developments in the issues of trafficking and labor legislation as overseen by two new important actors in the world polity: the League of Nations and the International Labour Organization (ILO). Although they promoted regulation of these issues at both international and national levels, their concern not to meddle with the internal business of states—including their control over gender relations—resulted in the continued emphasis on the protection of mothers rather than the empowerment of individual female citizens. At the same time, the international women's peace movement is shown to have promoted a powerful claim to a universal maternalism that demanded protection for all sons.

Chapter 4 addresses the more "consolidated" period of the world polity following World War II, with the simultaneous expansion of the nation-state system and extension of supranational institutions and organizations heavily influenced by the international human rights regime. In particular, the newly established UN Commission on the Status of Women and the ILO had a salutary effect on the spread of women's workplace rights across the globe. In a significant shift, these

were granted nationally not as protection for mothers but as rights of equal opportunity for female citizens.

By the time of the UN Decade on Women (1975–85), an increasingly coherent world polity linked women's rights with economic development efforts. An institutional effect emerged from the global campaign to incorporate women into national development: Some type of official national women's agency is now found in almost all countries.

Using a combination of archival, secondary, and statistical analysis, Berkovitch demonstrates that the "contagion" of women's rights across the world, particularly since World War II, is not some mysterious effect of diffusion but the result of the integration of nations into a wider cultural system. She also specifies the gendering of this system without ignoring the historical demands of women's rights activists for both protective and equal rights.

As can most global explanations, this one can be faulted for its generality. It largely erases the differences among countries, all of which are seen to be advancing, albeit at different paces, into the "Western" bureaucratized culture of the world polity. Case studies of national women's agencies show how adoption of world models is neither a smooth nor a wholly productive endeavor in countries where institutionalization, and women's rights, are contested matters (e.g., Anne Marie Goetz, The Politics of Integrating Gender to State Development Processes, 1995).

Moreover, the concept of the "world polity" itself is a bit slippery. At times associations, institutions, discourses, or policies emerge as the primary components. Their relative importance does depend on the historical context, but at times the agency of institutions and the discourses themselves seem overemphasized. As a central example, it is puzzling that international organizations and discourses are given more explanatory weight in the promotion of women's rights precisely when "second wave" women's movements are exploding worldwide (see chap. 5). Where are the contributions made by "women in development/gender and development" activists to both discourse and action on development from the early 1970s onward? In addition, the material basis for policy change is secondary in the overall argument. Given that workplace rights are the focus, it would be interesting to have a more systematic assessment of the timing of both industrialization and the spread of labor market theories based on efficiency to judge their effect.

Moral Spaces: Rethinking Ethics and World Politics. Edited by David Campbell and Michael J. Shapiro. Minneapolis: University of Minnesota Press, 1999. 268p. \$19.95.

International Society: Diverse Ethical Perspectives. Edited by David R. Mapel and Terry Nardin. Princeton, NJ: Princeton University Press, 1998. 263p. \$35.00.

Yosef Lapid, New Mexico State University

In a seminal article published in 1992, Steve Smith astutely observed that the bizarre "forty years' detour" that had taken the scholarly field known as international relations (IR) away from moral questions and ethical dilemmas was coming to an end. At the heart of this "normative turn," wrote Smith, lies a renewed recognition that the IR "subject is unavoidably normative" (Steve Smith, "The Forty Years' Detour: The Resurgence of Normative Theory in International Relations," Millennium 21 [1992]: 490–514). The two excellent but strikingly different edited volumes under consideration confirm that a normative "turn" of sorts continues to mature in IR. They also suggest that the character of the scholarly work resulting from this trend will become increasingly more

contested as more parties lay claim to shaping IR's revitalized normative agenda. Contextualized in this manner, a joint consideration of these two books becomes a richly rewarding undertaking. At stake is the ability of a rapidly expanding group of scholars who profess a shared interest in both IR-related ethical questions and dilemmas and in an open and dialogical scholarly style to engage productively in scholarly practice.

Produced under the auspices of the Ethikon Institute, the format of the Mapel and Nardin volume is carefully shaped to serve the worthwhile mission of "bringing scholars representing a diversity of moral viewpoints into structured dialogue with one another." This mission is elegantly restated at the end of a brief, but effective, introduction by the two editors. It also lists the three related questions addressed by all contributors: What is the proper constitution of international society? What is the basis for the authority of laws and institutions in this society? And to what extent do these laws and institutions adequately reflect a concern for justice in a world of diverse cultural communities and deep economic disparities? The thematic unity of the book is further enhanced in the concluding chapter, written by David Mapel, which offers some suggestive interpretations regarding the main issues that emerge from this volume.

Perhaps the most attractive feature of the Mapel and Nardin volume is the editorial decision to opt for a nontraditional approach that allows spokespersons for alternative viewpoints in five ethical perspectives—legal positivism (Terry Nardin and Frederik G. Wheelan), natural law (Robert P. George and Richard B. Friedman), Kantianism (Pierre Laberge and Fernando R. Teson), contractarianism (John Charvet and Chris Brown), and cosmopolitanism (Brian Barry and David Miller)-to engage one another directly. The decision to include three religious perspectives-Jewish (David Novak), Christian (Max L. Stackhouse), and Islamic (Sohail H. Hashmi)—s also commendable, but the conspicuous omission of such major religions as Buddhism, Confucianism, and Hinduism cannot be easily explained. As a rule, the thirteen chapters are solidly researched and effectively written, although some of the philosophical chapters, in particular, are densely argued and may be less accessible to entry-level readers.

The volume edited by David Campbell and Michael J. Shapiro falls in the general area of ethics and world politics but involves a drastic change of vocabulary and intellectual landscape. This is hardly surprising, as Moral Spaces deliberately sets its sight at redefining the entire budding subfield of normative IR theory. One page into the bold and forcefully argued Introduction, Campbell and Shapiro submit that "the 'ethics and international affairs' literature is based on some questionable fundamentals" (p. viii). Noteworthy in this context is the explicit identification of a previous volume by Nardin and Mapel (see note 6, p. xix) as an exemplar of the kind of work that is being challenged or rejected. The main charge leveled against such "orthodox frames of reference" largely echoes the postmodernist critique of all modernist attempts to legislate morality through universally applicable codes of conduct. "Rather than engaging in the traditional search for a grounding for supranational principles," write Campbell and Shapiro, "the various contributions investigate the contingencies involved in specific historically situated

encounters" (p. ix).

In a spirited effort to replace "an epistemologically oriented ethics of discovery" with an "ethics of encounter without a commitment to resolution or closure" (p. xvii), in Moral Spaces editors and contributors alike draw heavily, but not exclusively or uncritically, on the work of Derrida, Foucault, and Levinas, in particular. The effort to establish a

principled distinction between a conventional "theory of ethics" and the (Levinasian) "ethical relation" is intriguing, as is the editorial emphasis on "the radical entanglement between moral discourses and spatial imaginaries" (p. ix). Resulting from all this is the demarcation of a promising research agenda that seeks a sustained (re)consideration of the relationship among space, subjectivity, and ethics in international relations and world politics.

All the contributions in Moral Spaces, a provocative and well-conceived volume, converge on this theme. This is remarkable in view of the fact that, with the exception of an original essay by Michael Dillon, all other eight chapters were previously published. In different ways, each individual chapter indeed stands alone as a significant scholarly contribution. Diverse issues are covered, including the link between community and responsibility (Daniel Werner); the figures of the "stranger," the "refugee," and the "emigre" (Michael Dillon, Bonnie Honig); suffering and justice (William Connolly); critical humanism (Kate Manzo); state-sanctioned violence (Patricia Molloy); and the ethics of global market research (Richard Maxwell). Combined into one volume, these chapters make a persuasive case for a far more radical expansion of the range of alternative perspectives that deserve serious consideration in shaping the agenda of the current normative turn in IR.

As already noted, I find considerable merits in both these volumes. In a basic sense, both address ways to overcome the same serious shortcoming in IR, namely, the absence of a well-conceived and properly developed normative element. And both offer the reader sophisticated and up-to-date contributions on current thinking on the nature of international ethics and/or ethical relations. In at least one fundamental sense, however, these volumes also give occasion to some alarming or disturbing thoughts. One cannot possibly walk away from them with any optimism about the prospects for dialogue and cross-fertilization between the approaches that they present and advocate.

Yet, profound differences in respective points of departure and underlying premises notwithstanding, one cannot, or perhaps should not, preclude the possibility of productive mutual engagement between them. This is particularly the case in view, for instance, of Mapel and Nardin's strong emphasis on the need to promote conversation between "dissimilar, even irreconcilable voices" (p. 13). This emphasis does not translate, however, to even one accidental reference to alternative ethics and international affairs projects or literatures similar to those favored by Campbell and Shapiro. And the latter, despite their gestural effort to reassure the "mainstream" that they conceive of their project as an alternative but "not the only alternative, and not the alternative" (p. ix, emphasis in original), repeatedly, and perhaps unnecessarily, engage in the kind of oppositional rhetoric that can easily turn scholarly discussions into bitterly polarized battlegrounds. Perhaps we see here the making of yet another major missed intellectual opportunity by those who hold radically different ontological, epistemological, and axiological views, but who share an interest in the same scholarly project (i.e., advancing the cause of a productive "normative turn" in IRs) to find some common ground for sustained dialogue and discussion. It is a deeply contested field, and there is perhaps no reason to believe, or to hope, that would-be participants in the shaping of IR's reenergized normative agenda would be able somehow to escape this contestation. Still, one cannot easily dismiss the thought that wider intellectual, disciplinary, and practical interests would be better served if such were the case.

Alignment Despite Antagonism: The United States-Korea-Japan Security Triangle. By Victor D. Cha. Stanford, CA: Stanford University Press, 1999. 373p. \$59.95 cloth, \$19.95 paper.

Mel Gurtov, Portland State University

What accounts for the "puzzling" uncooperativeness that generally characterizes relations between South Korea and Japan? Realist theory and balance-of-power politics cannot account for the constant friction in a relationship that, on strategic grounds, ought to be consistently cooperative. Victor Cha explores the dilemma, in both theoretical and policy terms, by using as his basic framework the notion of "quasialliance," "in which two states (Japan and South Korea) remain unallied but share a third party (the United States) as a common ally" (p. 3). Rejecting as incomplete explanations of Japan-Korea conflict that are based on historical animosity, economic structural differences, and domestic political influences, Cha seeks-for the most part, successfully-to extend realist analysis in order to arrive at a richer understanding not only of Japan-Korea relations but also of the still dangerous northeast Asia security situation.

An important contribution of Cha's well-researched book, which covers 1965 (when relations between Japan and South Korea were normalized) to 1998, is to qualify two different and widely accepted notions of what lies at the heart of Japanese-South Korean tensions: either the ugliness of their past relations or a common threat (North Korea) and need of a U.S. deterrent is a sufficient explanation. Beginning with an elegant case study of the 1965 normalization, Cha closely examines various phases in the volatile relationship. Of importance to both past and future policy in northeast Asia is his finding that the less Korean and Japanese leaders are convinced of the U.S. commitment, the more they are inclined to autonomous policymaking and cooperation with one another. Insecurity, in a word, promotes friendliness.

Yet, one wonders if this quite logical conclusion is sufficient. Even in 1965, but more so as we move into the 1980s and 1990s, Korean and Japanese choices were framed by factors other than U.S. security policies. Indeed, Cha does not neglect to mention them: pressures at home from political opponents, for example, and concerns about trade and technology imbalances that have long favored Japan, not to mention the constant intrusion of historical memory. Measuring "enmity," cooperative versus competitive behavior, and especially perceptions of the U.S. commitment (which often has been ambiguously communicated) is, it must be conceded, exceedingly difficult. In the end, it is hard to say that the case has been proven on behalf of the quasi-alliance theory. Rather, what Cha has accomplished is to put in the reader's mind a new set of variables to help explain Korean-Japanese tensions.

As Cha acknowledges (pp. 198, 230), the quasi-alliance theory may have less applicability today than in the Cold War era. In the new circumstances—diminished external threats; increased multilateral diplomacy; emphasis on the economic, technological, and financial components of security; and increasing pluralization of domestic political life-international relations in northeast Asia since the late 1980s are far more complex than they were during the Cold War. It is likely that Japanese and Korean security dependence on the United States will remain for some time, but there are many signs of change that make the realist argument, even with Cha's refinements, less compelling. Among them are South Korea's substantial trade and investment ties with China; the Four Party Talks (which might extend to six outside parties) on a final settlement of the Korean conflict; the multilateral Korean Peninsula Energy Development Organization

(KEDO), formed to alleviate North Korea's energy problems in exchange for a freeze on Pyongyang's nuclear weapons program; bilateral military consultations among China, South Korea, and Japan as well as those countries' involvement in the ASEAN Regional Forum to discuss security issues; and prospects for regular trilateral summit meetings of Korean, Chinese, and Japanese leaders.

Although Tokyo and Seoul are concerned as always about being abandoned by the United States, cooperation between them increased in the 1990s, bilaterally and multilaterally, at a time when the U.S. commitment to Asian security remains firm. For better or worse, Washington has not abandoned its Cold War priority of maintaining a balance of power and emphasizing its bilateral alliances in Asia. In such circumstances, relations between Japan and South Korea should be rocky, according to the quasi-alliance theory. In fact, although some long-standing issues remain, there are several indications of real improvement, such as joint financing of KEDO, Korea's removal of its ban on many Japanese cultural imports, coordination of policy on North Korea's missile tests, and President Kim Dae Jung's visit to Tokyo in fall 1999 in an effort to put historical bitterness behind the two countries. The explanation for these recent changes lies not only in Realpolitik—the shared concern of South Korea and Japan about North Korea's intentions—but also in South Korean democratization, U.S. prodding, and defense policy initiatives by both Japan and South Korea. A quasi-alliance and a strategic triangle remain, but they are not the only games in town.

Finance Capitalism Unveiled: Banks and the German Political Economy. By Richard Deeg. Ann Arbor: University of Michigan Press, 1999. 312p. \$49.50.

J. Samuel Barkin, University of Florida

Is globalization undermining the traditional Continental European model of finance capitalism and causing it to converge with the Anglo-American model? Richard Deeg argues that it is not. He begins with two archetypes, the coordinated market economy and the liberal market economy, representing the Continental and Anglo-American models, respectively. He concludes that there is some convergence, but the coordinated market economy model as represented by the German system of corporate finance is retaining its distinctiveness.

The book is divided into three sections. The first presents a theoretical overview of the two models of capitalism that Deeg uses as ideal types, coordinated and liberal market economies, and the arguments as to why globalization should be causing a convergence of the two. The second section examines the history and evolution of banking in Germany from the nineteenth century through the present. It explains the tripartite structure of German banking—the parallel development of the large private commercial banks, the public savings banks, and the private cooperative banks—and the principle of competition among rather than within the groups. It also specifically looks at the process of "Anglo-Saxonization," a combination of marketization and securitization, of the German financial system. The third section provides three regional case studies: North Rhine-Westphalia, Baden-Wurttemberg, and the East. The first two of these case chapters look at the evolution of finance capitalism within the Land in question, and the third looks at developments in what was East Germany since unification. These cases suggest that the German financial model is not monolithic, that there are important regional differences.

Deeg is trying to do two things: provide a detailed devel-

opmental history of the German system of corporate finance and make a case in the larger debate about globalization and the convergence of national financial systems. Both efforts display strengths and weaknesses. As a history of the development of the relationship between financial institutions and industrial corporations in Germany, the book for the most part succeeds. Drawing on primary sources and on an extensive reading of the secondary literature in English and German, Deeg paints a portrait of German finance that is both broad and detailed. As a history of the modern German financial system, this book is well worth reading. The two empirical sections, on the development of the tripartite system and on the regional cases, are not as well integrated as they might be, however. Yet, the two sections individually highlight phenomena (intersectoral rather than interfirm competition, and the degree of difference in financial systems in different parts of Germany) that do not get as much press as perhaps they should.

As an entry in the debate on the effects of financial globalization this book makes a useful but flawed contribution. It poses interesting and important questions and grounds answers in empirical research, a process often forgotten in the occasionally ideological and rhetorical debates on the subject. But the method through which Deeg matches questions and answers is problematic in two ways. The first concerns the model of the German system that the author uses as a rhetorical starting point. The coordinated market economy model is defined as one in which banks hold large stakes in industrial corporations and are thus able to play major roles in corporate management and strategy. As a result, this model of capitalism encourages longer time horizons in industrial strategy and management. Deeg then argues at some length that this model is inaccurate. But in many ways the model is a red herring. The idea that industrial restructuring in Germany has been less than ideally managed, or that government plays a significant role in the German corporatist system of industrial governance, will not come as surprises to anyone familiar with the German economy. But the efforts to argue against the idealized model sometimes seem to contradict the argument about globalization; many of the ways in which German finance is diverging from a pure coordinated market economy sound very much like convergence with the liberal market economy.

The second methodological problem is that the dependent variable of the argument is not always clear. It seems to oscillate between the banking system narrowly defined and the financial system more broadly defined, on the assumption that what is true for one is true for the other. But this assumption does not necessarily hold. For example, it may turn out that banks in liberal market systems, particularly in the United States with the demise of Glass-Steagall, will come to resemble the German model of Allfinanz. But it could be the case at the same time that German finance is more generally becoming less bank centered and more equity centered. In other words, some features of Anglo-American finance may be converging toward the German model, while other features of the German model trend toward the Anglo-American norm. The lack of clarity concerning the dependent variable undermines some of the potential for nuance in the argument.

Finally, some trends are barely mentioned that might have been discussed at greater length. Two examples are the role of European Union competition authorities and the generational change occurring in the Mitelstand. The former may well end up separating Land governments from their savings banks, making these institutions more like private corporate banks. What might this do for the tripartite structure upon

which the German financial system is based? The latter may, as Deeg points out, result in a trend among Mitelstand firms toward public ownership and thus toward equity rather than bank finance. This would make German finance look much more like the Anglo-American model than is currently the case.

In short, this book is a valuable resource for students of German finance, of German industrial development, and of processes of financial globalization. It does not in the end provide convincing support for its thesis that the German financial system is maintaining its coordinative character in the face of the pressures of globalization, but the process of getting there is both useful and informative.

Civilian Control of the Military: The Changing Security Environment. By Michael C. Desch. Baltimore, MD: Johns Hopkins University Press, 1999. 184p. \$69.00 cloth, \$22.95 paper.

Peter Feaver, Duke University

This book is a hallmark of the recent renaissance in civilmilitary relations theory, and it shares the characteristics common to the revival: an explicit emphasis on theory building/testing and an effort to link the study of civil-military relations to other debates within political science. Desch's book is the most ambitious of these new treatments and already has proven influential in shaping the debate within the academic and the policymaking communities.

Desch's first contribution is to operationalize his dependent variable so as to capture variation in civil-military relations even in countries that have not experienced the traditional bogeyman of the field, a military coup. The primary dependent variable is civilian control, which Desch defines as whether civilian policy preferences prevail over military preferences when the two diverge. Military preferences can prevail even without a coup; witness what happened to President Clinton's effort to lift the ban on gays serving openly in the military. Such a definition is not without its limitations—military influence can be so subtle and pervasive that it does not even generate publicized policy disputes for the analyst to measure—but it has more analytical purchase than most alternatives.

To explain variation in civilian control, Desch develops a structural model based on the nature and intensity of the threats facing the state. External threat has been a key variable in most civil-military models, but Desch adds considerations of internal threat to generate a two-by-two table: high external and high internal threats; high external, low internal; low external, high internal; and low external, low internal. Following social conflict theory, Desch claims that the intensity of the threat determines cohesion: When threats are weak, military organizations will be weak and divided; when threats are great, military organizations will unify so as to counter the threat more efficiently. The direction of the threat determines the orientation of military organizations, outward (against the enemy) or inward, where it can pose a challenge to civilian rule. Thus, the worst relations are found with a low external threat and a high internal threat; the best relations are with a high external threat and a low internal threat.

Note that whereas both Huntington and Lasswell predicted that the Cold War would produce bad civil-military relations in the United States—because the high threat would necessitate large forces that would chafe against the constraints imposed by liberal society—Desch's model makes the opposite (and correct) prediction.

In two important threat configurations, Desch's model is, by the author's own admission, indeterminate. When both internal and external threats are high, the model cannot say whether there will be good civilian control, because the external threat has oriented the military outward, or bad civilian control, because the threats have unified the military and the presence of an internal threat is sufficient to distract the military inward. When both threats are low, the model cannot say whether there will be good civilian control, because the military will be divided (and thus, presumably, easy to control), or bad civilian control, because without any threats the military's orientation will be uncertain (or because the divided institutions slip into contentious factionalism). Which outcome prevails depends, according to Desch, on ideational factors, such as organizational culture and clashing world-views, a story that transpires more or less off Desch's structural stage.

Desch's model is attractively parsimonious and generalizable, giving an account for shifting patterns of civil-military relations in great as well as small powers and under a variety of different international conditions. Perhaps because the model is so parsimonious, however, one wonders whether the causal mechanisms are undertheorized. For instance, it is not clear why an intensified internal threat does not unify the government as a whole, thus strengthening all governmental institutions, including the civilian ones with which military institutions might vie for political power.

Desch does suggest six causal mechanisms (in his model they are intervening variables), but in doing so he muddies somewhat his otherwise clear dependent variable of obedience: (1) the degree of experience the leaders are likely to have; (2) how unified the civilians are likely to be; (3) the kind of control mechanism civilians are likely to adopt; (4) how unified the military is likely to be; (5) the orientation of the military; and (6) how divergent the ideas of civilians and the military are likely to be. Although Desch does not treat them in this way, these intervening variables are in fact the direct predictions of his model and thus collectively represent an alternative dependent variable that I would call a "conflict" variable. Conflict is probably related in some degree to obedience; we would expect that higher degrees of conflict will be associated with lower degrees of obedience. The two variables are, however, logically separate: A country can experience lots of conflict and friction even while, and perhaps precisely because, civilian preferences are consistently prevailing over military preferences.

Desch tests his model with case studies of twentieth-century (and primarily post-World War II) civil-military relations in eight countries (Argentina, Brazil, Chile, France, Germany, Japan, Soviet Union/Russia, and the United States). Since threat conditions in these countries fluctuated during the time studied, he has ample variance on his explanatory and dependent variables. Historians might quibble with the reliance on an extensive secondary literature, but this is precisely the kind of cumulation of knowledge that normal science supposedly affords, and it allows Desch to be far more comprehensive and sweeping than a more archive-intensive study might permit.

Desch sees more support for his model in the case studies than do I, but I find his empirical judgments compelling even about ambiguous facts. Indeed, his coding on U.S. civilmilitary relations is likely to be one of the most cited points in the book. In a very important appendix, Desch documents whose preferences prevailed in roughly seventy-five "major civil-military conflicts" since 1938. He finds that civilian preferences clearly prevailed in all but four conflicts during the Cold War, but military preferences prevailed in seven of the twelve cases in the post—Cold War period.

The book is important because it makes a bold argument

based on a clearly advanced theory and tests it against an extraordinary range of cases. Desch's exceptional empirical scope bears emphasis, since it distinguishes his work from other theoretically ambitious treatments of civil-military relations (including my own), which tend to look at only a few or even just one country. No other study has paired such a generalizable and parsimonious theory with such a diverse range of cases. On reach alone it is a landmark study.

As might be expected, to pursue such an ambitious goal Desch had to accept quite a lean model, and one might still want a theory that would pay greater attention to the micro-foundations of civil-military relations. But on its own terms the argument is provocative and compelling and highly recommended.

Russia Confronts Chechnya: Roots of a Separatist Conflict. By John D. Dunlop. Cambridge: Cambridge University Press. 1998. 234p. \$18.95 paper.

William Zimmerman, University of Michigan

At century's end, the problem of war between clearly independent states seems to have been largely resolved. The realists' key distinction between politics under conditions of anarchy and other politics turns out to be a wise one, if and only if that distinction is not conflated with the distinguishing politics among states and politics within states. Anarchy, self-help, and the persistent danger of large-scale violence largely exist within states, or former states, especially within those states wherein elites are staking out claims for the independence of some territorially specific parts of that state. The Yugoslav wars in the 1990s have been an obvious example in the Balkans. There have been multiple instances in the Caucasus as well, where it is likely more blood has been shed in the past decade, although without coverage by CNN, than in the Balkans. There have been wars there within the Republic of Georgia and between Armenia and Azerbaijan over Nagorno-Karabakh. There, too, occurred the Russian-Chechen conflict in 1994-96, which resumed in 1999 when forces from Chechnya, probably not controlled by the national leadership of the republic, attacked neighboring

Especially for those who do not read Russian, John B. Dunlop's Russia Confronts Chechnya provides a valuable prehistory to the 1994–96 war. It is largely sympathetic to the Chechen cause. As is more common for historians than political scientists, the analysis is largely buried in the description

But the key points do emerge. (1) Like the Serbs and the Kosovars (and unlike the Serbs and the Croats), the Russians and the Chechens have been at each other's throats for some while now; the first conflict per se dates from 1722 and Peter the Great's efforts to expand into the Caucasus. (2) The cause of these conflicts was not the radical Islamic faith of the Chechens. Rather, the causal arrow largely went the other way. The Chechens, Dunlop argues, became "observant Muslims" as a result of the "perceived massive aggression on the part of the 'infidel' Russians" (p. 10), which transformed them from "superficially" Islamic peoples into "observant Muslims." (3) Ethnic cleansing is scarcely a new phenomenon. Much of the story of Chechnya in the past 150 years is one of forced migration followed by the return to the Caucasus of many survivors of the migration. The most recent forced migration took place in 1944 under Stalin. (4) Ethnic awareness for the Chechens and others in the northern Caucasus—as opposed, for instance, to clan identity was largely a consequence of Soviet efforts to minimize a "'mountaineer' identity" among all the peoples of the Caucasus. (5) There has not been much learning in Moscow about how to handle Chechnya. The temptation to resort to a policy composed of some blend of deceit, repression, and deportation has been difficult to resist, even though such policies, while sometimes successful in the short run, have generally been counterproductive over the longer haul.

These observations and the historical narrative in which they are imbedded are intended by Dunlop to serve as the backdrop to a successor volume that will treat the massive defeat of the Russian forces in Chechnya in the 1994-96 war. That war began in December 1994, only a few months after President Boris Yeltsin stated: "If we used force in Chechnya, it would . . . lead to such turmoil, so much bloodshed, that no one would forgive us afterward" (p. 164), and it ended when General Alexander Lebed negotiated an agreement with the Chechens in summer 1996 that virtually recognized the de facto independence of Chechnya. Sad to relate, there will probably be need for future volumes as well, beginning with a third to cover the latest round that began in 1999. In that round the Chechens, by virtue of their actions since the 1994-96 war, had far less by way of the moral advantage so central to Dunlop's account of the prehistory of that war, and the Russians demonstrated they had altered their tactics in response to the lessons of the humiliating 1994-96 defeat (and NATO practices in Kosovo) but little else.

The Third World Beyond the Cold War. Edited by Louise Fawcett and Yezid Sayigh. New York: Oxford University Press, 1999. 256p. \$78.00.

Ashok Kapur, University of Waterloo, Ontario

The Cold War and the Third World are two overly written, overly studied, and undertheorized topics in the social sciences literature. This book is another attempt to theorize, and readers need to grapple with many meaty questions. Was the Cold War a turning or a punctuation point in history compared to the two world wars? What is the scholarly record in explaining and predicting the end of the Cold War and its effect on the Third World? What is the effect of state behavior in the post-Cold War Third World? What is meant by the Third World, and what has changed in it since the end of the Cold War? Was the Cold War an agent or a source of change in the Third World? Can change in the Third World be understood best by way of a top-down or a bottom-up approach? Is there a Third World beyond the Cold War? Part I of this book offers thematic assessments, and Part II provides regional analyses.

The volume makes several points, but there is no consensus among the authors. (1) The ties among the developing countries have weakened; Third World solidarity was a temporary illusion. (2) The concept of Third World identity is fragile. (3) There is no accepted definition of the Third World or about the criteria to define it (economic condition, political culture, poverty, or relations to bipolar system), but there is a reluctance to drop the term. (4) Regionalization has emerged (pp. 6–10). (5) Chapters 7–8 speak about the marginalization of Africa and South Asia. (6) Recentralization of the state is occuring in the Middle East in the context of changing local and international conditions.

Recentralization is aided by the fact that "greater reliance on international capital for local development can actually increase the allocative power of the state and lead to 'recentralization.' "Two additional factors are at work. First, democratization is not on the agenda of the United States or major international financial institutions, which are more

concerned with governance (i.e., good public administration and managerial skills). Moreover, these global actors tend to overlook authoritarianism in the often intertwined causes of combating political Islam and of overcoming domestic resistance to economic liberalization. Second, as a result, the domestic and expatriate entrepreneurs who might be the potential engines of civil society are equally likely to fuse with the political elite in a property-owning class and collude in the construction of authoritarian liberalism (p. 228). This powerful insight is based on Middle East experiences, and it deserves to be tested in relation to other regional and state experiences.

The book also is about the failures of social scientists to explain and predict major international developments, such as the end of the Cold War and the consequences. This book argues that the end of the Cold War is not the end of the Third World, as realists and liberal theorists concluded (p. 96).

There is disappointment that hegemonic stability and the "unipolar moment" did not happen (p. 29). The authors disagree about the role of the state in international change. On the one hand, the realist emphasis on the state and the system of states is questioned (pp. 15, 17); on the other hand, it is asserted (chap. 9). What explains Third World outcomes? Consensus is lacking on this among the authors. Is it bipolar international structure? Is it the structure of the world economy? Is the regional power structure the appropriate level of analysis? Is the internal character of the unit important? Or should the emphasis be on the rise of "low security" issues, such as human rights and the environment in the context of state-society relations? The reader is teased to consider all the above, but the authors themselves are like the proverbial blind folks who offer different views of the elephant.

The volume offers definitions of Third World, but these are suggestive, not definitive. They include the usual inventory of contrasts: core-periphery; developed-developing; colonialısm-decolonization; central-marginal; dominant-subordinate; strong-weak states; and so on (pp. 18-21). Historically, the race question has been an issue in the anti-Western perception and revolt of the Third World. Surprisingly, this aspect and the role of nationalism in the Third World have been brushed aside. The effect of the Cold War on the Third World is considered, but again there is no consensus. The first view is that the Cold War and bipolarity increased or exacerbated the incidence of conflict in the Third World. The second view is that the superpower competition was constrained, and hence Third World's stability was enhanced. The third view is that the Cold War did not matter (pp. 22-3). It would be nice if the contributors had committed themselves to a position and argued it convincingly, rather than leave the reader to find the answers to the questions.

The book is flawed in the sense that definitive conclusions are not reached. Also, there are gaps in the framework. Bipolarity is treated as a given in the definition of the Cold War; yet, a close inspection of foreign policies and developmental policies of Third World countries would reveal that the context of decision making was multipolar, even in the heyday of the Cold War. For instance, the presence and influence of China in African and Asian politics begs analysis. Furthermore, from within the ranks of the Third World a number of regional powers—such as China, India, Vietnam, and Iran—have emerged. They have qualities of a "weak" society as well as a strong state; they are upwardly mobile in regional and international relations, and they have made strategic and diplomatic choices that made it impossible for

the international powers to decouple themselves from the rising regional power centers during the Cold War and following its end. The authors seem to be shy about discussing regional powers and regional security structures. Consequently, the work misses the opportunity to consider the changing structural context and the pattern of engagement between particular Third World states/societies and the international powers. If the Cold War symbolizes the presence and capacity of the stronger international powers, then the Third World symbolizes the presence of the "weaker" (not necessarily weak or insignificant) members of the system of states today. It would help to know about the strategies employed by the stronger and the weaker players vis-à-vis one another during and after the Cold War. Such an investigation could help capture the promise in the title and the subtitle of the book.

Despite its defects, the book is a bold venture. It should be required reading for graduate seminars dealing with international relations and the Third World. Contemporary theory obviously has failed to explain and predict major international events and to grasp the fundamental challenge of Third World nationalism, economic reform, and new military and nuclear capacity vis-à-vis Western policy interests and Western scholarly leadership. This study implicitly urges us to address the intellectual catastrophes of the social sciences, to revisit failed scholarship, and to find new research strategies to discover the points of engagement between specialists in the international relations and the Third World and between theoreticians and practitioners in the North and the South.

Even though the findings are not definitive and the volume lacks a unified research design, the editors have made a good contribution by opening up lines of inquiry that are likely to stimulate new researchers.

Must Global Politics Constrain Democracy? Great-Power Realism, Democratic Peace and Democratic Internationalism. By Alan Gilbert. Princeton, NJ: Princeton University Press, 1999. 316p. \$55.00 cloth, \$17.95 paper.

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In the nearly 20 years since the publication of Michael Doyle's essay on the so-called democratic peace ("Kant, Liberal Legacies, and Foreign Affairs, Parts I and II," Phulosophy and Public Affairs 12 [Summer 1983]: 205–35; [Fall 1983]: 323–53), hundreds of journal articles, books, and dissertations have appeared that support or attack researchers' claims about as well as the theoretical premises and empirical bases of the concept. Alan Gilbert's contribution to this debate is unlikely to convince the warring factions of their errors, but it offers a devastating critique of American "democracy" that does serious damage to the methodological arguments and epistemological foundations of the two sides.

The fundamental questions—Do democracies ever go to war with one another? Are democracies inherently more peaceful?—appear, deductively, to be answerable mostly in the affirmative. But what has come to be described as "the closest thing we have to a law in international politics" (Jack S. Levy, "Domestic Politics and War," Journal of Interdisciplinary History 18 [Spring 1988]: 653–73) is better understood as a custom than a law in the physical sense. It takes only one failure to invalidate a physical law, whereas customs are violated all the time. More than that, as a social institution, democracies (or, rather, their members) are intersubjective, which means that they change over time and across space. For several reasons, this renders problematic any efforts to

assess the empirical basis of the claim for the democratic peace.

First, what is a democracy? Are the democracies of ancient Greece epistemologically the same as those of the early nineteenth century and the late twentieth? Is the practice of democratic forms, such as contested elections, equivalent to institutionalized democracy? For example, both Croatia and Serbia had elected governments during their war over Bosnia, but are they any more than oligarchies with parliamentary forms, as Gilbert puts it?

Second, what is a war? For the purposes of building databases, war has come to be defined as organized violence in which there are more than 1,000 deaths. What about conflicts with fewer casualties? Or civil wars? Or interventions by great powers in the affairs of weaker ones?

Finally, what constitutes a statistically significant data set? John Mearsheimer argues ("Back to the Future: Instability in Europe after the Cold War," International Security 15 [Summer 1990]: 50-1) that, because war is so rare and democracies so uncommon, it is not surprising that there are so few cases of democracies at war. In response, supporters of the democratic peace thesis have combed the historical record to find additional examples of regimes that fit the requirements. To no one's surprise, they have discovered additional cases and engendered further disputes.

Why does this debate matter? There are two primary reasons, both of which are addressed by Gilbert and both of which emerge from the fact that this conflict is one more engagement in the perennial liberal-realist war over international relations theory. First, liberals claim, normatively, that peace is good; consequently, the spread of democracy should reduce the incidence of war. That would be good for American national interests, an argument taken up by the Clinton administration, which made "democratization" one of the pillars of its foreign policy. Second, realists counter, normatively, that such hope is a thin reed on which to base one's national security, and it is a dangerous illusion akin to the idealism of the interwar period. Power remains a more reliable means of preventing war. Regime structure does not matter.

Gilbert launches a grassroots-based guerilla assault on the epistemologies of both positions, and he also raises serious challenges to both liberal and realist conventional wisdom. He argues that the United States is better understood as an "oligarchy with parliamentary forms," with the result that public opinion, normally thought to constrain warlike behavior through democratic practices, is largely ineffective. He points, in particular, to the American propensity to destabilize and overthrow regimes that are, paradoxically, too democratic for Washington (e.g., Guatemala in 1953, Iran in 1954, Chile in 1973). Such interventions, although violent, are not normally counted as "wars" because they are "domestic" and result in too few casualties. Such omissions matter because the United States represents one member of the many democratic dyads whose historical behavior is deemed to constitute the empirical validation of the democratic peace thesis.

These interventions, justified on the basis of great power rivalry, have a more pernicious consequence. They generate what Gilbert calls "antidemocratic feedback," domestic corruption of U.S. democracy, and broad cynicism about American intentions. But such behavior, argues Gilbert, also has paradoxical tendencies that support the democratic peace thesis. Although oligarchic leaderships proceed to war with minimal attention to domestic opposition—indeed, treat it as tantamount to treason, as seen in the marginalization of Hans Morgenthau by the Johnson administration during the Viet-

nam War—the failure to attend to a common good fosters a "democratic internationalism" among public movements opposed to the adventurism of the leadership. Gilbert calls this the "democratic feedback from international rivalry." He extends this general argument to condemn, as well, the oligarchic propensities of global capitalism to ignore both a common good and public pressure for democratization of international economic institutions and practices.

The controversy over the democratic peace thesis has been, and continues to be, driven not by "objective" scientific goals but largely normative ones. Intentionally or not, Doyle's original proposition served the Reagan administration's campaign on behalf of "democratic" regimes aligned with the United States against the Soviet Union. Today, the argument that democracy and markets go together supports the American campaign in favor of open markets and economic growth, and critics of marketization are seen as crypto-Marxists and losers in the great historical triumph of liberalism. Realists warn that belief in propositions such as the democratic peace can only end in sorrow, for to trust the good intentions of others—even democrats—is to tread the same path that led to Munich. Moreover, there is a large national security complex to whom "perpetual peace" might pose other kinds of dangers. The openly normative thrust of Gilbert's book is a breath of fresh air in a debate that appears to be increasingly concerned with statistical minutiae as opposed to ethical concerns and political behavior.

Ballots and Bullets: The Elusive Democratic Peace. By Joanne Gowa. Princeton, NJ: Princeton University Press, 1999. 136p. \$27.50.

John M. Owen IV, University of Virginia

Over the past several years Joanne Gowa has published a string of noteworthy attacks on the widely held proposition that liberal democracies seldom if ever fight wars against one another—the so-called democratic (or liberal) peace. Now Gowa combines, refines, and expands her attacks into a general offensive to which defenders of the democratic peace will have to respond. Its theoretical and empirical claims, if true, are devastating to the research program. Gowa argues that the analytical foundations of the democratic peace are wobbly at best; that the statistical evidence for it is inconclusive and even contrary; and that the structure of the international system better accounts for what appears to be democratic peace. She concludes that the United States should not promote democracy in other countries, at least insofar as America's goals include peace.

Gowa opens by arguing that there are no compelling a priori reasons to think that democracies would not fight one another. Democratic leaders are just as self-interested as others; democratic institutions should in principle constrain leaders no more than autocratic ones; and high trade levels should not prevent war inasmuch as states have incentives to continue trading during hostilities. Although Gowa's arguments make sense under certain rational-choice assumptions, scholars working under different assumptions will be unmoved. For example, much of Gowa's skepticism about democratic institutions rides on her assumption that leaders are not myopic (pp. 21–3). Many scholars would be delighted to find one such leader in the real world.

More compelling are empirical findings that suggest the democratic peace is an artifact of the Cold War. Based on an examination of pairs of states between 1815 and 1981, Gowa

finds no statistically significant relationship between democracy and peace before 1946. The same results obtain when the dependent variable is either militarized interstate disputes (MIDs) or alliances. What is more, she finds that before 1904 democracies were *more* likely to get into MIDs and *less* likely to ally with one another. But the relationships among democracy, peace, MIDs, and alliances are robust from 1946 to 1980, which suggests that either democracies themselves have changed (a hypothesis Gowa argues has insufficient theoretical support) or some omitted variable accounts for the post–World War II peace among democracies. The omitted variable, Gowa argues, is interest. During the Cold War, democracies did not fight one another because they faced a common physical threat, the Soviet Union, not because they were democratic.

Gowa's empirical inferences about the democratic peace turn on some dubious operations on the data set. An earlier criticism called these operations ad hoc (Zeev Maoz, "The Controversy over the Democratic Peace: Rearguard Action or Cracks in the Wall?" International Security 22 [Summer 1997]: 162-98); Gowa now attempts to justify them theoretscally. Her first and more serious move is to eliminate the two world wars and thereby 59.1% of all the warring dyads (p. 48). This she justifies by subtly changing the central question from "Do democracies ever fight one another?" to "Do democracies ever fight one another in 'non-general' wars?" The problem with general wars, she contends, is that they have a distinct set of causes, in particular "buck passing" and other diffusion mechanisms (p. 47). Whether or not that contention is correct, Gowa never explains why diffusion mechanisms would upwardly bias the democracy coefficient.

Gowa's second move is to chop the remaining data set into three periods: 1816-1913, 1919-38, and 1946-80. A chisquare test shows that the p-values, or probabilities that random chance could account for the democratic peace, are 0.42 in the first period (0.21 if Spain is not counted as democratic in 1898), 0.17 in the second, and 0.004 in the third (pp. 61-2); thus, the peace among democracies is indeed more impressive as one moves through time. Gowa grounds this second move on the proposition that the structure of the international system differed in these three periods (p. 55): 1816–1913 was multipolar, 1919–38 "unstable," and 1946–80 bipolar. Yet, suppose we consider the interwar period to be multipolar (it was arguably no less stable than 1890-1914): combining 1919-38 with 1816-1913 yields a p-value of less than 0.015, a figure that would stimulate the salivary glands of any betting person.

Gowa's claim that the peace among democracies is significant only during the Cold War, then, is problematic. It turns on an arbitrary change in the dependent variable and a highly contestable assertion about the structure of the international system between the world wars. Yet, her findings that democracies are significantly likely to get into MIDs and significantly unlikely to be allies before 1914 do challenge those scholars who assert that the democratic peace implies generally smoother relations among democracies. Gowa's results suggest that democracies begin to favor one another most during crises, that is, their preferences are fluid rather than fixed.

Gowa's positive argument—that the allegedly democratic peace is an artifact of common security interests produced by postwar bipolarity—has been challenged elsewhere for its use of alliances as proxies for interests (William R. Thompson and Richard Tucker, "A Tale of Two Democratic Peace Critiques," Journal of Conflict Resolution 41 [June 1997]: 428–54). Thompson and Tucker report that anocracies, or

states with both democratic and autocratic properties, are significantly likely both to be allies and to get into MIDs. Because common interests would imply fewer MIDs (assuming fixed interests), alliance portfolios are not good proxies for interests. In response, Gowa here offers an historical narrative of major-power relations in various periods to demonstrate that interests and alliances generally coincide (pp. 70–82). This narrative is primarily a description of the rise and fall of various great power alliances, however, and thus is inconclusive. Moreover, it suggests, counter to Gowa's own argument, that domestic politics matters. Britain lost interest in an alliance with Austria-Hungary when the Conservatives took office in 1880, and Germany dropped its alliance with Russia in 1891 partly because of "political shifts within [Germany]" (p. 73).

Gowa undermines her own conclusions in two ways. First, she argues for a robust autocratic peace (pp. 103-8); but if such a thing exists, then states' domestic properties must matter, a proposition more compatible with the democratic peace program than with her structural realism. Second, she closes the book with policy recommendations that, according to her own argument, are superfluous. Gowa is concerned that U.S. policymakers actually believe in democratic peace and therefore are imprudently trying to spread democracy. But she has just spent an entire book arguing that democracies, like all states, always respond rationally to the imperatives of the international system. If the United States after 1989 is an exception to her universal rule of state rationality, then America and other democracies at other times could be as well. As soon as one acknowledges that ideas can affect international relations, one allows that there may indeed be a democratic peace.

International Relations on Film. By Robert W. Gregg. Boulder, CO: Lynne Reinner, 1998. 310p. \$55.00 cloth, \$22.00 paper.

Juliann Emmons Allison, University of California, Riverside

Robert Gregg makes it clear that popular culture has contributed more than Stanley Kubrick's manuacal Dr. Strangelove to our understanding of the world. According to Gregg, movies dramatize the people and events that mark the practice of world politics in ways that often challenge conventional wisdom about international relations, and they almost always provoke rousing discussion. His novel text emphasizes these points by reviewing 150 feature films in terms of contemporary theories of international relations and the evolution of the state system. Gregg's fluid exposition of the multifaceted union between films and the study of international relations exceeded my expectations for a supplemental introductory text on that topic. Perhaps most important, International Relations on Film offers instructors a valuable alternative means of reaching students who are increasingly technologically worldly yet remarkably uninformed about the world in which they live.

The text opens with an introduction to the ways in which film and international relations might be usefully related. More specifically, Gregg explains that feature films not only attach a picture to the thousands of theoretical and historical words that constitute the study of international relations but also increase our knowledge about the world. He is, however, quick to caution that the considerable degree of historical license permitted in film, combined with the tendency of predominantly American and European film makers to focus on the major conflicts of their times, make movies better teachers of war, in general, than of the particulars of peace.

The introduction is followed by ten substantive chapters, each of which focuses on a single concept—sovereignty, nationalism, intervention, espionage, crisis decision making, war, economics, international law, culture, and domestic politics—selected for its joint importance to international relations theory and prominence on screen. These chapters, as a whole, provide a nice review of major themes in international relations theory by demonstrating the ways in which the cinematic treatment of selected concepts support or contradict scholars' expectations about world politics. A reflective conclusion suggests that the persistence of these concepts in film over time, despite the emergence of such current issues and themes as the spread of infectious diseases and the role of women in positions of power, is a sign of continuity despite change in the practice of international relations.

Although Gregg does not in any way categorize the concepts emphasized in International Relations on Film, his discussion of a number of them, the norm of sovereignty and the principle of nonintervention in particular, follows a straightforward appraisal of their centrality to realism and the paradigmatic state system. The chapter on sovereignty, for instance, questions the enduring utility of the Westphalian territorial state. Considering the increasing consolidation of the world's advanced economies, despite the rampant diffusion of democracy and capitalism that has occurred since the end of the Cold War, is it reasonable to vest power ultimately in the independent and equal nation-state? In response to this question, Gregg uses The Mission to illustrate that the universality of the sovereignty norm was already debatable just a hundred years after its establishment. In that film, an agreement between Spain and Portugal to redraw the boundaries between their territorial possessions in South America sacrifices papal support for the Jesuit missions there as well as Church protection for the native Guarani Indians from predominantly Portuguese slave traders. Clearly, according to Gregg, European nations have thus long been more sovereign than other nations and cultures.

The chapter on the principle of nonintervention likewise questions the value of the state system. Here Gregg follows a theoretical discussion of nonintervention, or the absence of forcible or dictatorial involvement by one nation in the internal affairs of another, with a comparison of the onscreen exploits of the United States in Latin America to the failure of the international community to arrest the genocide in Cambodia under the Khmer Rouge. He presents Under Fire, in which the U.S. government and American journalists join Nicaraguans in revolt against the Somoza regime; Salvador, a fictional account of the murder of Archbishop Romero and the rape and murder of nuns during the U.S.-supported civil war in El Salvador, and Missing, wherein a man charges the United States with his son's disappearance and death during a military coup in Chile as indicative of the immorality of the U.S. clandestine intervention in Latin America. Gregg contrasts such "calculated and purposeful" intrusions with the failure of anyone at all to challenge the Khmer Rouge. Gregg's cinematic touchstone in this case is, of course, The Killing Fields, which chronicles a Cambodian interpreter's struggle to survive a slave labor reeducation camp following the Khmer Rouge takeover.

Another subtle categorization of concepts treated in the book includes chapters—such as those on economic interdependence, international law, and culture—that examine the myriad ways in which domestic and international institutions, broadly defined, shape world politics. The chapter on international law, for one, provides a fine comparison between

structural realism and institutionalism that emphasizes the importance of morality to a complete understanding of international affairs. Gregg uses Casualties of War, the story of American soldiers who are tried and court-marshaled for raping and murdering an innocent girl while on patrol in Vietnam, to illustrate how the moral disposition of individual nations may arguably assure the potential for ethical international relations.

Institutions also figure prominently in the chapters on economic interdependence and "the clash of civilizations," in which Gregg discusses the consequences of cultural miscommunication for international trade and finance. His focus is the late Cold War economic competition between the United States and Japan, which, Gregg argues, was exacerbated by the failure of principals on both sides to interpret cultural cues correctly. According to Gregg, this cultural clash resulted in the kind of American paranoia about Japanese influence in the United States featured in such films as Rising Sun and Gung Ho. Economic interdependence and antagonistic business practices dovetail in Rusing Sun, which casts a mercantilist Japan as a threat to the security of the United States on account of its favorable position in negotiations to acquire Microcom, a strategically valuable American computer software company. Interdependence takes a back seat to cultural confusion in the comedy Gung Ho, which pits American individualism against Japanese company loyalty when "Assan" saves Hadleyville, Pennsylvania, by taking over that town's dying automobile plant.

Even the very selective summary of theories and films provided here is sufficient to show that International Relations on Film fulfills Gregg's purpose for writing it, that is, to demonstrate the ways in which feature films highlight certain aspects of world politics. It is a pity he stopped there. Gregg's juxtaposed reviews of international relations theory and selected films imply that popular and scholarly perceptions of critical issues of world politics, such as armed intervention or economic liberalization, often diverge. What does this mean for politics? Whose version of the reality of international crises and other events matters? A quality response to questions such as these could have elevated Gregg's otherwise very informative and entertaining textbook to the level of essential reading for anyone doing research on the relationship between the public, if not pop culture, and foreign policymaking.

Resolving Regional Conflicts. Edited by Roger E. Kanet. Urbana: University of Illinois Press, 1998. 257p. \$31.95 cloth, \$21.95 paper.

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The end of the Cold War has had numerous effects on the study of world politics; two of the more visible have been to focus scholarship on (1) intrastate conflict in addition to interstate conflict and on (2) regional, as opposed to global, explanations. This seems rather odd when so much of our daily life is bombarded with notions of globalization. The volume edited by Kanet is another example of efforts to look to the uniqueness of regional relationships for explanations of general phenomena. There are both costs and benefits to this approach.

The volume consists of ten chapters, each of which addresses some aspect of regional conflict. These chapters do not cohere well. Some focus on a conceptual framework with which to think about regional conflict and its management, some on specific conflicts, some more broadly on regions, and

some on mechanisms for the management of conflicts. What is missing is an introductory chapter that provides a framework to which each chapter can be linked. For example, in chapter 1 Edward Kolodziej develops a way to think about regional security communities. His hierarchical model of a security system reflects the "degree to which the states and people of a region are willing to resolve their differences and govern their interdependent affairs through peaceful bargaining and negotiations rather than through coercive threats and force" (p. 16). The model is useful, even though it implicitly assumes that the differences across regions provide most of the variation in the degree of conflict. None of the other contributors use this conceptual framework to guide their analysis.

The following three chapters focus exclusively on the Irish or the Pakistani conflict. In order, they are an anthropological treatment of the background to the Irish conflict, a normative treatment of the case for impartiality in efforts to manage that conflict, and an historical description of the issues involved in Pakistan's regional security. None of these chapters even hint at the usefulness of Kolodziej's theoretical framework, and even the successive chapters on Ireland do not seem to be linked in any useful manner.

The fifth chapter, by Stephen Cohen, presents another useful framework for thinking about the specific regional aspects of what he calls "paired minority conflicts." He correctly assumes that the conditions for peaceful relations can be identified through an understanding of the causes of conflict. He then asks and answers: "How are psychological minorities created out of physical majorities" (p. 110)? A number of factors, he posits, contribute to this, such as traditions, domestic sources of external perceptions, and generational experiences. His thinking then leads to coping strategies for threatened minorities, not all of which would seem to be at the immediate disposal of the threatened group, but they surely represent theoretically possible alternatives. This chapter, like chapter 1, does not appear to help shape the thinking of some of the more case specific chapters. In many ways this is unfortunate.

Part III starts with a philosophical discussion by Paul Schroeder. Again, the connection to an overall theme of the volume is lost in the walk through political history, power politics, and "utopian" ideas. An assumption at the core of his argument is that "the fundamental causes of war have not remained the same in different regions and eras" (p. 143). To many this assumption is questionable, even though it is consistent with the organization of the volume. Schroeder articulates three sources of what he calls "utopian hopes," none of which "has ever worked or ever can": realism, legalism, and normative or peace through justice ideas. The remaining chapters focus on the role and success of peacekeeping efforts in various conflicts. Paul Diehl examines the conditions for success in Somalia, Cambodia, and the former Yugoslavia. Four factors are considered when evaluating the outcomes of each of the UN efforts: neutrality, geography, third-party behavior, and subnational actors. The next chapter, by Munck and Kumar, examines the effect of outside interventions in El Salvador and Cambodia, although they focus on a different set of criteria to judge intervention effectiveness. The two consecutive chapters that discuss the effect of peacekeeping operations in Cambodia focus on different criteria and come to different conclusions. Diehl argues that "UNTAC is generally regarded as a success" (p. 170), whereas Munck and Kumar conclude that some parts of the UNTAC operation had "negative effects," and in general the overall program achieved only limited success in Cambodia. The beauty of these chapters is that they give us specific criteria by which to judge intervention effectiveness, although the gist is that different criteria obviously lead to different evaluations. The last two substantive chapters focus on Eastern Europe and Russia, respectively, and have a similar fit to the overall volume.

I do not mean to give the impression that the volume is not useful, but it makes a contribution that is somewhat different from my initial expectations. I was teaching a course in conflict management at the time I reviewed this book, and my students had an assignment to analyze management efforts in a particular conflict. When they came to me for advice on how to approach a specific issue or problem, I found myself referring them to the individual chapters in Resolving Regional Conflicts. In their own individual ways, the contributors to this volume provide a useful overview of or framework for thinking about regional conflicts, either as background to a specific conflict or a general framework. So what becomes a liability in one intellectual environment is an asset in another.

Some of the discontinuity in the volume could be-and perhaps should have been—overcome by a more coherent job of editing and pressing the authors to hold to a conceptual framework. The underlying assumption in a number of the chapters, and surely in the Introduction and first chapter, is that the primary explanations for conflict can only be found by a regional focus, and this will remain debatable. There is little in the volume to convince me that a focus on more general explanations should give way to regionalism. Certainly, any effort to manage conflict effectively should be attuned to the unique contribution of a specific environment, but the volume could have profitably adopted a general framework that was subsequently applied across a number of regional conditions. In the end the book should be considered more for the independent contribution of each chapter than for its contribution to our theoretical understanding of the resolution of regional conflicts.

The Currency of Ideas: Monetary Politics in the European Union. By Kathleen R. McNamara. Ithaca, NY: Cornell University Press, 1998. 185p. \$39.95 cloth, \$16.95 paper.

Monetary Politics: Exchange Rate Cooperation in the European Union. By Thomas H. Oatley. Ann Arbor: University of Michigan Press, 1997. 222p. \$49.50.

Political Economy of Financial Integration in Europe: The Battle of the Systems. By Jonathon Story and Ingo Walter. Cambridge, MA: MIT Press, 1997. 337p. \$25.00 paper.

Aspects of European Monetary Integration: The Politics of Convergence. By Alison M. S. Watson. New York: St. Martin's, 1997. 226p. \$29.95 paper.

Richard Deeg, Temple University

How can one explain the emergence and consequences of exchange rate cooperation and monetary union in the European Union? This, very broadly, is the question that unites these four books

Story and Walter provide a highly detailed and exhaustively documented historical account of "the politics and diplomacy surrounding the EU's financial services area, exchange-rate regimes, and repeated attempts to make a big leap to monetary union" (p. 105). The first three chapters review the history of negotiations over the European financial services area (i.e., the creation of a single market in financial services) and monetary union politics in Europe, including efforts at exchange rate coordination and the macroeconomic imperatives associated with this. In my as-

sessment, the key point of the book lies buried in the middle of chapter 2 (p. 42): It can be deduced from macroeconomic theories that once European states committed themselves to fixed exchange rates (essentially starting with the European Monetary System in 1979) the logical implication is much deeper economic and political union. The authors marshal a great wealth of detail to show how this dynamic has played out and that although the ultimate outcome (deeper union) is predictable, getting there is anything but simple, as member states, financial market actors, and central banks fight for their divergent interests.

To the credit of Story and Walter, they view monetary and exchange rate cooperation as part and parcel of the much broader process of integrating financial markets in Europe and, ultimately, the broad political project of European integration. Thus, chapters 4–8 are devoted to explaining the distinctive national financial market models—and, by extension, distinctive brands of capitalism—that exist in Europe (with separate chapters on France, Germany, and the United Kingdom). For Story and Walter the politics of financial market and monetary integration, especially from the mid-1980s forward, are driven largely by the divergent structural interests associated with each of these models. Yet, as integration proceeds, they foresee inevitable convergence among them.

One strength of *Political Economy* is that it illustrates the complex issue linkages—security, economic, political, and social—involved in negotiating the path to further integration. Its chief weakness is that it does so (mostly) as a straight historical narrative largely bereft of an explicit theory of integration or international cooperation. More troubling, however, is that the book often gets lost in its own detail, and the logic of its organization is never really transparent until the end. Indeed, I recommend that a reader digest chapter 10 and the Epilogue first, as it is there that the link between the historical account and central analytical themes is finally conveyed.

Watson's book uses various international political economic and macroeconomic theories, as well as public choice models of politics, to analyze the pressures in favor of economic and monetary union (EMU) and the specific economic and political challenges member states face in realizing and sustaining it. The first three chapters provide an historical overview of monetary integration efforts, starting with the largely failed Snake of the early 1970s through EMU in the 1990s. Watson portrays the drive to monetary integration as the result of numerous, mutually reinforcing economic and political forces, ranging from the growth of international capital mobility to domestic political interests (e.g., taming inflation) to geopolitical imperatives (e.g., binding reunified Germany to the integration project). As do Story and Walter, Watson takes the position that monetary and exchange rate cooperation are inextricably linked to the single-market project and especially financial market integration in Europe, that is, progress in all these areas must more or less go together (p. 51).

Watson's primary subjects of analysis, however, are the convergence criteria laid down in the Maastricht Treaty, which were to be met before any member state could join EMU. Watson is particularly interested in analyzing the economic and political rationale behind the selected convergence criteria, that is, which theoretical arguments were, or could be, used to argue the necessity for creating them. She also analyzes the respective difficulties member states are likely to encounter in meeting them as a result of divergent domestic institutions and the implications of these criteria for domestic politics and policies. In separate chapters she does

this analysis for each of the convergence criteria—price stability, budgetary conditions, and interest rate and exchange rate convergence.

Ultimately, Watson aims not so much at making an independent theoretical argument that explains the why and how of EMU as at reviewing established theories and models and employing them to elucidate the causes and consequences of EMU. The character of the work is more that of a textbook, particularly since the author devotes significant portions of numerous chapters to reviewing various theoretical models and literature before applying them. For readers particularly interested in EMU and the convergence criteria, this is a useful book. For readers who are looking for more innovative uses of theory and provocative analysis of exchange rate cooperation and monetary integration, read on.

Why would states give up their national currencies, a key component of sovereignty? Why would political actors from both the Left and Right agree to give up monetary policy autonomy? These are the leading questions McNamara sets for herself in analyzing three episodes of monetary cooperation in Europe: the Bretton Woods system, the Snake from the early 1970s, and the European Monetary System (EMS). In setting up her own model, McNamara first reviews four theories of international cooperation-hegemonic stability theory, business cycles and cooperation, institutional asymmetry, and interest group theories—that could be used to explain the creation and success/failure of each of these three historical cases. She finds deficiencies in all these theories for explaining the Snake and EMS, the two purely European efforts at exchange rate cooperation. McNamara then constructs her alternative theory, which she builds upon Mundell's "Holy Trinity": "Policymakers can choose only two out of three policy options at any one time: free capital flows, a fixed exchange rate, and monetary policy autonomy" (p. 44). McNamara argues that rising international capital mobility, starting in the late 1960s, forced European policymakers to choose between the two remaining options: pursuing a fixed exchange rate system with other European states or preserving domestic monetary policy autonomy.

From this position McNamara argues that the Snake failed because policymakers tried to eat their cake and have it too, that is, maintain fixed exchange rates while pursuing independent (and divergent) monetary policies in a world of rapidly increasing capital mobility. When push came to shove, European policymakers abandoned any real commitment to fixed exchange rates. The EMS, in contrast, was quite successful. The difference this time was that policymakers finally recognized that high capital mobility made the cost of "squaring" Mundell's "trinity" prohibitive. But they could still choose between a fixed exchange rate regime and monetary policy autonomy, and they chose the former. Why? The answer, for McNamara, is the power of ideas. European policymakers went through a learning process in which they first had to accept that their cherished Keynesianism would no longer work. But they needed a policy paradigm with which to analyze the world, and monetarism, buoyed by the successful example and proselytizing of Germany, became their beacon in the night. In the late 1970s and early 1980s policymakers across Europe gradually converged around a monetarist, low-inflation macroeconomic policy model. But this policy convergence does not itself explain the decision to abandon independent monetary policy for EMS. McNamara argues that policymakers were committed to fixed exchange rates because they considered it important for facilitating intra-European trade and reducing the complexity and cost of administering the Common Agricultural Policy. Mc-Namara closes with a brief analysis of EMU, which she sees

as a natural progression from EMS, especially in light of ever greater international capital mobility.

McNamara's book is tightly and cogently organized. The narrative is well written, and theories are lucidly presented and analyzed. The author adopts the capital mobility hypothesis (which argues that rising capital mobility forces policymakers to pursue convergent monetary policies), but she fills in some of its gaps in arguing that, although capital mobility may create a structural constraint upon state behavior, states still have choices as long as they are willing to pay the price. McNamara's solution—and important theoretical contribution—is grafting the capital mobility thesis onto an argument about the importance of ideas and how policymakers come to their decisions within these constraints. In these respects the book is triumphant. But there are some significant soft spots, and I note two here.

First, McNamara essentially takes the position that domestic interest groups do not matter all that much when explaining policymakers' decisions about exchange rate cooperation in Europe (p. 59). But given the fact that exchange rate policy is tightly linked to monetary (interest rate) policy, about which domestic interests undoubtedly care a lot, and that there are many empirical and theoretical arguments to the contrary (Oatley, to whom I turn next, for example), she needed to bring forth much greater empirical data to support this position.

Second, McNamara makes a plausible argument that monetarism became the new policy paradigm across Europe, but given the centrality of this to her argument, she devotes relatively little detail to tracing out the process by which these ideas infiltrated policymakers' thinking and determined their decisions. This is all the more important because others (such as Oatley or Story and Walter) suggest that European policymakers, once they committed themselves to fixed exchange rates, had little choice but to toe the line of monetary rigor drawn by the Bundesbank. This was not so much because of monetarist convictions (which they may have indeed held) as because the Bundesbank was the actor that captured the exchange rate system's single degree of freedom, that is, it set the monetary policy for everyone: The Bundesbank was the tail that wagged the European dog. McNamara chooses to portray the Bundesbank's influence as deriving from its success in persuading other European policymakers of the correctness of its economic philosophy, not from the exertion of real hegemonic power. But the Bundesbank was not above using implicit but unmistakable threats (i.e., coercive power) to bring down any exchange rate system that did not meet its demands (see Oatley, p. 63).

Oatley deftly welds together economic and public choice theories, statistical analysis, and historical case studies to produce a sophisticated and superb book in which he addresses two primary questions. First, how can one explain the creation and evolution of European exchange rate institutions? Second, how can one explain the significant cross-national and longitudinal variation in European policymakers' ability to stabilize exchange rates within such institutions?

In answering the first question, Oatley juxtaposes his own explanation with neoliberal institutionalism, which explains international institutions as arising in order to realize gains from cooperation. He critiques this as a functionalist argument that therefore inadequately explains the motives driving the creation and evolution of such institutions. The theory also neglects the distributive elements embodied in international institutions. Without completely rejecting neoliberal institutionalism, Oatley uses public choice theory to develop an alternative model resting on the premise that "explaining the creation and evolution of exchange rate institutions and

variation in policymaker's ability to stabilize exchanges rates within these institutions requires domestic politics-based models of monetary policy and models of [international] bargaining power" (p. 4). In neoliberal institutionalism, institutional evolution is driven by the quest for additional gains from cooperation. In Oatley's model, it is driven by the redistribution of existing benefits as joint gains from cooperation erode (p. 181). Thus, the EMS was initiated by Helmut Schmidt and Valéry Giscard d'Estaing to assist each of them in advancing their respective (and distinctive) domestic agendas, and changes in EMS institutions were driven by actors' attempts to shift the costs of cooperation and/or increase their benefits from cooperation.

In answering the second question, Oatley juxtaposes his model against the capital mobility hypothesis (CMH). Echoing McNamara, Oatley notes that the CMH does not say why policymakers chose fixed exchange rates rather than monetary autonomy and why they converged around the Bundesbank's preferred standard. But in filling these gaps, he diverges from McNamara. Using European cases, Oatley conducts a statistical test of the CMH against his domesticpolitics based model of monetary policy. He finds little support for the CMH and significant support for his model, with the exception of the French and Italian cases, where Left-leaning governments pursued monetary restriction, counter to the predictions of his model. Oatley ultimately saves his model by examining the French and Italian policy shifts in the 1980s through extensive case study analysis. McNamara explains this shift by arguing that these leftist governments became monetarist converts, but Oatley relies more on the achievement of coalitional stability, the ascendance of proprice-stability factions within dominant parties, and increased central bank independence to explain the policy shifts in France and Italy. It was not capital mobility that forced policy change—indeed, Oatley argues that policymakers in both countries limited capital mobility for much of the 1980s—but changes in domestic politics. Moreover, pragmatic policymakers in both countries used the EMS institutions to help them loosen domestic institutional barriers to a tight money policy and fiscal austerity. Only after these policymakers prevailed in the domestic arena (after 1983) could there be convergence in monetary policies and nominal exchange rate stability in EMS.

A criticism I have of both McNamara and (to a lesser extent) Oatley is that their models do not attempt to endogenize and systematically assess the influence of a broader and more general commitment by European political leaders to advancing European political integration, or how decisions in other policy areas, such as financial market integration, affect decisions about monetary policy and exchange rate cooperation. For example, the French decision to remain within the EMS in 1983 rather than pursue an independent economic strategy may well have been more-or at least as much—a result of a decision not to sacrifice the greater European project than a decision for the policies and politics of austerity (or monetarism). This connection between the broader political commitment to integration and specific decisions that constitute integration is brought out clearly by Story and Walter and Watson in their analyses.

Altogether, these four books provide a wealth of empirical and theoretical material on exchange rate cooperation and monetary union in Europe. If one could read only one of them, Oatley's book is the clear choice. Yet, I hasten to add that reading McNamara and Oatley in tandem yields a stimulating theoretical discourse. Finally, although all these books were completed when the move to full EMU was still

uncertain, much of their analyses is still very useful for understanding the current developments and future of EMU.

National Self-Determination and Secession. Edited by Margaret Moore. New York: Oxford University Press, 1998. 285p. \$65.00.

Stephen M. Saideman, Texas Tech University

Yugoslavia's violent disintegration revealed that self-determination is difficult to apply in practice. Because the right of self-determination applied only to colonial territories (narrowly defined) from the end of World War II to the end of the Cold War, the complexity of self-determination was overlooked until recently. This book presents a debate about whether and which groups should be allowed to secede.

Margaret Moore introduces the book by delineating three lines of thought: just cause theories, choice theories, and theories of national self-determination. The first half of the book presents these ideas, and the last three chapters are more empirical. One striking omission (although the last three contributions come close) is a defense of the old conventional wisdom that the right of states to their territorial integrity is superior to a group's right to self-determination.

Ironically, Allen Buchanan's book, Secession: The Morality of Political Divorce from Fort Sumter to Lithuania and Quebec, 1991, which has set the terms of the debate, was a challenge to the conventional wisdom of the time, as it seemed to grant more support for secession than previously allowed. Yet, in subsequent debates and in this volume, he presents the most restrictive view of the right to self-determination. Buchanan argues that the right to secede is a remedial right, like the right to revolution. A group should be allowed to secede only if it faces persistent violations of human rights or had been previously free. Of course, how one determines whether either condition exists is quite difficult. Furthermore, Buchanan is quite clear in arguing that no groups deserve support if they are seceding from a legitimate democracy (p. 17). Thus, the Quebec separatists do not have a legitimate case, according to Buchanan. Canadians outside Quebec would agree with his key point that easy terms of secession would make it possible for a minority to blackmail the majority.

Wayne Norman supports Buchanan's restrictive view by arguing that a less restrictive right to self-determination would encourage more secession, which would provoke more violence. This assumption, present in many of the other chapters, stands on perhaps the weakest empirical grounds. Events in East Timor may have encouraged the Acehnese to intensify their efforts to become independent because they were both responding to the same pivotal actor, the government of Indonesia, but East Timor matters less for groups in other states (David Lake and Donald Rothchild, *The International Spread of Ethnic Conflict*, 1998). Norman seems to be on better ground when arguing that a restrictive right to self-determination would improve the plight of groups by providing states incentives to be less oppressive (p. 44).

Kai Nielsen argues (as does David Miller in a separate chapter) that nations are entitled to self-determination and that it would be best for each nation to have its own state, converting Ernest Gellner's (Nations and Nationalism, 1983) definition of nationalism (a nation is a group that seeks to have its own state) into a right. Nielsen maintains that this right to secession is based on the individual right to autonomy that liberalism values. Interestingly, Nielsen applies his analysis only to advanced liberal democracies as a direct challenge to Buchanan. Nielsen qualifies his argument by suggesting that ethnic nationalism is to be condemned but liberal

nationalism is to be celebrated and rewarded because it is not exclusivist (by definition). Liberal nationalism in a liberal democracy aims to protect a culture of a nation but will, according to Nielsen, not harm the cultures of others. Since ethnic nationalism tends to spawn more secessionist movements (it is not clear whether liberal nationalism exists in reality), it is not clear whether Nielsen's argument can be applied.

Daniel Philpott largely ignores nationalism in his argument for an equally general right to secession. He views self-determination as a form of democracy. We ought to take senously (by analyzing) his argument that allowing secession to take place may result in less violence than in denying groups secession. He ultimately maintains that his views would not condone many more secessionist efforts than Buchanan's, which raises more questions than it answers.

The other contributions to the volume raise important questions about many of the assumptions in the previous chapters. Moore emphasizes the territorial nature of secession but believes that boundaries by themselves do not or ought not to have moral force. Beiner challenges the liberal basis of the previous chapters by arguing that a focus on rights increases conflict, since rights are about the creation and enforcement of boundaries between individuals (p. 162). John McGarry focuses on postsecession politics for both minorities and majorities in the new state, arguing that secession is likely to solve very little.

Donald Horowitz's contribution challenges the more permissive views of self-determination. It raises a crucial problem that most of the authors overlook or downplay (e.g., Nielsen, p. 123), namely, that in any society, multiple identities coexist, and the salience of each depends on the political context. Consequently, to say that one nation is deserving of self-determination privileges one identity at the expense others, which may reemerge in the new state and/or in the rump state. This not only is likely to lead to conflict after the secession but also raises a critical issue that this book was supposed to address: Who is the self that is determining its future?

Finally, Rogers Brubaker, in a piece written for another book, argues that nationalism has been misconceived. There are two problems with this chapter. First, only part of it fits with the rest of the volume—when he argues that there is no one-size-fits-all solution to ethnic conflict. Second, it does not serve well as a conclusion to the volume. Although the book should not end with a definitive stance on the issues (that is not its purpose), the final chapter should have suggested where the debate ought to go in the future.

This book provides an excellent summary of a very lively debate. It would be useful in upper division undergraduate courses and graduate courses in political theory, both for its debate about rights and for its application of liberal and communitarian theory to live, on-going, policy-relevant issues. For that reason alone, this book also would be of value to courses in ethnic conflict.

The Choice for Europe: Social Purpose and State Power from Messina to Maastricht. By Andrew Moravesik. Ithaca, NY: Cornell University Press, 1998. 514p. \$59.95 cloth, \$22.50 paper.

Jeffrey J. Anderson, Brown University

Andrew Moravcsik addresses a central puzzle in the study of European integration: Why have sovereign governments "chosen repeatedly to coordinate their core economic policies and surrender sovereign prerogatives within an international institution" (p. 1)? The resulting uniqueness of the European Community (EC) demands a convincing explana-

tion. The conventional wisdom, Moravcsik argues, has accorded far too much weight to geopolitics, such as the goal of binding Germany to the West or the pursuit of a federal vision of Europe, in shaping the development of the European project since the 1950s. Supranational actors also have received undue credit for driving the integration process and directing its institutionalization. The reality is quite different. The EC emerged as the result of rational decisions made by member governments in pursuit of core economic interests. Over the course of forty years, choices for Europe crystallized not because of supranational influence but from the relative bargaining power of the three largest member states: France, Germany, and Britain. European institutional arrangements were designed to enhance the credibility of commitments among governments, not to achieve a federal Europe or to realize procedural efficiency.

To support these revisionist claims, Moravosik presents a "rationalist framework" (p. 19), an eclectic construction assembled from diverse schools in comparative politics, international political economy, and international relations. He examines five grand bargains, starting with the Treaty of Rome and concluding with the Maastricht Treaty, and he parses each into three temporal-analytical stages: preference formation, interstate bargaining, and implementation. Attached to each stage is a set of competing hypotheses: for stage one, the relative importance of economic versus geopolitical interests; for stage two, the relative importance of national versus supranational actors; for stage three, the relative importance of credible commitments versus an assortment of considerations, including Europeanism and technocratic efficiency.

Moravesik maintains that his aim "is not to close the debate over the fundamental causes of European integration but to renew it" (p. 85). The ensuing analysis, in which methodological rigor is a running theme (more on that below) and conventional wisdoms are swept away time and again, leaves little doubt that he is trying to accomplish both simultaneously. His innovative framework is surely worth exploring further. Although Moravesik succeeds in shifting the theoretical debate away from the stale reference points of the EC studies literature toward general social science concerns, his approach raises many questions. Is it analytically sound, for example, to separate preference formation from bargaining, or bargaining from implementation, given the potential for interactive effects? The structured narratives in this book hint at such recursiveness, but clearly a more extensive discussion is warranted. The author also provides new grist for a mill constructed in the early 1990s—the debate over the relative importance of the European Commission and the member states in advancing integration. Moravesik was a protagonist in this scholarly exchange, and his critics will have their hands full trying to rebut the new evidence suggesting that governments rule the European roost

The author's most revisionist claim—national economic interests are the main motor of integration—is also the least convincing. Few contemporary observers of the European Union will be surprised to learn that difficult negotiations over the Common Market, economic and monetary union (EMU), and other economic initiatives were the occasion for intense lobbying by domestic producer groups advancing not geopolitical but economic interests, and that the resulting interstate bargains represented carefully crafted compromises over matters of economic substance. In short, coordinating "core economic policies" (p. 1) tends to call forth economic interests, economic disagreements, and economic compromises.

Moravcsik admits as much in the opening chapter, and were that the sum total of his argument, it would be unlikely to generate much controversy. What gives the thesis bite is that he consistently relegates geopolitical interests to the causal sidelines: They influence bargaining outcomes on the margins and only within parameters set by the primacy of economic interests. What is extraordinary—and here one must tip one's hat to Moravcsik for faithfully presenting the data—is that these marginal effects invariably impart true distinctiveness to the European project, that is, why it emerged not as a simple free trade area but something more, and why its supranational components have been augmented and strengthened over the years. Indeed, Moravcsik acknowledges the special nature of geopolitical effects but downplays their significance. This is odd, given the book's central puzzle.

Minimizing the import of geopolitics often takes on needlessly provocative dimensions. Many readers will be puzzled by Moravcsik's portrayal of national leaders, including Charles De Gaulle, who behaves more like the French Minister of Agriculture than president of the Republic. His conclusion that German unification had no effect on the Maastricht Treaty negotiations is frankly incredible. True, EMU was on the agenda at least two years before the Berlin Wall came crashing down, and the German chancellery and Foreign Ministry supported the initiative. At no time during this period, however, did the Germans commit to a firm deadline for the introduction of a single currency, despite the entreaties of EC partners and commission officials. Unification rendered Germany's qualified—perhaps even empty commitment to EMU unsustainable. For geopolitical reasons, Helmut Kohl brushed aside the concerns of the Bundesbank and Finance Ministry, much as he did in 1990 over the terms of German economic and monetary union, and committed his country to a firm timetable. Moravesik's narrative disregards this crucial shift in the German stance on EMU.

In the realm of method, Moravcsik makes a very strong claim about the data that underpin his analysis. In contrast to previous studies of integration, which relied on secondary literature and "soft" primary sources (memoirs, official press releases, interviews, and the like), which are inherently prone to bias or distortion, this book is based overwhelmingly on "hard" primary sources. Moravesik concedes that in many instances, because internal government documents are still under lock and key, he is forced onto softer ground; nevertheless, he assures the reader that only facts gleaned directly from "hard" primary sources are extracted from the secondary literature, not interpretations or conclusions of the author in question. All well and good. Yet, on closer examination, there is little to distinguish this book from other monographs that attempt to test theory by means of qualitative empirical analysis. In five case study chapters spanning 386 pages, there are 917 footnotes altogether, about 2% contain references to internal government documents, the hardest of "hard" primary sources. Secondary sources, not to mention political memoirs and other soft primary sources, make up the remaining 98%. Moravcsik also relies frequently on open-ended interviews he conducted during the 1990s. The reader is left to wonder why his subjects, who include acting and former prime ministers, a European Commission president, and other high-ranking officials, would be less inclined than those quizzed by other scholars to put a favorable spin on events in response to questions posed by someone whose work was sure to be widely read.

This aspect of the book's methods would be a minor matter were it not given such emphasis by the author and were it not used to dismiss entire bodies of scholarship, such as historical institutionalism (p. 491), for relying too much on secondary sources. It also leads Moravcsik to questionable conclusions; for example, he quotes an internal German government document that appears in Timothy Garton Ash, In Europe's Name (1993), to support his contention that economic interests drove Helmut Schmidt's approach to monetary cooperation in the 1970s (p. 254). Nowhere mentioned is Garton Ash's subsequent observation that the verb employed in the German text (abdecken) is also strongly suggestive of parallel and reinforcing geopolitical motivations. This simply underscores that there is no such thing as an unvarnished fact, particularly where the written word is concerned, and that scholars have an admittedly difficult obligation to grapple with both facts and interpretations drawn from all manner of sources if they hope to arrive at a complete picture.

In the summary chapter, Moravesik describes the European Union as "an institution so firmly grounded in the core interests of national governments that it occupies a permanent position at the heart of the European political landscape. Therein lies the political achievement and the ongoing social-scientific puzzle" (p. 501). He is surely right. One can only hope that EC scholars, in responding to his invitation to join a renewed debate, will choose to focus on real issues of substance, of which there are many in this volume, and not be distracted by contentious overstatements, of which there are, alas, many here as well.

Power Ties: Economic Interdependence, Balancing, and War. By Paul A. Papayoanou. Ann Arbor: University of Michigan Press, 1999. 193p. \$50.00.

Mark R. Brawley, McGill University

In recent years, scholars have aimed to gain better insight into international relations by incorporating material from various subfields. This can be seen in the melding of subjects within security studies and international political economy; it can also be observed in the closer attention paid to causal arguments drawn from several different levels of analysis. Taking up these challenges simultaneously has been difficult, and often the resulting works fail on one or more counts. Often, important themes clash within the text. In *Power Ties*, Paul Papayoanou has taken up these tasks and masterfully handles the various elements. The book stands as an excellent example of the sort of work of which we should expect to see more in the future.

Papayoanou takes on a classic theory in international relations: balance of power. As he notes, this well-known and well-worn phrase covers a number of very different arguments; he selects as principal targets for criticism the versions presented by Realists such as Kenneth Waltz (and the later reformulation by Steven Walt into a theory of states balancing against threats). The criticisms raised, however, are more in the nature of a call for better specification of expected results and for recognizing contextual and situational variants. It is on these same grounds that Papayoanou then tries to redress the weaknesses of existing theories.

As the author's criticisms suggest, remedies can be found in elaborating the different factors that affect how status quo powers react to the threats posed by revisionist states and in clarifying the factors that encourage revisionist states to pursue an alteration of the international status quo. Such factors can be found in both the systemic and domestic political and economic spheres. What is innovative about Papayoanou's approach is the way in which the logic of signaling games is employed to organize and integrate the material from different traditions and theoretical approaches.

Yet, those who try to incorporate theories based on different assumptions face a barrier. How does one integrate the theories without undermining each element's coherence or meaning?

Papayoanou overcomes this barrier by using the logic of signaling (from game theory) to pull together material from different levels and traditions. He argues that leaders of status quo powers choose their strategy for dealing with revisionist challenges by calculating the effect of extensive economic ties with other status quo states or with revisionist states. The degree of ties between the status quo state and other defenders of the status quo shapes not only the leaders' perceptions of domestic support for an alliance but also the credibility of that alliance in the views of the potential ally and the revisionist power. This notion allows Papayoanou to bring together some existing arguments about economic interdependence deterring war and domestic politics as a constraint against playing power politics (as well as on decision making more generally), although in a fashion that respects the fundamental insights of Realism on great power alignments.

This integration does not come without a price, however. Because of the emphasis on perceptions of economic ties between states, the key evidence Papayoanou needs to present is difficult to grasp. It is not evidence of actual links per se, or even of public opinion on the links between economies, but rather the views of key decision makers concerning the public's views of ties between economies. Moreover, Papayoanou must show that such factors have played a key role in the behavior of particular states. A small amount of evidence carries much of the argument.

Nevertheless, the organization of the evidence shows off the strength of Papayoanou's views. The author first lays out the questions concerning a particular historical period, then presents the standard historiography for that period. That is followed by the typical interpretations found in the political science literature. This highlights the disjunctures between the two disciplines, a disjuncture that is understandable but uncomfortable for political scientists. The difference in interpretation is understandable given our different endeavors: Historians emphasize the uniqueness of events, whereas political scientists stress generalizable aspects of cases. Nonetheless, historians' detailed descriptions should prove useful for political scientists. Too often, however, details are lost, and historians routinely scold political scientists for running rough-shod over evidence. Papayoanou illustrates how a sophisticated argument can reconcile the differences between the historians' narratives and those generated by political science theories. The argument is sophisticated but not overly complex, so that it retains its applicability across a wide range of cases.

There are only a few areas in which the book could have been improved, and perhaps organization of the cases is the most significant. These are grouped historically (which is very helpful for appreciating the variance in views between historians and political scientists), but it might have been more useful to group cases together on a different basis. For instance, the model is supposed to give us insight into the actions of the revisionist powers, but the patterns there are not easily discerned because these cases are not presented together. It is easier to see the similarities in the strategies of the status quo democracies because these form the focal point of each chapter.

All in all, *Power Ties* is an excellent example of the sort of work we all should be aiming to complete: It addresses shortcomings in existing theories by complementing those arguments with insights from other theoretical traditions.

More important, this refinement is achieved by integrating the elements into a single logically coherent model. The book should be widely read within the field, both for its findings and for the manner in which Papayoanou resolves some of the pressing difficulties we all face in improving the generation, testing, and refining of theories in international relations.

Democracy at the Point of Bayonets. By Mark Peceny. University Park: Pennsylvania State University Press, 1999. 254p. \$55.00 cloth, \$18.95 paper.

Mi Yung Yoon, Hanover College

Although promoting democracy throughout the world has been a goal of American foreign policy in the twentieth century, U.S. pursuit of this goal has been inconsistent. Mark Peceny questions why the United States has chosen to adopt proliberalization policies, defined as the combination of support for free and fair elections and other efforts for liberal reforms, during its military intervention on some occasions but not on other occasions. He argues that, given the increasing significance of democracy promotion in American foreign policy, understanding this puzzle is important in its own terms (p. 3). To unravel this puzzle, he examines the record of U.S. efforts to promote democracy during military interventions using both quantitative analyses of those interventions in the twentieth century and case studies based on secondary sources and documentary evidence (p. 6). Specifically, he questions under what conditions the United States decides to promote democracy during its military interventions. Peceny uses realism and America's liberalism, two of the most frequently tested theories of international relations, as theoretical tools and examines which of these two plays the most significant role in shaping decisions.

The book has eight chapters, each of which begins with a brief introduction and ends with a nice summary. Following a short introductory chapter, chapter 2 quantitatively tests the relative effect of realist and domestic liberal variables on presidential choices during the military interventions. Peceny finds that the realist concerns provide the best explanation for the initial choices of presidents, whereas congressional pressure from the domestic liberal approach best explains their final choices (p. 45). In the four subsequent chapters, these findings are further examined through six case studies. These explain with clarity how the partisan dynamics in Congress shape the legislature's reaction to the intervention policies of presidents. Chapter 3 discusses U.S. interventions in Cuba and the Philippines after the Spanish-American War. Peceny states that liberal pressure from Congress against McKinley's nonliberalization policies in both countries compelled him to shift his initial decision to proliberalization policies in Cuba, but it failed to do so in the Philippines because of the warfare there and the dominance of the president's party in Congress after the new elections. The next chapter examines Kennedy's policies toward South Vietnam. Peceny argues that "the commitment to democracy was contingent on security considerations" during his administration (p. 92). Kennedy initially adopted prodemocracy policies toward Vietnam to serve U.S. security interests, but he abandoned these policies when the security situation in Vietnam appeared to warrant it. When Kennedy shifted to nonliberalization policies, liberal pressure from Congress was nonexistent or barely visible because liberal Democrats did not want to challenge their party's leader (p. 99).

Chapter 5 investigates U.S. military intervention in El Salvador during Reagan's first term, specifically, Reagan's policy shift from hard-line to proliberalization policies.

Peceny argues that realist considerations explain Reagan's initial policy choices, but congressional pressure reinforced by antiintervention groups provides a good explanation for the shift. This chapter reemphasizes the strength of domestic liberalism over realism in explaining final presidential choices. It is followed by a chapter on Clinton's proliberalization policies during U.S. military interventions in Haiti and Bosnia. Peceny argues that the end of the Cold War makes the present era the most conducive in U.S. history to the adoption of proliberalization policies, and "both the realist and the domestic liberal arguments are pushing in the same direction" (p. 8). Yet, he points out, U.S. support for proliberalization policies was limited in both cases because Congress and the public were reluctant to pay high costs or to use force.

Chapter 7 answers the "so what?" question by examining whether U.S. military intervention and proliberalization policies actually lead to successful democratization in the target state. Using multivariate analyses, Peceny finds that "the promotion of proliberalization policies during a military intervention rather than intervention itself has the most powerful impact on democratization in target states" (p. 199). The concluding chapter is not much more than a summary of the findings. Throughout the book, Peceny argues that when a president adopts proliberalization policies in reaction to congressional pressure, he does so to build domestic consensus for intervention. Thus, promotion of democracy has been used as an ideological tool for legitimizing U.S. military interventions.

The book is clearly written and well organized. By combining both quantitative analyses and case studies, Peceny presents the strengths that each approach can offer. The two quantitative chapters report robust findings of multivariate analyses, and the case studies and footnotes provide a wealth of detailed information. By focusing on presidents' policies during military interventions and examining the effect of proliberalization policies on target states, Peceny brings much freshness to the literature, which has focused on the factors of intervention.

The book is not without its shortcomings. Chapter 2 needs more thorough and expanded discussion on realism and America's liberalism to relate them to the realist and domestic liberal arguments on proliberalization and nonproliberalization policies. By drawing indirect military intervention cases from a single source, Peceny excludes some cases that could be qualified as indirect military interventions (e.g., Pakistan in 1971, Mozambique in the mid-1980s, Somalia in 1988). A table listing durations in days and months as well as years would have been useful, because they determine many of the findings of this study. To examine whether presidents' policies changed during an intervention, Peceny coded them "as proliberalization or nonliberalization in the first and last two months of their administration's policies during an intervention, or in the first and last week of interventions of shorter duration" (p. 19). This is an impressive accomplishment, since determining the beginning and ending dates of each military intervention is a challenging task. Conflicting dates are reported in the literature, and it is often difficult to know the dates for indirect military interventions because of their subtleness.

With no clear justification, Peceny argues that "the presidential selection process places limited constraints on presidents when it comes to the adoption of proliberalization policies during military interventions" (p. 28). Yet, a few of the case studies, especially Vietnam during the Kennedy administration, suggest that the presidential election, a statistically significant variable in other studies of U.S. military

intervention or use of force, may have played a larger role in shaping decisions than Peceny credits it. Therefore, I question whether the models in chapter 2 should include the presidential election as an independent variable.

The above comments should not outweigh the overall quality of the book. It is well written and provides an excellent account of the continuity and change in U.S. policies during military interventions by integrating the realist and domestic liberal approaches. I highly recommend this work to scholars and to advanced undergraduate and graduate students interested in U.S. military intervention and democratization.

The Myth of Global Chaos. By Yahya Sadowski. Washington, DC: Brookings Institution Press, 1998. 267p. \$28.50.

James H. Mittelman, American University

Yahya Sadowski, a professor at the Johns Hopkins School of Advanced International Studies, tackles a key problem in world order studies: Following the Cold War, what are the interactions between globalization and the outbreak of violence? And how can a sound diagnosis of this set of relationships inform the corpus of knowledge on conflict analysis and U.S. foreign policymaking? To get at these questions, he proceeds logically by setting forth the premises of chaos theory, critically assessing it through the use of qualitative and quantitative data, and drawing out the implications for political strategists.

According to Sadowski, "global chaos theory is an intellectually elegant construct, built on well-established traditions. It offered Americans a sense of direction, when the end of the cold war had sowed confusion and division" (p. 75). The theorists' core argument is that globalization spawns political violence, manifested in a rise of clashes, mainly explosions of cultural pluralism. They share a common view that societies in the grip of economic globalization experience jarring social disruptions and are, thus, prone to anomie, which in the Durkheimian sense depicts a condition of irrational violence and a shift, or in some cases collapse, of values. A disparate group, chaos theorists fall into different subschools in terms of their respective emphases: the collision of cultures worldwide, the role of economic forces as propellants of chaos, and "ethnonationalism" or "religious nationalism." For the most part, however, the purveyors of this theory are popularizers of its tenets. The proponents include leading journalists, such as Robert Kaplan; management gurus, such as John Naisbitt; futurists similar to the Tofflers; politicians, including Senator Daniel Patrick Moynihan; and scholars with broad appeal, Samuel Huntington being perhaps the best known among them. Tracing the chaos theses in diverse contexts—such as the Horn of Africa, central Africa, and the Balkans-Sadowski wends gingerly through a minefield of debates about the root causes of ethnic, religious, and intercivilizational

As the title of his book suggests, Sadowski finds that chaos theory cannot stand the test of evidence. Finely grained case studies and what he calls "back-of-the envelope" measures of globalization are, in an effort to detect correlations, compared with the frequency and range of culture strife. He lambasts Huntington, taking cognizance of various critiques of the "clash-of-civilizations" thesis, and pokes holes in global chaos theory, as propounded by Kaplan and other publicists. Notwithstanding its flaws, this way of thinking has had considerable influence in Washington, mainly as a series of epigrams that are used in sound bites, not as a vision or grand strategy. In conclusion, Sadowski presents his own recom-

mendations for U.S. foreign policy: greater flexibility, better experts, skillful diplomacy, military preparedness (not necessarily expanded budgets), and more support for economic development. These are conventional, decidedly less than novel, proposals.

Yet, the book makes an important statement insofar as it focuses on values and relates an intersubjective framework to political and economic conditions. This endeavor is valuable for readers who seek to understand the mindset in Washington and, more broadly, among U.S. foreign policy intellectuals. The book's virtues, however, are also its vices. An international audience is likely to regard this discussion of the views of predominantly U.S. writers as provincial, if not arrogant, inasmuch as it ignores seminal contributions emanating from countries whose structural positions in the global political economy differ from that of the United States. If Sadowski is interested in global chaos, why did he not decenter the debate and pitch it widely in terms of global scholarship, such as the well-known thesis on the "new medievalism," advanced as a globalization scenario by the late Susan Strange ("The Defective State," Daedalus 124, no. 2 [Spring 1995]: 55-75) in the United Kingdom, picking up on a seminal concept introduced by the Australian Hedley Bull (The Anarchical Society: A Study of Order in World Politics, 1977)? Strange considered the discourse among analysts in and around Washington without granting it the elevated status of "theory." Like other political economists, she knew its varied dimensions but in terms of "the Washington consensus," that is, the geopolitics and geoeconomics of neoliberalism, on which there is vast scholarship, a route that Sadowski chooses not to travel.

To be sure, Sadowski's subject is the *métier* of political economists, but the author does not venture far into this realm. Although the classical political economists put forth a sophisticated notion of social structure—Smith and Ricardo, as well as Marx, regarded class as a key factor—Sadowski's probings of social structure delve into ethnicity, religion, and gender, which are pivotal for explaining anomic violence, and barely touch on class forces.

In going much beyond the chronicling of manifestations of global chaos, and in searching for underlying factors, the author is right to attack purveyors of the notion of chaos. In my view, it is a catchall, too vague as a category and too blunt as a tool of analysis. The question then becomes: In this grab-bag, exactly what is chaos, and what links very different phenomena? Sadowski is, moreover, on the right track when he introduces the concept of globalization, but it is undertheorized in *The Myth of Global Chaos*. To add structural depth to this book, it would be worth considering the varied genres of globalization research and the positions of globalization theorists, who have examined the implications of a radically new division of space and a compression of time in terms of a lessening of social and political control over economic forces—in short, a global transformation of ways of life.

Apart from the silences noted above and the need to deepen the structural aspects of the analysis, *The Myth of Global Chaos* constitutes a lucid study of what is driving the major conflicts after the Cold War, and it pushes the debate about ways to understand them. All in all, Sadowski's engaging and ambitious work offers several salient formulations that challenge U.S. foreign policymakers and all those concerned with the dynamics of violence in a globalizing world.

States, Firms, and Power: Successful Sanctions in United States Foreign Policy. By George Shambaugh. Albany: State University of New York Press, 1999. 248p. \$21.95.

Daniel W. Drezner, University of Chicago

In the past decade, the American use of economic coercion has increased dramatically. Among the cases that have garnered the most attention are extraterritorial or secondary sanctions, such as the Helms-Burton Act and Iran-Libya Sanctions Act. These measures punish firms and individuals, rather than entire nations, for trading with countries embargoed by the United States. These sanctions pose a puzzle for scholars. It is commonly assumed that secondary sanctions do nothing more than mollify domestic audiences and needlessly rile allies. How can extraterritorial sanctions be explained?

States, Firms, and Power addresses this gap. George Shambaugh examines the post-1945 history of U.S. sanctions against foreign firms and discovers that the record of extraterritorial sanctions is more complex than previously thought. He argues that the effectiveness of these sanctions has been greater than expected (50% exactly). Success is dependent upon the trade dominance of the United States (the share of U.S. exports and imports in the relevant sector) in the industrial sector of the targeted firm, as well as the target state's trade dependence (the net utility the target derives from the trading relationship, given the opportunity costs of adjustment) on the U.S. market.

Shambaugh uses a social power framework to address the question of sanction effectiveness. This framework, in contrast to realism, claims that power is not a fungible resource. "Thus, unlike the structural models, issue- and context-specific models assume that trade dominance in one industrial sector will enable a state to exercise power over foreign companies operating in that sector, but not necessarily in others" (p. 10). Sanctions that would cause a target government minimal pain could be devastating to a firm within that country.

From this sector-specific approach, three hypotheses follow. First, extraterritorial sanctions should be more effective when the U.S. market is an important one for the sector in question. Second, when the target is dependent upon U.S. markets and suppliers, it will be more likely to acquiesce. Third, targeted firms are more likely to concede when U.S. power is derived from the ability to close off American markets, rather than the ability to disrupt supplies of inputs.

Shambaugh tests these hypotheses with a statistical analysis of 66 U.S. extraterritorial sanctions from 1949 to 1986 and two in-depth case studies: the 1982 sanctions against Western European firms involved with the construction of the Soviet natural gas pipeline, and sanctions to enforce Operation Exodus, a mid-1980s effort by the United States to restrict high-technology exports to the Soviet Union.

Shambaugh's conclusion may not sound dramatic: "Sanctions and incentives are likely to be effective when the targeted actor is dependent upon the particular resource that it is being offered or denied" (p. 10), but it should be placed in context to understand its import. Recent literature on sanctions either claims that they never work (Robert Pape, "Why Economic Sanctions Do Not Work," *International Security* 22 [1997]: 90–136) or that they only work when backed by a preponderance of military power (Clifford Morgan and Valerie Schwebach, "Fools Suffer Gladly: The Use of Economic Sanctions in International Crises," *International Studies Quarterly* 41 [1997]: 27–50) Shambaugh's point that sanctions work when they are economically costly to the target rebuts these arguments.

The author also does an excellent job of applying this

framework to the Helms-Burton and Iran-Libya cases, pointing out that the former has been more effective than commonly acknowledged, whereas the latter has been ineffective. He concludes that secondary sanctions do not create conflict because of nominal violations of sovereignty norms. Rather, "secondary sanctions tend to cause intergovernmental conflict precisely because they can provide an effective means for states to influence the activities of foreign firms and individuals operating abroad" (p. 161).

These results are useful but incomplete in several ways. One problem is the failure to consider alternative explanations. Shambaugh contrasts his social power framework with a hegemonic power explanation, but this is an odd choice. There was very little variation in U.S. power during the period under review, so it is not surprising that it would not explain much. It would have been more useful to compare the social power approach with others that stress issue linkage in a more sophisticated fashion. This is an important question, since the book implies that linkage policies are generally fruitless. For example, does sectoral trade dependence do a better job of explaining the cases than the target state's overall trade dependence on the United States? What about the presence or absence of an international institution? These possibilities are not examined systematically.

The statistical chapter is also problematic in places. The explanation of the variable codings is convoluted at times and may require two or three rereadings. The operationalization of U.S. hegemony is somewhat unusual, but there is no explanation given for it. In the regression that compares the social power framework with the hegemonic power explanation, Shambaugh claims that key variables are statistically significant, but his own significance criteria show otherwise (compare the statements on p. 61 with the table on p. 63).

The most important problem is the unexplored relationship between the firms targeted for sanctions and their host countries. One conclusion that comes through in the empirical material is that secondary sanctions are much more likely to succeed if the host country implicitly or explicitly supports the sanctions, but there is little explanation for the conditions under which states will choose to support or oppose sanctions. Curiously, Shambaugh does generate one hypothesis on this question: States are more likely to acquiesce when the sanctioning state threatens to cut off the supply of strategic goods; but there is no substantive test of this hypothesis. As a result, his claim that the social power framework can be used to explain the behavior of firms and states must be discounted.

Faking It: U.S. Hegemony in a "Post-Phallic" Era. By Cynthia Weber. Minneapolis: University of Minnesota Press, 1999. 168p. \$37.95 cloth, \$14.95 paper.

Anne Sisson Runyan, Wright State University

What happens when queer theory "takes on" U.S. foreign policy? Suddenly, the dry and all-too-familiar landscape of rational choice, power politics, military interventions, and regime building gives way to an iconoclastic phantasmagoria of castration, love-scorned melancholia, male hysteria, and cross-dressing. At the center of this passion play is the U.S. body politic, which loses "his" vital member at the hands of Castr(o's/ating) Cuba, denying "him" "straight" forward power projections and reflections in the Caribbean She/Sea. Ever since this dismemberment, which ushered in a "post-phallic" era, U.S. foreign relations in the Caribbean can be read as a series of compensatory strategies designed to "fake" phallic power. Faking it, however, means that the United

States must strap on a queer organ, which ironically "(re)covers America's international phallic power while at the same time throwing its normalized (or straight) masculine hegemonic identity into crisis" (p. 7). By revealing how U.S. identity and foreign policy in relation to the Caribbean has become queered, feminist international relations and queer theorist Cynthia Weber seeks to disrupt the denial of this masculine identity crisis in the interests of "unharnessing" phallic power and hegemony.

How does Weber support her claims about the queering of U.S. foreign policy? By rereading "in the Barthesian sense the U.S. collective 'national fantasy' about its hegemony" (p. xiv) as articulated in the words of American foreign policymakers and configured in the actions of the United States since 1959 in Cuba, the Dominican Republic, Grenada, Panama, and Haiti, Weber presents a mise en scene in which she traces the gender (re)codings of a U.S. protagonist as it tries to (re)read itself through its increasingly antagonistic "others." What emerges is a riotous cast of characters whose very names signal the sexual-scape of the U.S.-Caribbean theater. The story opens with a castrating Castro, whose hypermasculinity emerging from the feminized space of a U.S.-controlled Caribbean thwarts but does not quell the "narcissitic love" of the United States for the Caribbean She, which it needs to assure/reflect back its own masculinity. Like Balzac's Sarrasine, the United States cannot accept that the object of its desire is no longer the woman "he" thought "she" was. Even when the now ambiguous "she" in the form of Castro's Cuba becomes the "trophy mistress" of another man (the Soviet Union), which the United States tries but fails to stop, the now neutered United States, in the grips of love-lost "melancholia," embarks on a program of "rephallusization" to recover its lost member and love. (Consider the contemporary reenactment of this in the Elian Gonzalez controversy over who is the father, Cuba or the United States.)

"Seeing red" everywhere and attempting to cover up its melancholic "blues," the United States, under the "Johnson" administration, invades the Dominican Republic to "whiten out" its loss of Castro's Cuba. But in doing so it "overexposes" itself, substituting hegemony with unpopular naked power, which "stripped" it of its "status as a loved object" and revealed a more "phantom-like" phallus in the eyes of the Caribbean "Sea/See" (p. 54). In order simultaneously to reclaim/reclamate a hegemonic phallus and recalm the Caribbean Sea/She, "screen" star Ronald Reagan performs an act of prestidigitation, making the United States believe it has a real (not just a "reel" or simulated) phallus through the invasion of Grenada and turning the Caribbean Sea/Seethe into a refeminized "U.S. lake" through the Caribbean Basin Initiative. But this "remasculinization of America," which Susan Jeffords (The Remasculinization of America: Gender and the Vietnam War, 1989) also tracks in her analysis of Vietnam cinema as it shifts from guilt-ridden characters in the 1970s to Rambo in the 1980s, did not last, for its fiction wore thin as Reagan himself became frayed around the edges in the face of the Iran-Contra scandal.

Bush (whose very name and signature slogan—"Read my lips!"—are evocative of female genitalia) is unable to sustain even a simulated erection, so he and his also similarly hapless counterpart, Noriega, who was a man without a "canal" to prove his own masculinity, are thrown into "male hysteria," which is signaled by an "excessive miming of masculinity" to cover up for "what's missing" (p. 98). But the U.S. invasion of Panama and subsequent arrest of Noriega are not achieved by masculine international power projection but by feminine tactics of "encirclement" and "entrapment" that are played

out in domesticated space. This is the defining or definitively "queering" moment of the "New World [Post-Phallic] Order," which sets the stage for the United States in drag playing "Who's got the phallus?" in Clinton's "intervasion" of Haiti. Just as Clinton disavowed having sex with Monica Lewinsky by claiming he only used a fake phallus (a.k.a. the cigar), he also embraced the strategy of "male masquerade" (or cross-dressing to appear not to have a phallus but also to keep "others" guessing as to whether there is a phallus under that dress) vis-à-vis Haiti by dressing up U.S. action there under the mantle of the United Nations and deploying a good cop-bad cop team of negotiators (Carter, Powell, and Nunn), who together added up to an indeterminately gendered (or "transvestite") U.S. presence.

Through this rewritten script of international relations as intersexual relations, the reader begins to see the "altered state" of the United States as Weber does: "A white headless body of indecipherable sex and gender cloaked in a flag and daggered with a queer dildo harnessed to its midsection" (p. 1). Captain America gives way to Austin Powers, International Man of Mystery, not only in this scholarly reimagining but also in the national/popular imaginary. Indeed, just as this heady and hilarious mixture of poststructuralist, psychoanalytic, queer, feminist, and critical analysis must be read against and in between the lines of the traditional stiff stuff of international relations as an act of counterhegemony and disarmament, it also is illuminating to read it with the latest exploits of Austin Powers in mind, who loses his "mojo" in the 1960s, ineffectually tries to recover it, and then decides he does not need it to fake it. Of course, the audience knows that the comical, mixed gender figure of Austin Powers is only kidding himself and, in fact, never really "had" "it" at all, despite and because of all his posturing. But this is also what Weber invites her audience to know about the United States: Its dominant stories about itself are but artificial props (or "queer accessories") that (self-)deceptively prop up its hegemonic power. Whatever your response to this highly provocative, revelatory, and unforgettable take on "everything you wanted to know about U.S. foreign relations, but were afraid to ask," one thing is for sure—the United States and his/her/ its relations will never look the same to you again.

Media and Political Conflict: News from the Middle East. By Gadi Wolfsfeld. New York: Cambridge University Press, 1997. 386p. \$19.95 paper.

Robert M. Entman, North Carolina State University

This important book explores the influence of the news media in the frequent instances when political conflicts occur between unequal sides, such as a large state acting against a smaller one, or a government against a protest group. The defining characteristic of these circumstances is that one side has far weaker coercive resources than the other—or none at all. Wolfsfeld terms the side with the preponderance of resources "authorities," and they confront "challengers." He advances a "political contest model" to describe and explain the ways media shape and are shaped by struggles for control over outcomes. Although the model is applicable to international conflicts, the book concentrates on domestic conflict between protest groups and government authorities. Wolfsfeld seeks to explain how antagonists and the media influence one another and, in particular, when the media are most likely to play an independent role in political conflicts. Adding a much needed non-U.S. focus to literature in this area, after elaborating his model, Wolfsfeld offers several illustrative case studies based on political conflicts in Israel.

Wolfsfeld argues that the political process is more likely to influence the media than vice versa: The media usually react rather than initiate or control events. But this is not a brief for the minimal consequences of the media. In many circumstances, the media's decisions do help set the public agenda, magnify political success and failure, serve as independent channels for challengers, mobilize new participants into a conflict, and affect the understandings of events held by elites and ordinary citizens alike. The degree to which the media exert some independent influence heavily depends on the degree of control over newsworthy political events that authorities can exert. When the authorities can thoroughly control the flow of information and the behavior and talk of elites, their perspective will structure the media's framing of events. News may then become little more than government propaganda. But when authorities' control is shaky, the media obtain more potential sources and perspectives from which to choose, opening space for more independent coverage that can further undermine authorities' control. This means that the media's influence over the outcome of conflicts varies over time and circumstance, depending on such factors as the political resources, skills, and power of antagonists; the perceived state of public opinion; the ability of journalists to gain access to events; and the nature of events themselves. Any simple, general conclusion about the media's role and power therefore is likely to mislead.

In a few well-written chapters, Wolfsfeld lays out the important variables in the media-government relationship with precision and insight, making distinctions that are often omitted or poorly specified in the literature. For example, while he acknowledges the enormous structural advantages exercised by governing elites in the contest to shape media coverage and public perspectives on events, he identifies the forces that can counterbalance government's sway. In his formulation, the ability of challengers to influence the media depends on each side's value to the media, divided by its dependence on the media. Value is determined by such matters as a participant's political and social status, degree of organization and resources, willingness to engage in exceptional (and thus newsworthy) behavior, and ability to exert some control over political events. Dependence is determined by a group's access to government power and its need for external support. Authorities do not automatically have more power over the media than challengers. For instance, lower ranking officials often have less value to reporters seeking good copy than leaders of powerful interest groups, and presidents perceived as unpopular and ineffective may be more dependent on media favor and less valuable to journalists than a charismatic senator or even a group leader. As the examples suggest, Wolfsfeld's model can help us understand not only conflicts between authorities and outside challenges but also the frequent circumstances in which there is no unified "government" version of events, when elites themselves conflict. In such conditions, the media may contribute to determining who actually governs.

The heart of the book consists of a series of case studies of political contests: protests in Israel for and against the Oslo peace accords, competition over the Israeli government's responses to the Intifada protests by Palestinians (a major goal of both sides was influencing the U.S. media, not just Israeli), and conflict over the Gulf War. Each study provides a sensitive account of media-government interactions that illustrates the theoretical model. One distinguishing characteristic of the cases is that, again atypically for many media studies, they are based not only on content analysis but also on Wolfsfeld's interviews with participants in the conflicts. He managed to obtain candid discussions of media strategies

and calculations from government officials, protest group leaders (Israeli and Palestinian), and journalists. For instance, in analyzing the Intifada, Wolfsfeld concludes that media did not cause but did affect the course of that movement. The Intifada illustrated a protest group overcoming the government's normal ability to control both newsworthy events and information flow. The media coverage enlarged the scope of the conflict, bringing in potential allies to the Palestinian side, and boosted the international visibility and thus morale of the Palestinians. The willingness of Palestinians to expose themselves to physical danger day after day, combined with the presence of international media to record newsworthy images of the physical conflicts, and with substantial disagreement about the situation among Israeli and U.S. elites, heavily constrained the options of the Israeli government.

Although the case studies provide many valid insights, one can sometimes quibble about Wolfsfeld's application of his own model. For example, in making his point about the importance to media images and public perceptions of exerting or losing control over events, Wolfsfeld argues that "all of the public relations experts in the world could not correct the damage in the news media that followed the explosion of the car bomb that killed over 200 Marines" in Lebanon in 1983 (p. 25). In fact, the Reagan administration's public relations experts, by using their other resources (such as control over information) and in the absence of active elite opposition, turned even this disastrous, unexpected event into a relatively benign media narrative. That reading is supported by the fact that approval of Reagan's poorly rationalized Lebanon policy rose by 15 percentage points, to 65%, after the tragedy. But it is certainly true that—as illustrated by the Intifada circumstances often arise in which the public relations machine fails to convert the media to cooperative mouthpieces for government.

Wolfsfeld modestly claims his book may lack generalizability because of its focus on the small and perhaps unique state of Israel. Yet, the model he offers seems broadly applicable. More clearly than any previous study, this volume identifies the important variables that structure relationships among government, the media, and challenging groups, and it provides a plausible and testable account of how some of the key questions that occupy political communication scholars may be answered. The book merits wide and careful attention among scholars of political communication and political participation.

Security, Strategy, and Critical Theory. By Richard Wyn Jones. Boulder, CO: Lynne Rienner, 1999. 196p. \$49.95.

David A. Welch, University of Toronto

The Victorian travelog is a fascinating literary genre. Generally written in exquisite prose, the typical work presents itself as an authoritative exploration of a foreign land and its people. But despite a handful of brilliant insights, it all-too-obviously betrays an interloper's eye and a stranger's intimacy, painting a distorted picture of the whole. The composition is too hasty; the brush is too broad; the palette is too limited; and the artist, lacking sympathy with the subject, caricatures and cartoons. The writer also cannot resist—with imperial condescension—comparisons with Britain. A smattering of positive ones, on minor points, is de rigeur, but on major points the gist is always the same: "These people ought to be more like us." Yet, the comparisons unwittingly yield fascinating insights into Victorian Britain itself. In short, the

travelog—ostensibly a lens—is unfailingly a much better mirror.

Richard Wyn Jones has written the equivalent of a Victoian travelog. He is a critical theorist traveling in the land of "traditional" security studies. But he improves on the original genre, for his self-examination is explicit. His book is designed both as lens and as mirror. The mirror is superb. The lens is problematic.

Part I, "Traditional and Critical Theory," is an efficient and penetrating orientation to the Frankfurt School and the evolution of critical theory from Horkheimer and Adorno to Habermas, Honneth, Beck, and Feenberg. It stresses three issues central to the study of security: the social role of theory, technology, and emancipation. Wyn Jones makes no secret of his sympathy for Horkheimer's original project, but he is sharply critical of its historical trajectory. The discussion is subtle, sophisticated, hard-nosed, yet respectful. The prose is graceful and accessible to boot. I know of no better entrée to the subject.

Wyn Jones defines critical theory in opposition to "traditional" theory, which, in his view, posits a radical separation of subject and object, maintains a rigid fact-value distinction, and embraces a "scientific objectivist" epistemology. Critical theory, in contrast, insists that subjects and objects are mutually implicating; embraces dialectical materialism; denies the possibility of value-free or value-neutral knowledge; and self-consciously seeks to promote the goal of human emancipation through the device of immanent critique, revealing the transformative possibilities inherent in the status quo. It is on this last point that Wyn Jones is most critical of theorists such as Horkheimer, Adorno, and Habermas; they, for one reason or another, have abandoned immanent critique and have lost their normative zeal. Wyn Jones favors reenergizing critical theory along Gramscian lines.

reenergizing critical theory along Gramscian lines.

Part II, "Traditional and Critical Security Studies," is primarily an assault on neorealist theorists and Cold War nuclear hawks. Wyn Jones criticizes them for fetishizing and anthropomorphizing the state, for having an overly narrow and traditionally military understanding of security, for ignoring the ethical implications of their work, and for having an "erroneous, undialectical, and ahistorical understanding of technology and particularly the interaction of military technology and strategic culture" (p. 166). They speak exclusively to elites, legitimize the status quo, and silence alternative approaches. Wyn Jones does not bother to hide his antipathy here, and the discussion is unsubtle, unsophisticated, and disrespectful. The prose is still graceful and accessible, but as an entrée to "traditional" security studies, it is deeply flawed.

What explains the stunning difference between parts I and II? Part of the answer, surely, is asymmetrical familiarity. Wyn Jones knows his critical theory, but his reading of "traditional" security studies is extremely narrow, and the number of people whose work fits his description is very small indeed. He offers several cogent and powerful critiques of this corpus, but they are not particularly new. The most devastating demonstrate that, on its own terms, neorealism does not perform particularly well, but this is precisely why so few security specialists concern themselves with it today. The vast majority of scholars who understand themselves to be studying "security" in a "scientific" way avoid anthropomorphizing the state, are keenly sensitive to questions of organizational and strategic culture, appreciate the importance of belief-systems, have a sophisticated understanding of military technology, and, increasingly, concern themselves with a much broader range of issues than just interstate military conflict.

In his exhortation to broaden and deepen the security agenda, Wyn Jones has somehow completely overlooked an enormously rich and diverse community of scholars who have done just that. There is no room in his taxonomy for Graham Allison, Steve Chan, Dan Deudney, William Dixon, Barbara Farnham, Peter Feaver, Ted Robert Gurr, Ole Holsti, Thomas Homer-Dixon, Robert Jervis, Charles Kupchan, Deborah Welch Larson, Richard Ned Lebow, Jeffrey Legro, Jack Levy, Joseph Nye, John Owen, Barry Posen, R.J. Rummel, Scott Sagan, Jack Snyder, Janice Gross Stein, Stephen Van Evera, and John Vasquez (to name a few). Compounding the lack of familiarity is a certain amount of confusion. For example, when Bruce Blair's very traditional analysis leads to the conclusion that strategic and political culture can affect force-structure decisions (The Logic of Accidental Nuclear War, 1993), Wyn Jones fails to notice that traditional theory can handle cultural variables, too. Instead, he interprets Blair's treatment of technology as "critical" (pp. 140-1).

To some extent, Wyn Jones is handicapped by the Frankfurt School's fundamental misunderstanding of the scientific project. It is debatable whether anyone since Bacon held to a "radical" distinction between subject and object and the simple correspondence theory of truth that goes with it, but if so, these had certainly disappeared by the early 1930s with the work of Erwin Schrödinger, Werner Heisenberg, Kurt Godel, and Niels Bohr. Far from "undermining" the naturalscience model (p. 101), developments such as these took it to a much higher level. Likewise, critical theorists misunderstand the supposed "value-neutrality" of modern science. Scientists are generally motivated by a desire to make the world a better place; are rarely indifferent to the uses others make of their discoveries; and appreciate that, in certain circumstances, their values and beliefs can affect their interpretation of ambiguous experimental results. Most scientists are also aware that values, beliefs, and "power" shape research agendas and resource allocations (an insight critical theory and poststructuralism have helped draw to their attention). But they insist that we can know at least some important things about the world regardless (e.g., the value of Planck's Constant or the speed of light).

The crucial point is that a researcher's interaction with the object of study, and the values he brings to it, represent nothing more than sources of uncertainty and error whose seriousness varies with context. Ceteris paribus, these are more a problem for social than for natural scientists. Within natural science, observational effects are more a problem at microscopic than macroscopic scales. We can never eliminate these errors entirely. But they do not imply either (1) that all knowledge is socially constructed or (2) that one set of statements about the world is as defensible as another. We cope tolerably well with these sources of error by adhering to methodological norms that make them easier to spot. Foremost among these is the publicity and transparency of evidence and inference.

Ironically, it is precisely these norms that provide Wyn Jones with his best critical tools. Relatively little of his critique—either of critical or "traditional" theory—is dialectical or immanent. He has a sharp eye for faulty inference; he is concerned when he sees a lack of "fit" between theory and evidence; he prefers claims that are "closer to the truth" (p. 45) and "more attuned to social reality" (p. 156); and he seeks a "superior understanding of security" with "a broader utility" (p. 1). These are all criteria quite congenial to "traditional" theory properly conceived.

And herein lies the transformative potential immanent in Wyn Jones's work. But to realize that potential, he will have to rethink a central pillar of critical theory as he has defined it: Its willingness to make moral judgment a truth-criterion. This is a mistake for three reasons. First, as we learned from the devastation Lysenko wrought on Soviet biology, one cannot necessarily wish the world into working one way rather than another. Second, if one truly seeks to persuade others, it rarely pays to cast your difference as a Manichean struggle, accusing them of an "atrophy of reason: a pathology, a moral blindness, the type of thinking that was exhibited in the mechanized slaughter at Auschwitz" (p. 130). Third, and finally, moral visions themselves cry out for justification. Few would disavow "human emancipation" as an abstract goal, but there is ample room for legitimate debate about what that ought to mean concretely, and how best to achieve it.

The Effectiveness of International Environmental Regimes: Causal Connections and Behavioral Mechanisms. Edited by Oran R. Young. Cambridge, MA: MIT Press, 1999. 326p. \$55.00 cloth, \$25.00 paper.

Christopher C. Joyner, Georgetown University

International environmental regimes are political institutions, but do they really matter in interstate relations? If so, to whom, in what ways, and why? Moreover, what factors and circumstances determine their effectiveness? The product of a multiyear project directed by Oran Young, the regimemeister at Dartmouth College, this volume systematically addresses these queries. The twelve authors critically assess three international regimes concerning marine pollution, a shared fishery, and transnational acid rain deposition. Their aim is to determine whether and to what extent these arrangements actually affect the behavior of participant states. Both in terms of theoretical design and policy analysis, the authors succeed commendably.

"Effectiveness" is a tricky, multidimensional concept, but this project treats it with considerable balance and sophistication. The authors examine how and to what extent "effectiveness" is produced by each regime. Each case study adheres to a template designed to describe the problem, the stakeholders and their interests, and the main attributes of the regime. The critical focus, however, properly falls on the operation of the regime and the degree to which it affects the behavior of participants. Although effectiveness is not measured through quantitative calculations, the authors' assessments are, on balance, analytically rigorous and empirically impressive.

Oran Young and Marc Levy introduce the volume by cogently setting out the definitional parameters of effectiveness and explaining the standardized method used by each research team. Readers will gain much from this essay in its own right, as it thoughtfully elaborates on the multifunctional character of regimes, especially those pertaining to transnational environmental concerns.

In the first case study, Ronald Mitchell, Moira McConnell, Alexei Roginko, and Ann Barrett examine whether improvements in the rules regulating marine pollution reflect the actual influence of the MARPOL 73/78 Convention (the international regime for regulating discharges of oil by tankers), or are more the result of changes in the interests of the most powerful actors. As their findings reveal, the key to the regime's success lies in its regulation of an equipment subregime, which made operation of the vessels cleaner and less pollution-causing, thus contributing to the effectiveness of the regime overall.

The second case study, authored by Olav Stokke, Lee

Anderson, and Natalia Mirovitskaya, concerns fisheries management in the Barents Sea and is distinctive for being the sole conservation regime in this volume. Bilateral agreements between Norway and Russia comprise the regime, although serious questions of jurisdiction persist over who may fish where, when, and for how much to ensure that the balance of neither any particular species nor the regional marine ecosystem as a whole is disturbed. As the authors rightly conclude, even the best decisions by a fisheries regime can be undercut by scientific uncertainty about how much stock is present now, as set against the need to harvest how much stock tomorrow.

The third case study, by Don Munton, Marvin Soroos, Elena Nikitina, and Marc Levy, looks at acid rain control. Specifically, the authors evaluate the regimes created for Europe in the 1979 Convention on Long-Range Transboundary Air Pollution (LRTAP) and for North America in the 1980 Canadian-U.S. Memorandum of Intent (and codified in the 1991 Canada-United States Air Quality Agreement). The aim is to determine whether these agreements reduced acid rain deposition and, if so, how these effects were transmitted by governments into policy changes. This study well illustrates the conflictive interaction among governments, industry, labor, and environmentalists, who are the key actors involved in most struggles over domestic regulation.

It is curious that none of the studies discusses the contributions to regime effectiveness made by "soft" law (i.e., the misnomer referring to nonbinding legal norms). In the case of marine pollution such soft law surely is relevant, especially in the multiplicity of codes adopted by the International Maritime Organization for facilitating regulation of shipping, navigation, and safety standards of vessels sailing the high seas. Likewise, in the case of the Barents Sea, consideration of the FAO's Code of Conduct for Fisheries would seem prudent for governments to impose on their nationals who fish there. It also would have been useful to have maps illustrating the scope and nature of these environmental regimes. For example, a map showing the jurisdictional implications of allocating regional fisheries, in particular the disputed region between Norway and Russia and the loophole area among these two states and Iceland, would have been especially appreciated, as would another depicting the transnational scope of the LRTAP acid rain regime for Europe and North America.

These quibbles notwithstanding, certain "truths" emerge from this compendium of analyses. First, regulatory environmental regimes do matter. They alter government attitudes, shift national priorities, change legal perceptions, and produce new norms that affect policy preferences. Second, multilateral regulatory regimes are not panaceas for critical environmental problems affecting a region. At a minimum, genuine cooperation among governments is essential for regime effectiveness. Why? Environmental problems are complex—politically, economically, socially, scientifically, and legally. They defy easy solution by any state or group of states. Multiple domestic and international actors, with conflicting interests and values, make solutions even more difficult. Furthermore, regimes can shape expectations about anticipated outcomes. This finding unmistakably surfaces from the three cases analyzed.

The cases provide compelling evidence that regimes can be effective, as they facilitate and enhance intergovernmental cooperation on environmental collective-action problems. They demonstrate that regimes foster social norms rooted in considerations of legitimacy and authoritativeness, which help guide the behavior of individuals and governments alike. That is true

for international oil shippers, fishers in the Barents Sea, or industrialists in Europe and North America. In addition, regimes contribute to social learning, not only in defining norms for behavior but also in fostering the need to know more about geophysical processes that contribute to the problem, in order to arrive at appropriate solutions that might be taken by the regime, a point underscored by the long road to transnational acid rain control. Similarly, regimes can function to shape the identities of actors and thus influence how they behave in bargaining to obtain benefits and assign roles among participants in the regime. The bottom line is this: International environmental regimes can and do influence the behavior of their member states. They serve to define roles and bestow authority for regulating international conduct. And they enhance interstate cooperation and facilitate learning about serious transnational problems.

The cases analyzed make it evident that environmental regimes do make a difference. They exert a certain influence on the social behavior of actors and institutions, and they can maximize interests and opportunities of actors. States tend to bear the institutional costs imposed by a regime so long as the benefits and rules of the regime serve and enhance the state's individual interests. Regimes also enable participants to attain these interests through shared norms and reciprocal expectations. These are important findings, worthy of serious reflection. It is not surprising, then, that a careful perusal of the conceptual approach and substantive case analyses contained in this volume will yield high intellectual payoffs, not only for experts interested in international relations and regime theory but also for scholars of international law and international institutions.



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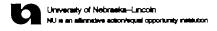
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A Nation of Organizers: The Institutional Origins of Civic Voluntarism in the United States

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disconnected from government. Using newly collected data to develop a theoretically framed account, we show that membership associations emerged early in U.S. history and converged toward the institutional form of the representatively governed federation. This form enabled leaders and members to spread interconnected groups across an expanding nation. At the height of local proliferation, most voluntary groups were part of regional or national federations that mirrored the structure of U.S. government. Institutionalist theories suggest reasons for this parallelism, which belies the rigid dichotomy between state and civil society that informs much current discussion of civic engagement in the United States and elsewhere.

ublic life in the United States has long been Prooted in voluntary membership groups as well as competitive elections. From churches and unions to social groups and reform crusades, membership associations have provided paths into active citizenship, allowing Americans to build community, pursue shared goals, and influence social and political affairs. Americans excel at the "knowledge of how to combine" that is the "mother of all other forms of knowledge" in a democracy, marveled Alexis de Tocqueville ([1835-40] 1969, 516-7) in the 1830s. By the 1890s, British visitor James Bryce (1895, 278) observed that "associations are created, extended, and worked in the United States more ... effectively than in any other country." Recently, social scientists have used cross-national survey data to document the extraordinary proclivity of Americans to participate in voluntary groups (Almond and Verba 1963; Curtis, Grabb, and Baer 1992; Ladd 1999, 131-6).

Despite longstanding agreement that voluntarism is central to American democracy—and notwithstanding its frequent invocation in theoretical and policy pronouncements—surprisingly little is known about the development of voluntary membership associations in the United States. Students of American political development might have tackled this issue but until now

have focused on class formation, political parties, and public policymaking. In debates now raging about America's civic health, everyone refers to traditions of voluntarism, but assumptions prevail in the absence of systematic evidence.

THE CURRENT WISDOM: SMALL WAS BEAUTIFUL IN AMERICAN CIVIC LIFE

Classic American voluntary membership groups are widely presumed to have been spontaneous and particular creations, fashioned within relatively bounded local communities; neighbors and friends coalesced outside politics and apart from involvements with extralocal government. Pundits and normative theorists regularly espouse this vision of America's civic past. Peter Drucker (1993, 9) contrasts America's tradition of "voluntary group action from below" to "the collectivism of organized governmental action from above," and George Will (1995) portrays voluntary groups as neighborly "little battalions" doing battle with "the federal government's big battalions." "Before the modern age," write conservative political theorists Michael Joyce and William Schambra (1996, 11-2) in a crisp formulation of conventional wisdom, "American life ... was characterized by both its selfcontainment and its cohesiveness. Individuals were closely bound to one another by strong families, tightly knit neighborhoods, and active voluntary and fraternal groups. Through these small, local, 'human-scale' associations, Americans not only achieved a sense of belonging and connectedness but also tackled the full range of social and human problems that today have largely become the province of government." Liberal theorists rarely disparage government, yet communitarians (e.g., Sandel 1996) suggest that national interventions have compromised local civic virtue. As Beem (1999, 197) shows in a wide-ranging review of current scholarship, theorists of all stripes focus on local communities and consider "governmental actions and ...large political organizations ... at best irrelevant to, and, at worst, inimical" to democratic civil society. Among empirical scholars, similar presumptions

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span disciplines. Historians portray U.S. voluntary groups as local, informal, and profusely varied-until industrial modernization brought standardization and bureaucracy. Versions vary (cf. Wiebe 1967 and Ryan 1997), but the main story line features the eclipse of once vital particular communities by nationalizing forces (for a critique of such historiography, see Bender 1978). Offering another variant of the standard wisdom, political scientists Gerald Gamm and Robert Putnam (1999, 513) use U.S. city directories from 1840 to 1940 to tally groups they assume were "obscure, scattered, and often small." Their analysis reveals that smaller cities and places outside the East had greater numbers of voluntary groups per capita. Reasoning from the theory of social capital outlined in Putnam's Making Democracy Work (1993), Gamm and Putnam (1999, 533, 549, 551) argue that the U.S. "civic core was in the periphery" because associations were "created and sustained most easily" in "slow-growing" communities that were "relatively small and homogeneous."

Indeed, theorists of social capital have become the latest exponents of the small-is-beautiful school of civic virtue. In this perspective "horizontally" but not "vertically" organized groups foster and sustain face-toface networks essential for healthy democracy. "Taking part in a choral society or a bird-watching club can teach self-discipline and an appreciation for the joys of successful collaboration," reasons Putnam (1993, 90); and small groups foster societal trust and governmental efficiency. To test such ideas, Putnam measured the density of purely local sports, recreational, and cultural groups in various regions of Italy. "Local branches of national organizations" were deliberately excluded because "organizations 'implanted' from the outside have a high failure rate," whereas "the most successful" groups are "indigenous... initiatives in relatively cohesive local communities" (Putnam 1993, 91-2, including n. 35). Invoking widely held ideas about U.S. civic history, Putnam (1993, 91-2) concludes that regions of Italy thick with local recreational and cultural groups "rival Tocqueville's America of congenital joiners."

AN ALTERNATIVE ACCOUNT OF AMERICAN ASSOCIATIONAL DEVELOPMENT

Small-as-beautiful understandings of America's civic past prevail today, but a quite different account appears in historian Arthur Schlesinger's (1944) classic article, "Biography of a Nation of Joiners." Focusing on "voluntary bodies of sizable membership, reasonably long duration, and fairly large territorial extent," Schlesinger (pp. 2, 25) portrays the development of a "vast and intricate mosaic" of large-scale associations "reaching out with interlocking memberships to all parts of the country."

In colonial America, Schlesinger (1944, 5) argues, voluntary groups were few and usually tied to local church congregations. But the struggle for independence from Britain taught "men from different sections valuable lessons in practical cooperation," and "the adoption of the Constitution stimulated still further

applications of the collective principle." A new associational model crystallized in the early 1800s, a time of flux and experimentation in the democratizing republic. Ambitious organizers developed a standard approach: They chose an "imposing" name, "sent forth . . . agents on the wide public," and "multiplied" "subsidiary societies . . . over the length and breadth of the land." Associations began to organize along the lines of "the Federal political system, with local units loosely linked together in state branches and these in turn sending representatives to a national body" (Schlesinger 1944, 11). Subsequently, the Civil War brought a "heightened sense of nationality," redoubled "Northern endeavors to plan far-flung undertakings," and so gave "magnified force" to association-building in the late 1800s (Schlesinger 1944, 16).

Although not explicitly theoretical, Schlesinger's overview highlights the role of national organizers who learned from political experience, and it suggests that translocal federations fostered local chapters. What is more, Schlesinger's evidence resonates with the ideas of scholars (e.g., Berman 1997; Evans 1997; Levi 1996; Tarrow 1996a) who criticize social capital theory for downplaying the influence of government in civil society. As Sidney Tarrow (1996a, 395) puts it, "the character of the state is external" to the social capital model, because "civic capacity" is seen "as a native soil in which state structures grow rather than one shaped by patterns of state building." Reinforcing doubts about such thinking, Schlesinger suggests that American voluntary groups developed in close relationship to the representative and federal institutions of the U.S. state.

UNRESOLVED ISSUES AND NEW EVIDENCE

Were nationally organized associations prevalent or scarce in America's past, and did translocal linkages encourage or undercut local voluntary groups? What, if anything, did governmental institutions and episodes of nation-state formation have to do with the development of U.S. membership associations? The contrasting perspectives we have reviewed need to be adjudicated with systematic data and methods of analysis. Ideally, we would like to draw a "sample" from a master directory listing membership groups of all types and sizes, past and present. But no such census exists; and post-1955 directories miss many groups that lived and died in the past. Because no straightforward random sample can be drawn, we triangulate, using several sources of data.

To explore Schlesinger's hypotheses more systematically than he was able to do, we consider not just scattered examples but the entire universe of very large U.S. membership associations, using data from an ongoing study (Skocpol et al. 1999) of the origins and development of all U.S. voluntary groups, apart from churches and political parties, that ever enrolled 1% or more of adults as members. In the larger study, directories and historical works were used to compile the names of groups whose membership might have ex-

ceeded 1% of U.S. adults (the baseline includes both genders for mixed associations, but 1% of men or women for single-gender groups). Data have been uncovered to determine that 58 associations exceeded this threshold. Here we examine 46 groups that recruited 1% of American men and/or women at any time prior to the 1940s, when Schlesinger's overview ended.¹

A study of only very large associations would obviously bias our findings, so we use two additional kinds of evidence. Historical directories and compilations (Breckinridge 1933, Part I; Palmer 1944; Preuss 1924; Schmidt 1980; Stevens 1899) enable us to situate very large groups in relation to other translocal organizations. In addition—and this is the crucial evidentiary aspect of our study—we use city directories to obtain listings of locally present voluntary groups of all kinds (see Appendix B). For the same geographically dispersed set of cities examined by Gamm and Putnam (1999), we ask what proportion of all groups listed in city directories were part of translocal federations. We also probe the relative stability of strictly local versus translocally connected groups. By combining different bodies of data and looking for overlaps between universes of national and local groups, we are able to go well beyond what previous scholars have done. We can document and theorize anew about the relationship of local to translocal association-building in the historical formation of American civil society.

LARGE MEMBERSHIP ASSOCIATIONS: PREVALENCE, FORMS, AND ORIGINS

Most observers assume that large voluntary associations (apart from political parties and religious denominations) were absent in preindustrial America. But conventional wisdom is mistaken, as Table 1 reveals. Moral crusades and political movements; labor unions and farmers' associations; veterans' and women's groups; recreational and civic associations; and fraternal groups of many sorts-undertakings of each type attracted hundreds of thousands or millions of members. To be sure, some large membership organizations passed out of existence after brief campaigns to attain a policy goal, and others flared up and died down within just a few years. Nevertheless, most of the 46 groups listed in Table 1 fit Schlesinger's conception of large and persistent membership associations. More than two-fifths crossed the 1% membership threshold before 1900, and more than three-quarters exceeded this mark before 1920. Large voluntary associations have flourished in all eras of U.S. history.

Table 1, which draws on group records, official histories, and scholarly studies (see Appendix A), indicates when and where the first organized unit of

each named association appeared and classifies the aims of the group's founders. In some cases, such as the Independent Order of Odd Fellows (Stillson 1897, 211-4) and the Young Men's Christian Association (Hopkins 1951, 15-9), the founders originally thought they were establishing what we call a local "portal" for a European-based group to pass into the United States.2 In other cases, founders envisaged a local group centered in a particular city or state and only later decided to pursue national ambitions. Other founders planned from the beginning to build a truly national association, even if it took some time to realize their plans. Still other founders negotiated combinations of previously formed local or regional groups. The General Federation of Women's Clubs (GFWC), for example, was pulled together in 1890 when the leaders of the Sorosis Club of New York City convened a meeting of about five dozen clubs from across the United States (Wells 1953, chap. 2).

Many scholars assume that combination of preexisting groups must have been the principal way national associations emerged, usually after the U.S. economy became more centralized at the very end of the nineteenth century. But Table 2 shows that the fusion of subnational groups was hardly the typical route by which ultimately large membership associations came into being. Combinations from below account for only 13% of all foundings of very large groups; and only another one-fifth were originally focused on a particular city or state. Remarkably, in more than three-fifths of cases, associational founders undertook national projects from the start; these launchings occurred from the early 1800s through the early 1900s.

Nation-State Formation and Voluntary Membership Federations

Major junctures of U.S. state formation clearly punctuated the development of translocal civil associations. No national groups emerged in colonial times. Apart from transnational religious denominations, the earliest translocal association was the Masons; lodges sprang up in cities, towns, and military garrisons, and "sovereign grand lodges" formed alongside the government of each colony. But the deeply rooted Masonic grand lodges (which corresponded to the states after the American Revolution) were never able to agree on a nationally unified governing structure (Stillson 1926, 226–7). Certain higher orders affiliated with Masonry eventually adopted unified structures, but basic "blue lodge" Masonry never took this institutional step.

The American Revolution and debates over the Constitution, along with contentious and evangelical religious movements in the new nation, spurred early Americans to organize all kinds of voluntary groups,

¹ Twelve other U.S. membership associations crossed the 1% threshold after 1940. Of these, five were founded and attained very large size between the late 1950s and the 1990s, seven others were founded before 1940 but grew very large only afterward. Reinforcing the conclusions reported below, six of seven founded before 1940 (and all five founded before 1920) were representative federations with intermediate tiers at the state or regional level.

² Two other associations included in tables 1 and 2 also moved into the United States from abroad: The Red Cross was founded as the U.S. national part of an international movement, and the Maccabees started in Ontario, Canada, and very quickly crossed the border with national ambitions in the United States. We classify both as national foundings, reserving the term "portal" for associations originally founded as local U.S. outposts of foreign-centered associations.

		_		S	Scope at Inception	uopa	Nationa	National Associational Structure	al Structure	Auration Scope at Inception National Associational Structure
Organization	Founding		Ending	Portal L	Local Nat"	l Comb.	Nat/Loc	Fed/St/Loc	Nat/MId/Loc	Above 1%
Ancient and Accepted Free Masons	1733	Boston		×			None	None	None	1810s to present
Interpolational Order of Cooking	1819	Baltimore		×				1825		18409-1950s
Anierkali iemperance society	1826	Boston	8		×			1826		1830s-1840s
the Christian Sabbath	1828	Now Vort	1833		×		1828			18308
American Artt-Slavery Society	# E	Roston	200		>			0		
Improved Order of Red Men	£ 52	Rathmore	20		< >			1853 1853		18308
Washington Temperance Societies	1845	Bellinos	01970					<u>\$</u>	•	19009-19209
The Order of the Sons of Temperance	5 5	Now Vort	2 5		< >		900 NO	None	None	1840s
Independent Order of Good Terminare	1851	1 H 2 N 7	0/8/5					48		1840s-1850s
Young Man's Chretian Association	8 4	Duck INT		;	×		1	1855		1860s-1870s
Junior Order of United American Mechanics	1067	Delicable	1	<	;		(1865)	1888		1890s to present
Knights of Pothles	38	Woohington DC	0/815		; ≺			6 88		1920s-1930s
Grand Army of the Recyblic (GAB)	900	Peoplingion, DC	7		≺ ;			1868		1870a-1930a
Benevolent and Protective Order of Elec-	1007	Man Vod	8				į	1888		1860 s- 1900s
Patrons of Husbandry (Nathonal Grands)	1887	Monthson DC			; ~		1871			1900s to present
Order of the Festern Star	188	Modern grow, DO			≺ >			1873		1870s, 1910s-1920s
Ancient Order of United Workmen	88	Moodyllo DA			< >			1878		1910s to present
Knightin of Labor	8 9	Philodolphio	1					1873		1880s-1900s
Nobles of the Mostic Ships	- F	rimercelpina	<u>-</u> B		<;				1878	18809
Women's Christian Temperatus Hasa	7/01	New TORK					1876			1910a-1980a
Boxel Arcenim	1 1	CHANGELIO			×			1874		1910 9- 1930s
Comen' Alleren	//8/	Exoston :			×			1877		19008
	//81	Lampasas, TX	96	•	×			1889 899		1880 s- 1890s
MECCALORES OF JOHN TO LOCATE	1878	Port Huron, MI			×			1883		19008-1910s
	88	Portland, ME			×			1885		1880s about 1820s
Antienceul Fied Cross	1881	Weshington, DC			×		(1881)		1905	1910s to present
	1882	New Haven, CT		•	×			1883		1910s to present
Modern Woodmen of America	1883	Lyons, IA			×		(1883)	1890		1890e-1830e
Colored Farmers' Alliance	1886	Houston, TX	1882		×			1886		1880a-1890a
American Federation of Labor	1886	Columbus, OH			×				1886	1880s to present
American Protective Association	1887	Clinton, IA	c1911		×			1887	!	1890a
Woman's Missionary Union	1888	Richmond, VA				×		1888		1920s to present
Loyal Order of Moose	1888	Louteville, KY			×		1889	 		1010e to process
National American Woman Suffrage Association	1890	WashIngton, DC	1920			×	1	1890		1010
Woodmen of the World	1890 0891	Omaha, NE			×			1890		1000
General Federation of Women's Clubs	1890	New York				×	(189C)	1804		1900-19308
American Bowling Congress	1895 2	New York			×	<	2 - 2 - 2 - 2 - 2 - 2 - 2 - 2 - 2 - 2 -	5 5		80/81-80/6
National Congress of Mothers (PTA)	1897	Washington, DC			< >		(2001)	1809		1930s to present
Fraternal Order of Eagles	1898	Seettle, WA			< >		4000	8 6 8 6		1920s to present
German American National Alliance	1801	Philadelinhla	1918		<	>	(0801)	<u> </u>		19009-19806
American Automobile Association	190	Chrome	2			< >	800	108		19108
Boy Scouts of America	1910	Weehlrotton DC			>	<	(30g)	1808 408		1920s to present
Ku Klan (Second)	1015	Attento	1044		< >		0181	1		1930s to present
American Legion	1010	Minnoondie	<u> </u>		< >			9181		19208
American Farm Bureau Federation	40.0	Chrones Chrones			<	>		1919		1920s to present
Old Age Revolving Pensions, Ltd. (Townsend)	1934	Long Boach CA	1053		>	<	3	8181	•	1920s, 1940s to present
Congress of Industrial Organizations	5 25	Diffehingh	3 5		< ;		45			1930a
			1		,					

Note: For data sources, see Appendix A. "Duration" gives the date and location from which each named association was launched and inclosuse an ending date for groups that no longer exist. As further explaned in the text, "Scope" refers to the land of local or national association envisaged by the organizers at inception. "National Associational Structure" gives the date of adoption of the first national constitution and shows that 13 groups started with national/local arrangements, but seven of these later shifted to other structures. "Above 1%" shows all decades in which associational membership (even brieff) exceeded 1% of all U.S.

Founding Era	International Portal	Local: Focused on City or State	National	Combination of Existing Groups	Total Large Associations Founded
Colonial period (prior to 1790)	1 (100%)	0 (0)	0 (0)	0 (0)	1
Early national (1819–59)	2 (20%)	3 (30)	5 (50)	0 (0)	10
PostClvll War (186099)	0 (0%)	6 (22)	18 (67)	3 (11)	27
Twentleth century (1900–40)	0 (0%)	0	5 (62.5)	3 (37.5)	8
Total	3 (6.5%)	9 (19.5)	28 (61)	6 (13)	46

even in tiny towns (Brown 1974; Mathews 1969). Early in the life of the fledgling republic, moreover, popularly rooted membership associations were organized on a national scale. They took shape at the critical sociopolitical juncture between the 1820s and 1840s, when voting rights were extended to most U.S. adult males and competing political parties (themselves federations of local and state organizations) emerged to mobilize the mass electorate (Aldrich 1995, chap. 5; Shefter 1994, 66-71). The American Temperance Society, the American Anti-Slavery Society, and the General Union for Promoting the Observance of the Christian Sabbath (GUPOCS, which agitated to close U.S. post offices on Sunday) all aimed for large size and geographical spread because their organizers wanted to change national mores and influence state and national legislation. These massive crusades appealed to-and helped stimulate—a democratically aroused citizenry (John 1995, chaps. 5-7; Tyler 1944, Part III).

In the same era, nonpolitical groups also moved toward national projects and institutions. Of these, by far the most important was the Independent Order of Odd Fellows, which was destined to become an organizational model and seedbed for hundreds of other membership federations. Odd Fellows were the next significant fraternalists after the Masons to enter the United States; the original ones were transplanted English workingmen who met regularly to perform the moral rituals and acts of mutual aid for neighbors and travelers that were the transnational stuff of fraternal life (Clawson 1989). But Oddfellowship in America soon shifted from a series of coastal outposts of English orders into a national project under the tellingly labeled leadership of Baltimore's "Washington Lodge No. 1," which hoped to connect preexisting Englishchartered lodges while simultaneously chartering new ones in America. The Baltimore Odd Fellows split into multiple organizational levels during the 1820s, separating a "Grand Lodge of Maryland" from the original Washington Lodge, and then forming a new "Grand Lodge of the United States" juridically separate from the state-level unit (Independent Order of Odd Fellows [IOOF] 1844; Stillson 1897, Div. Π , Sec. I and Π). The leader in Baltimore, Thomas Ridgely, went on the road to persuade lodges in other states to join or form under the Maryland-centered jurisdiction. Potential recruits were made a very attractive offer: They could establish local and state lodges and send representatives to national meetings in Baltimore but would not be subordinated to the Marylanders.

American Odd Fellows fashioned a new structure that imitated and paralleled the U.S. constitutional order. As chronicler Henry Stillson (1897, 214) explains, immigrant members with "superior discernment" realized "the impracticality" and "especial unfitness for this country" of English-style governing arrangements (which coordinated local lodges through national committees of notables) and "found their model in the political framework of the United States." Probably the Marylanders led the way for much the same reason that small state representatives had been the first to ratify the 1789 U.S. Constitution: Representative federalism with state as well as a national "sovereign" jurisdictions afforded extra prestige and leverage to people from smaller states. Using their three institutional levels to outmaneuver New York City lodges, which harbored ambitions of their own, Baltimore Odd Fellows made themselves the founders of a new fraternal republic that broke from British allegiance to become America's Independent Order of Odd Fellows, whose new constitution unmistakably echoed the U.S. Constitution (IOOF 1844, xv):

Whereas, it has been found expedient, and of great importance to mankind, to perpetuate those institutions which confer on them great and essential benefit. Therefore, the GRAND LODGE OF THE UNITED STATES..., for the more effectual purpose of binding each other in the bond of one common Union, by which we will be enabled to insure a co-operation of action,... and to secure unto ourselves and posterity more effectually the blessings which are to be derived from so valuable and beneficial an institution, do ordain and establish the following as the CONSTITUTION... OF THE INDEPENDENT ORDER OF ODD FELLOWS.

Similar representative federal institutions were soon adopted by many other brotherhoods, including America's first indigenously spawned fraternal organization, the Improved Order of Red Men, which evolved from a Baltimore-centered "tribe" into a three-tiered order (Lichtman 1901, chaps. 5-6). More telling, three-tiered arrangements were adopted by minority-ethnic orders, such as the Ancient Order of Hibernians, launched in 1836 (Ridge 1986); the German Order of Harugari, launched in 1847 (Stevens 1899, 234-5); and the Bohemian Slavonic Benevolent Society, started in 1854 and explicitly modeled after the Odd Fellows (Martinek 1985, 22). As new ethnic groups arrived in the United States, they formed associational edifices topped with national and state bodies even when there were barely enough members to form chapters scattered across a few large cities.

Organizational routines that included local membership meetings, standardized rituals, and the dispatch of elected officers and representatives to regular state and national governing conventions soon proved attractive to more than just fraternal groups. Early mass temperance associations, for example, went through a period of organizational experiments, some of which faltered (Dannenbaum 1984; Krout 1925). The American Temperance Society proved too top-down to sustain its popular appeal, and it evolved into a national center for publishing and lobbying (much like late-twentiethcentury American professional advocacy groups). The Washingtonian movement experimented during the 1840s with radical, bottom-up democracy (much like 1960s-style New Leftists), only to find that loosely networked, entirely flexible local groups with few rules and no state or national governing structures could not sustain themselves beyond the initial popular fervor (Grosh 1842; Maxwell 1950). Thereafter, many Americans interested in "the temperance cause" flowed into the Sons of Temperance, founded in 1842, and the Independent Order of Good Templars (IOGT), founded in 1851, both of which achieved enduring new syntheses of moral fervor and representative federal organization. The Sons combined temperance advocacy with lodge rituals and the provision of social benefits (Hodges 1877); and the Good Templars adapted fraternal forms to America's first civic experiments with gender and racial inclusion, allowing women and African Americans to become members and serve as elected leaders (Fahey 1996).

If Schlesinger was right about early-nineteenth-century Americans converging on a model for large membership associations that paralleled governmental federalism, he was likewise correct that the Civil War brought a "heightened sense of nationality" to association-building. As Table 2 shows, associations that would manage to grow very large emerged at an accelerated rate starting in 1864, and most postwar foundings were nationally ambitious from the start. Half the eventually large groups founded between 1819 and 1859 were initially national projects, but in the late 1800s more than two-thirds of such launchings were

national projects.³ In the same era, hundreds of other nationally or regionally ambitious associations were also launched (Palmer 1944; Stevens 1899).

Following the Civil War, the national-state-local model diffused across various kinds of voluntary endeavors in addition to fraternal brotherhoods and sisterhoods. It was adopted by veterans' associations (from the Grand Army of the Republic to the American Legion); by independent women's groups (from the Woman's Christian Temperance Union, to the General Federation of Women's Clubs, to the National Congress of Mothers, which eventually became the modern PTA); by farmers' organizations (from the Grange, to the Farmers' Alliances, and ultimately the American Farm Bureau Federation); and by assorted moral and political crusades (including the YMCA, which added a state tier to its organizational structure in 1866, as well as Christian Endeavor, the American Protective Association, and the National American Woman Suffrage Association).

In one especially telling case, the Knights of Columbus, federalism hardly came easily. Founded in New Haven as a local Irish Catholic men's group, the Knights of Columbus was initially embedded in church parishes and dioceses and was closely supervised by Catholic clerics (Kaufman 1982, chaps. 1-4). As cognate groups emerged across Connecticut, they remained so embedded. But a deliberate switch to a local-state-national federated structure with elected lay leaders came in the late 1880s and 1890s, when leaders decided to take the Knights of Columbus national in competition with the Masonic Knights Templar and other Protestant-dominated fraternal associations. Pressures to compete and legitimize the undertaking drew the Knights of Columbus toward the governance model widely used by nationally ambitious associations of that time, even when "going federal" meant breaking from the original diocesan mold. The group also imitated the standard U.S. associational practice of electing lay officers, instead of having priests or bishops head its local, state, and national councils.

Overall, nearly three-quarters of the U.S. membership associations that grew very large before 1940 (34 of 46 groups) developed federated organizational arrangements that resembled the representative, three-tiered institutions of U.S. government. As Table 1 indicates, 28 of these 34 adopted the federal-state-local form when they first established a national organization. Six others shifted from a national-local arrangement to the multitiered structure that included state units. Interestingly, several of the associations that moved away from center-local arrangements did so after members outside the founding center pressed for the addition of state units with significant authority. For example, the General Federation of Women's

³ Many "national" federations incorporated modest numbers of members from English-speaking Canada, and sometimes people from Australasia and Europe as well. Although some of these were Americans living abroad, we do not count foreign-based members in deciding whether associations exceeded 1% of the U.S. adult population. Like Canadian baseball teams today, foreign chapters and members were incorporated into U.S.-centered insututions.

Clubs was orchestrated by New York clubwomen, but women's groups in Maine and Utah spontaneously established their own state federations and pressed the national center to accept the new institutional level (Wells 1953, 34–7). Similarly, soon after the 1898 launching of the Fraternal Order of Eagles in Seattle, Washington, members in New York campaigned to establish state-level "aeries" (Fraternal Order of Eagles 1913; O'Reilly 1904, 77–81).

Among the membership associations in Table 1 that did not adopt the federal-state-local format, four groups (the Knights of Labor, Red Cross, American Federation of Labor, and Congress of Industrial Organizations) developed other kinds of multitiered arrangements; two others, the blue-lodge Masons and the Washingtonian temperance movement, never developed national centers. Of the 46 U.S. voluntary associations that attained very large memberships before 1940, only six (13%) were permanently institutionalized as center-local organizations. A representatively governed intermediate tier, usually at the state level, was overwhelmingly typical.

Why Did National-State-Local Federalism Take Hold?

Institutional theories (see Hall and Taylor 1996) allow us to go beyond Schlesinger in understanding why America's largest membership associations (and hundreds of smaller ones as well) adopted an organizational structure similar to the institutional arrangements of U.S. government. Two arguments are relevant: hypotheses about "political opportunity structures" and ideas about organizational imitation.

Social movements often organize to take advantage of opportunities for leverage offered by governmental institutions (Kitschelt 1986; Tarrow 1996b). From the beginning, the American political system rewarded movements and associations able to coordinate efforts at the national, state, and local level. From temperance and antislavery crusades, to farmers' groups, women's movements, and nativist agitations, groups aiming to shape public opinion and influence legislators learned the advantage of cross-level organization. By serving as a bridge between local sets of citizens and elected

⁴ State organizations are not entirely absent in these cases. Masons, of course, have strong state-level grand lodges. The Red Cross made brief attempts to set up state units before settling on a regional arrangement. State organizations with very weak representation at the national level are parts of the AFL (and AFL-CIO), although international unions have always been the key units in the labor federations.

officials, associations could influence both Congress and state legislatures (for instances, see Skocpol 1992, parts 1, 3). Operating across levels, moreover, groups could pursue social as well as political change. "Our Order," explained the Right Worthy Grand Templar of the Independent Order of Good Templars (IOGT) in 1881 (quoted in Turnbull 1901, 88-9), "is organized to destroy the evils growing out of the drink traffic, and the individual use of alcoholic drinks." Because the "drunkard-makers have strong Local, State, and National Organizations," subordinate lodges reach out to save individuals and agitate public opinion, while "against the State Liquor Union" the IOGT arrays the state-level "Grand Lodge; and against the American Brewers' Congress and National Distillers Union" it deploys the national-level "R.W.G. Lodge."

But the response of activists to political opportunities and challenges is not a sufficient explanation, because many nonpolitical associations also adopted representative-federal arrangements. For the Odd Fellows, the Knights of Columbus, and other ritual or social associations, constitutional federalism was a way to coordinate activities across localities and regions. According to institutional theorists of organizational development (Powell and DiMaggio 1991), organization-builders who face complex challenges in conditions of uncertainty are inclined to copy well-understood, already legitimate models in their environment. Dynamic variants of sociological institutionalism (e.g., Clemens 1997) suggest that innovative adaptations of this sort are likely when ambitious but somewhat marginalized organizers (such as immigrants to America) confront unprecedented challenges or opportunities and are able to draw on a new "repertoire" of collective action. After the American Revolution, the U.S. Constitution offered a widely understood and prestigious model for cross-local coordination in an era when popular mobilization made sense for all kinds of purposes. Once some groups used this model successfully, others found it legitimating and competitively advantageous to follow suit.

Still, as the United States industrialized, representative-federal associations might have given way to classdivided or corporate-style associations paralleling the emergent national market economy. But a cataclysmic and pivotal political event, the U.S. Civil War, intervened to reinforce the legitimacy and practicality of popularly rooted federalism as the preeminent model for large-scale association-building. The United States in 1860 had little in the way of a standing army, so both sides in this internecine struggle relied upon civilian as well as elected leaders to assemble local volunteers into state units, and then to mold state units into the clashing armies and civilian relief organizations of the Union and the Confederacy (Brockett 1864; McPherson 1988, chap. 10). After the war ended, spirits soared on the winning Union side. Inspired by a new sense of national purpose and thoroughly familiar with federal models of popular mobilization, northern men and women who grew to maturity in the late 1800s launched many new mass-based federations, even as

of these, GUPOCS was a short-lived movement of church-based groups and individual petition-aigners; the Boy Scout troops and Townsend/Old Age Revolving Pensions groups were coordinated by corporate-style directorates. Center-local arrangements characterize the Elks, the Moose, and the Shriners, all of which evolved from interurban networks originally devoted to recreational activities. Interestingly, some years after the national foundings, factions within both the Moose and the Elks agitated for the establishment of state grand lodges. Such efforts did not succeed because the urban lodges did not want to give up their direct ties to the national center. Nevertheless, purely voluntary, nonsovereign state associations are allowed by both the Moose and the Elks.

electoral mobilization by clashing party federations also reached its peak (McGerr 1986).

Qualitative evidence suggests ways in which Union mobilization encouraged postwar association-building. Railroad workers who met during the Civil War launched the Ancient Order of United Workmen from Meadville, Pennsylvania, in 1868, aiming to bridge class divisions and offer insurance and cultural uplift to all American working men (Upchurch 1887). America's third largest fraternal association, the Knights of Pythias, was founded in Washington, D.C., in 1864 by young clerks who met in the wartime civil service and devised a ritual of sacrificial brotherhood that appealed not only to former soldiers but also to all Americans who hoped to reknit North and South (Carnahan 1890, chaps. 5-6). Another regionally disparate group of federal clerks started the Patrons of Husbandry (or Grange) in 1867. This happened after Minnesota native and federal agriculture official Oliver Kelley was commissioned by President Andrew Johnson to assess rural needs in the devastated South (Nordin 1974, chap. 1). Using Masonic ties to make contacts in the defeated region, Kelley soon realized that farmers, too, could benefit from a nationwide fraternity. With fellow officials-each of whom, like him, moved back and forth between Washington and his home region-Kelley designed a federation that incorporated some existing farm groups and stimulated the founding of thousands of local granges.

The Civil War also emboldened civicly minded women. Along with the famous wartime nurse Clara Barton, many other women and men who had been active in the wartime U.S. Sanitary Commission agitated from the 1860s to 1881 for congressional charter of the American Red Cross (Davidson 1950b). Meanwhile, females moved to the fore in the massive temperance movement. Willing to accept women leaders and members on equal terms, the IOGT held its own during the war and burgeoned afterward, prodding the Sons of Temperance to accept females. But American women wanted an even more predominant role. Determined to counter male drunkenness, which had been exacerbated by military service, and fight government policies favorable to the liquor industry, which had become a lucrative source of tax revenues during the war, reformers convened in Cleveland, Ohio, in 1874 to launch the Woman's Christian Temperance Union (WCTU). Some of these women had met in Union relief efforts; all of them applauded the women's crusades against saloon-keepers that spread in the Midwest during the early 1870s (Mezvinsky 1959). Grassroots protests were hard to sustain, however, so women gathered at a summer camp for the National Sunday School Assembly to institutionalize "the grand temperance uprising." In cadences resonant with the "Onward Christian Soldiers" rhetoric of Union victory, a "Committee of Organization . . . consisting of one lady from each state" issued a "Call" to organize the national WCTU (reprinted in Tyler 1949, 18). "In union and in organization," proclaimed the Call, "are . . . success and permanence, and the consequent redemption of this land from the curse of intemperance."

NATIONAL FEDERATIONS AND LOCAL GROUPS

Although translocal voluntary federations may have emerged early in U.S. history and proliferated after the Civil War, it is possible that local groups weighed much heavier in community life. And perhaps chapters of large federations often proved short-lived, as social capital theory might predict, leaving the ground to be tilled by purely local joiners and organizers. To evaluate these possibilities, we need data on characteristics of local groups.

Membership Groups in City Directories

Gamm and Putnam (1999, 524) show that voluntary groups listed in U.S. city directories peaked in relation to population around 1910. What kinds of groups were these? To find out, we analyzed listings for 1910 (or the closest year available) for the same 26 cities from every region studied by Gamm and Putnam.⁶ In Table 3, cities are arrayed from top to bottom according to their size in the 1910 Census, and their groups are classified into structural categories. We count as "federated" several kinds of translocally linked groups: churches, unions, chapters of very large U.S. federations (listed in Table 1), and chapters of smaller federations. We tally as "nonfederated" all membership groups, including church-linked sodalities, that were not clearly part of separately organized translocal federations.

Had we eliminated local units affiliated with translocal associations (as in Putnam 1993), we would have missed most of the groups tallied in Table 3. In every city, most of the groups listed in the directories were part of regional or national federations, ranging from a minimum of 63% in Boston to a maximum of 94.5% in Rome, Georgia. Local groups not so connected were slightly more prevalent in the larger cities, whereas groups in the smallest cities were overwhelmingly federated. Looking more closely, we see that, in addition to churches, very large membership associations were at the very heart of American civil society locally as well as nationally. Churches and other religious congregations, devoted to translocal world views and linked to federated institutions of various sorts, were numerous in every city. Equally prevalent were local chapters of large membership federations listed in Table 1. And most "unions" were linked to the American Federation of Labor or the Knights of Labor.

From 8% to 29% of groups in these cities were local chapters of federations other than the very largest. These smaller federations, which ranged from ethnic

⁶ We are grateful to Gamm and Putnam for giving us copies of some of the directories they used. In most cases, we obtained copies from libraries or historical societies in the respective cities or used microfilms in the extensive collection of city directories held by the Boston Public Library. These copies were either for the same years or within one year of directories used by Gamm and Putnam (1999, Appendix A).

						No industrial diodpo (70 of 74)		3	() adams a someone	, , , , , ,	
				National,							
Ц	Directory	Population	Total	Regional,	County	Total			Large	Other	Total
Clty, State		In 1910	Groups (M)	or State	or Clty	Nonfederated	Churches	Unlons	Federations	Federations	Federated
OM Story	1910	687.029	2.262	2	23	18	8	Ξ	19	25	2/2
Boston, MA	1911	670,585	2,162	6	28	37	15	13	16.5	18.5	8
San Francisco CA	1910	416,912	1,176	4	16	ଷ	15.5	15.5	77	27	8
Milwaukee. Wi	1910	373,857	1,223	2	29	3	15	12	15	27	8
Denver CO	1911	213,381	677	က	6	12	83	14	24	21	8
Portland, OR	1910	207,214	716	3.5	15.5	19	82	4	18	23	ಹ
Scranton PA	1910	129,867	598	0	ผ	ង	20	12	22	24	28
I cwell. MA	1910	106,294	386	0	28	83	19	Ţ.	20	প্ত	22
χ	1910-11	96,614	251	1.5	19	20.5	31	15.5	20	13	79.5
	1910	86,368	312	4	16	8	88	0	27.5	14.5	8
Troy NY	1910	76.813	390	-	88	8	19	-	25	21	88
	1910-11	66.950	397	0	35	ĸ	20	13.5	24	10.5	88
S	1911	58,833	188	က	19	ង	ස	1.5	26	11.5	82
I III Bock, AK	1910	45,941	221	8	13	15	88	-	20	28	8
Gabbaeton TX	1911–12	36.981	248	0	8	\$	19	13	20.5	13.5	8
Brookline, MA	1911	27,792	53	0	8	8	28	7	23	19	2
Burlington VT	1910	20,468	106	-	25.5	28.5	13	6	28	22.5	73.6
Bolse ID	1811	17.358	141	6.5	18.5	ĸ	25	12	38	ω	22
Borne GA	1913	12,089	72	0	5.5	5.5	53.5	2	26	13	94.5
Adrian MI	1909	10.763	72	0	17	17	8	7	37	17	æ
Pekh. II	1911	9,897	8	0	19	19	24	17	27	13	8
Bath MF	1912	968'6	8	0	20	ଷ	25	7	4	13	8
Bowling Green KY	1914	9.173	8	0	12	12	45	ო	23	17	88
	1908	7,508	88	0	12	12	17.5	7	43	29.5	88
. Imedian Offix KS	1910-11	5,598	52	0	우	10	83	0	4	21	8
Marysville, CA	1903-04	5,430	51	0	12	12	1	80	37	23	88
Average of City Percentages	entages										,
(nmwelghted)		,		2%	20%	22%	25%	8%	26%	19%	%82 2
Note: For data sources and equatmenta, see Appendix B. Among the nonfederated groups,	edjustments,	see Appendix B	Among the nonfede	wated groups, *	Vational, Regi	orriederated groupe, "National, Regional, or State" are locally artuated gro	cally entuated on	outpe with clear	arrly extresional con-	estuated groups with clearly extrational constituencest, "July and County" (sites	Cooping reserved

		 Percent In TI 	ree Consecutive	Directories, or L	ast Two	
City, State (Directory Years)	All Groups	Churches	Large Federations	Other Federations	Unlons	Loca
Adrian, Michigan (1890–91; 1900–01; 1909; 1921)	38	83.	48	13	33	
Bath, Maine (1892; 1900–01; 1912; 1919–20)	5 1	88	61	· - 40	0	24
Bolse, Idaho (1891; 1901–02; 1911; 1921)	43	62	57	38	. 43	9
3rookline, Mass. (1891; 1901; 1911; 1920)	42	78	43	22	0	38
Burlington, Vermont (1890; 1900; 1910; 1920)	40	76	66	34	43	19
eadville, Colorado (1890; 1902; 1909; 1918)	53	100	64	41	32	36
Pekin, Illinols (1893; 1900–02; 1911; 1921)	53	100	67	33	42	37
Rome, Georgia (1888; 1898–99; 1913; 1919)	43	· 45	70	21	0	0
Average of city percentages (unwelghted)	45%	79%	60%	30%	24%	19%

and insurance-oriented fraternal groups to nativist, patriotic, and service associations, were organized and functioned in ways similar to the large associations listed in Table 1. They held regular local meetings and supralocal conventions, and they deployed organizers and members to spread their message and establish chapters on a regional if not national scale.

Local groups not obviously linked to broader federations ranged from 5.5% of directory entries in Rome, Georgia, to 37% of entries for Boston. But some of these groups clearly had constituencies that transcended city boundaries. Large cities (like Boston) hosted headquarters for national or regional associations; smaller cities (like Boise, the largest in Idaho) sometimes served as associational centers for their state or region. In the final analysis, only a minority of the groups listed in each city directory (22% was the average across 26 sites) was specific to that city or county.7 Many of these were business or professional associations or elite clubs of one sort or another, which are hardly prime venues of democratic engagement. Choral groups, mutual aid societies, church sodalities, orchestras and bands, and sports and recreational clubs-the sorts of purely local, popular groups Putnam (1993, 91-2) considers typical of civic Americaaccounted for only a tiny fraction of groups in these

cities. In classic U.S. associational life, most popularly rooted groups were parts of translocal federations.8

Persistence and Volatility

Although the cross-sectional tallies in Table 3 reveal large proportions of federated groups, perhaps translocally linked churches, unions, and chapters were too short-lived to have much civic significance. To find out, we explored group stability. Analysis was done by hand by a knowledgeable coder, and issues of consistent coverage across directories had to be resolved (see Appendix B). We examined groups in eight regionally dispersed small cities, and we focused on similar times (circa 1890, 1900, 1910, and 1920) in order to control as much as possible for nationwide associational trends.9 In each city, we traced named groups (such as "Beacon Street Methodist Church" or "Maple City Camp No. 2884" of the Modern Woodmen). Groups are classified as relatively stable if they appear in three or more consecutive directories or in the last two.

As Table 4 shows, religious congregations were extraordinarily stable in these cities (with the partial exception of Rome, Georgia, which had a dispropor-

⁷ The average percentages presented in Table 3 are not weighted by the relative number of groups in different cities. Because larger cities (especially Boston) had somewhat more headquarters and local groups, "nonfederated groups" are 26% of all groups summed across all cities.

Were purely local groups more prevalent in decades before 1910? Examination of directories back to the 1870s suggests that federated groups were proportionately even more prevalent in earlier decades. We used all except three of Gamm and Putnam's (1999) eleven small cities; the others were set aside because directories with comparable coverage were not available at times close to the decade intervals.

tionately large number of churches, including some volatile white and African American Baptist congregations). Local chapters of America's largest voluntary federations (apart from AFL unions and the Knights of Labor) were also quite stable. In contrast, nonfederated local groups were not very persistent, contrary to what social capital theory suggests.

Among federated groups, unions were markedly less stable than churches and chapters of large membership federations. Part of the reason may be that labor groups were not consistently included by compilers of city directories. But when labor groups were listed, they often appeared at just one decade point, because they rose or fell in response to industrial struggles and economic booms and busts. For example, unions and Knights of Labor assemblies disappeared following the defeat of strikes in Leadville, Colorado, and unions multiplied temporarily during the ship-building boom in Bath, Maine, during World War I.

In six of the eight small cities, units of translocal federations other than the very largest were more persistent than nonfederated local groups, but in all eight cities such smaller federated units were much less persistent than chapters of the largest federations. The era between 1880 and 1920 witnessed the rise and demise of hundreds of insurance-providing fraternal associations (such as the Order of the Iron Hall and the Knights and Ladies of the Fireside), the vast majority of which remained modest in membership. Many of these groups soon proved economically insolvent, and the more successful ones tended to merge or turn into insurance companies after 1910.

Churches and local units of the largest voluntary federations proved the most persistent. The average of city percentages in Table 4 shows this, and another calculation documents the same point. Adding together all 450 groups that were relatively stable across these eight cities between 1890 and 1920, we find that 31% were religious congregations and another 38% were clubs or lodges connected to the largest crossclass national federations. Thus, more than two-thirds of all stable groups fell into these categories. Once founded, churches and chapters linked to the largest federations took firm root and became the enduring core of civil society in modernizing America.

Chapters of major federations flourished in part because national and state leaders in such vast civic republics as the Odd Fellows, the Grand Army of the Republic, and the WCTU assumed responsibility for sustaining as well as initiating local chapters. Reports of annual or biennial meetings describe all the steps taken by elected officers-and the many miles they traveled-to shepherd their flocks. To enhance their reputation for sound leadership, supralocal officers not only offered inspiration and programmatic suggestions but also fostered connections among chapters in their orbit. When a local club or lodge ran into trouble, moreover, supralocal leaders could make a real difference, especially in the larger, well-established federations. They might ask neighboring chapters to support faltering units (as in the "Big Brother Aerie" program mounted by the Fraternal Order of Eagles). During economic downturns, national or state officials might forgive shares of local dues; when meeting houses burned down, they orchestrated appeals for aid. Support from above could sustain locals of major federations, whereas disconnected groups or the chapters of weak federations often faltered.

VOLUNTARY FEDERATIONS IN A NATION ON THE MOVE

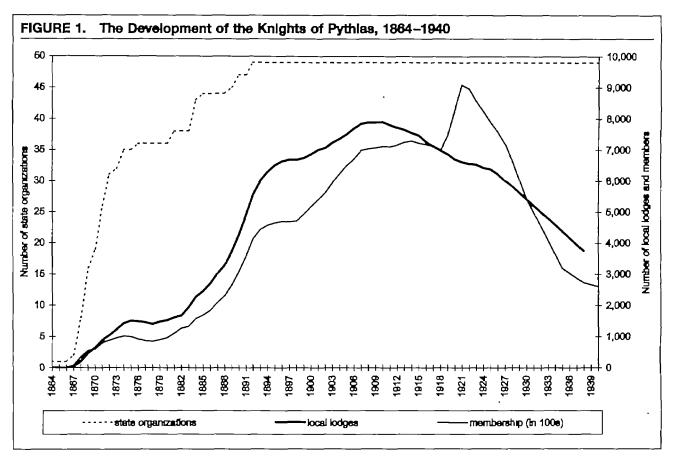
Because large membership federations were central to local communities as well as the nation, we need to know more about how they developed. As a first step, we can dissect the growth of very large membership federations with units at the state level as well as the local and national levels. How were such federations assembled?

We can readily imagine a pattern in which, after national organizers declare a new project, local groups spread and memberships swell; only later do state-level units emerge. In fact, a very different dynamic usually prevailed, namely, an encompassing network of statelevel units formed very early in the life of expanding federations. To illustrate this point, figures 1 and 2 display the slopes of membership growth and local and state organizational trends for the Knights of Pythias, a giant fraternal group that grew from a national project launched at the end of the Civil War, and for the General Federation of Women's Clubs (GFWC), which formed in 1890 as a combination of city groups. Despite disparate origins, both the Knights and the GFWC became institutionalized across many states well before their memberships burgeoned and before most local units were established.

These instances are not atypical. Figure 3 analyzes the relationship of membership growth and cross-state institutionalization for 30 of the 34 federal-state-local groups listed in Table 1 (Appendix A discusses the four omissions). This figure documents a strong relationship between the timing of recruitment of at least 1% of men and/or women and the timing of institutionalization in at least 60% of then-existing states and territories. 10 Extensive institutionalization and large membership growth often occurred around the same time: Half the groups in Figure 3 are on or very close to the diagonal. Yet, the figure clearly shows that 11 of 30 groups (37%) established institutions spanning at least 60% of states before recruiting 1% of adults into membership, even though this was quite an organizational feat at a time when new states and organized territories were joining the nation in thinly populated regions. For example, the WCTU is situated well below the diagonal in Figure 3; it had established "unions" in 60% of states and territories by the early 1880s, considerably before it had enrolled 1% of women (in the late 1910s).¹¹ Only three groups appear far above

¹⁰ We also examined charts with higher thresholds of state organization. Raising the bar to 70% or 75% causes data points to crowd toward the diagonal but does not change the underlying patterns reported here.

¹¹ The founding of a state unit was a significant marker of associa-



the diagonal in Figure 3, and two of these are only partially exceptional. The Woman's Missionary Union established state units across the South before recruiting its initial membership there; later, it added members in other regions before completing a nationwide state network. Starting from the West, the Fraternal Order of Eagles followed a similar pattern. Both federations began by spreading a wide cross-state institutional network within their home regions.

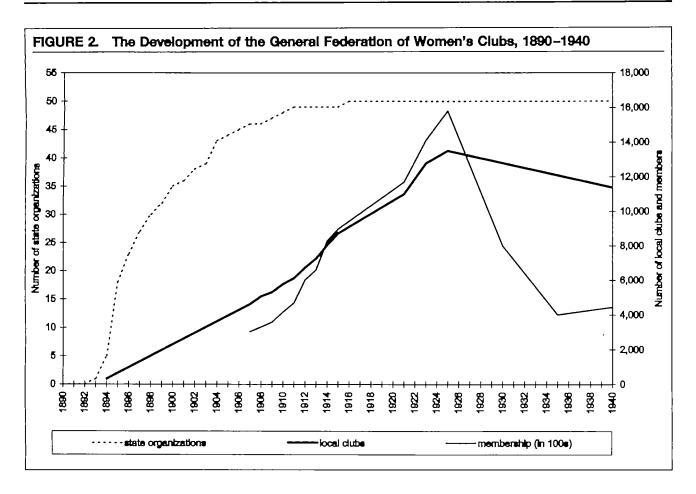
The fact that a wide network of state organizations was usually institutionalized before, or along with, the growth of substantial membership is strong evidence that intermediate as well as national institutions were important to the growth of U.S. voluntarism. Intermediate units were not just after-the-fact window-dressing for American voluntary federations. But what could supralocal institutions—national or intermediate possibly contribute in an era when voluntary groups depended on the willingness of many ordinary men and women to attend meetings regularly and pay dues? To prosper, almost all classic American voluntary federations had to sink strong roots in local communities and neighborhoods. How could apparently elaborate and top-heavy federal arrangements, replete with offices, paid organizers, and subsidized travel for elected leaders, possibly have aided that process?

We theorize that federal frameworks sustained na-

tional presence. Rules varied among federations, but from five to twelve local chapters normally had to be established in the state or territory before leaders could apply to the national center to "charter" a sovereign and representative state organization.

tionwide leadership networks and provided career lines, resources, and incentives for membership organizers. In the modernizing United States, nationally ambitious civic leaders had to spread ideas and recruit members in many places across a vast continent. Associational founders, such as Thomas Wildey of the Odd Fellows and Frances Willard of the WCTU, were constantly on the move, visiting as many locations as possible. Inspiring and effective, they seeded new groups wherever they went (on Wildey, see Stillson 1897, Div. II and III; on Willard, see Bordin 1986, chap. 8). Even so, hundreds to thousands of intermediate leaders had to do most of the work. Only they could tap into indigenous social networks and spread an association's ideals and models into every city and town, casting the net of membership and organization farther and farther afield. In turn, intermediate leaders had to be able to claim credit, coordinate activities, and gain access to portions of local dues. It made sense for national association-builders to foster, as soon as possible, intermediate institutions through which further organizing could proceed.

Embodying shared ideals and standardized associational routines, higher level leaders and institutional centers mattered precisely because the modernizing United States was so geographically and socially dynamic. Nineteenth-century Americans were constantly on the move, especially young men looking for work and entrepreneurial opportunities (Chudacoff 1972; Kopf 1977); indeed, interstate mobility rates reached an all-time peak in the mid-1800s (Hall and Ruggles



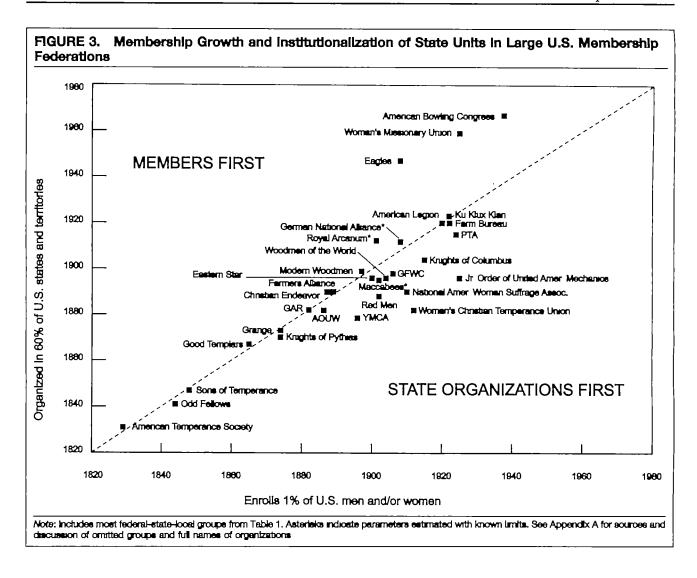
1999). As the United States expanded, translocally organized associations were very appealing (Berthoff 1971, chap. 27). People arriving in new places looked for familiar group meetings; members arranged "traveling cards" or introductions from native lodges or clubs to allow them admittance to cognate units elsewhere. Furthermore, if the new places did not already have familiar groups, authoritative supralocal centers and widely shared knowledge of standardized associational routines allowed members to become instant civic organizers. Strangers who shared the bond of membership in a nationwide association could coordinate their efforts, and local activists could contact leaders at higher levels for guidance and reinforcement. Based on primary testimonies, scenarios such as the following played out again and again. Each excerpt reveals notable feats of collective action by mobile Americans aided by national and state institutional centers and a translocal network of leaders.

The first Odd Fellows' lodge established in the Western Mississippi valley was Travellers' Rest Lodge, No. 1, in the city of St. Louis, for which a charter was granted by the Grand Lodge of the United States on the 18th of August, 1834.... St. Louis was then an insignificant frontier town, with about 7000 inhabitants. There were seven petitioners for this lodge "made up" of transient members then in and about the city: one from England; two from Kentucky; three from Pennsylvania; and one from Maryland. By the time the lodge was instituted all but one of the original signers of the petition had disappeared and others had to be substituted.... Samuel L. Miller of Harmony Lodge,

No. 3, of Baltimore, who was about to remove to Alton, Illinois, was commissioned [by the U.S. Grand Lodge in Maryland] to institute the lodge... At the close of the first year the lodge had 115 members" (reported in Stillson 1897, 355).

Being an account of the introduction of the Order of Knights of Pythias in the Grand Domain of Minnesota by ... David Royal who has been a continuous member of Minneapolis Lodge No. 1 for 27 years. In November 1868 I joined Wilmington Lodge No. 2 Wilmington Delaware. In the spring of '69 I arrived in this City [Minneapolis] and shortly after was employed as car builder for the C.M. & St. Paul Railways at their Shops in this City. In the winter of '69-70 I talked up Pythianism among the workmen and soon had a list of 13 names. I opened up correspondence with Supreme Chancelor Read who sent me some Blank applications for a dispensation [to open a lodge] and full instructions how to procede. About the first of June I received a letter from Supreme Chancelor Read stating that Bro[ther] Jacob H. Heisser of Marrion Lodge No. 1 of Indianapolis Ind had recently arrived in Minneapolis and had also written him about starting a Lodge. I was requested to drop Bro Heisser a line through the Post office and unite our efforts which request was complied

Saturday evening June 25 1870 a preliminary meeting was called [to apply for a charter].... I was chosen President and Bro Heisser [who had recruited two potential members was chosen] Secretary.... Supreme Chancellor Read arrived July 9th 1870.... At Odd Fellows Hall Minneapolis Minn July 11 1870 agreeable to a call of the Supreme Chancelor of the Knights of Pythias Samuel Read of New Jersey a number of Knights and Citizens of



Minneapolis and vicinity assembled for the purpose of organizing a Lodge of the Order (Royal [1890s] n.d.).

There are now [in the 1890s] seventy [women's] clubs in the Nebraska State Federation, and applications for membership constantly arriving. . . . To fully understand what State federation has done, it is well to consider that more than two-thirds of the clubs now auxiliary to it were coexistent with it, and would never have been formed at all but for the permanence of organization and the wider range of thought which union with it and the General Federation promised. In one town of about fifteen hundred inhabitants there had been no literary organization of any kind for ten years previous to this movement. The same is true of many other towns on these prairies, each with its quotient of intelligent, well-educated people, transplanted from the cultured atmosphere of the older States, who had become discouraged by the difficulties of their environment, but who are now developing State pride, and are enthusiastically alive to all the privileges of federated clubs (reported in Croly 1898, 779).

National and intermediate institutions, representatively governed, helped the modernizing United States become a nation of associational organizers as well as a collection of potential joiners. Supralocal centers provided resources and created incentives for leaders to

reach out and help establish new local units, even as these same centers continued to link and inspire the efforts of established chapters. Nationally standardized and shared institutional models also made it possible for every associational member to become an organizer, should need or opportunity arise (as it did for David Royal and Jacob Heisser after they arrived separately in Minneapolis). Supralocal institutions were anything but irrelevant or oppressive bureaucratic overhead. By making it easier for Americans to "combine," even when (actual or potential) "brothers" and "sisters" did not previously know one another personally, these arrangements furthered associational vitality in an expanding and mobile nation. Disparate local groups bubbling up sporadically and informally from below could never, we submit, have achieved the same widespread and stable civic results.

RECONCEPTUALIZING CIVIC VITALITY IN AMERICA AND BEYOND

Our findings reveal the theoretical as well as empirical weaknesses of localist arguments about the roots of American civic voluntarism. Drawing on social

capital ideas, for example, Gamm and Putnam (1999, 551), hypothesize that small American cities were associationally prolific around 1900 because they were more socially enclosed than large metropolises. In fact, cities of all sizes in modernizing America were well connected to one another and to rural hinterlands, and voluntary groups in communities of all sizes were usually part of the same nation-spanning voluntary federations. Indeed, groups located in the smallest cities were the most federated of all. This makes sense because associational organizers tried to spread their network into even very small places, and because Americans on the move wanted to found or join familiar groups near home or work. In the apt words of the quotation regarding the Nebraska State Federation of Women's Clubs, federated chapters linked their participants to "the permanence of organization and wider range of thought ... promised" by "union with" representative state and national associational centers. Classic American voluntary associations were not at all expressions of geographically bounded social enclosure. 12 Multitiered national federations were the key institutional supports of classic American voluntarism because they simultaneously sustained intimate solidarities and connections to wider worlds.

Although often discussed as either top-down or bottom-up creations, classic U.S. membership federations were actually a well-institutionalized combination of both processes. Supralocal leaders articulated shared goals, world views, and identities, even as they spread organizational models and helped local chapters organize, persist, and pursue varied activities. Authoritative federations were effective holding environments for civic life. But they were not bureaucratic because they operated on representative principles and relied on the willingness of ordinary people to join local chapters, attend meetings, pay dues, and elect conscientious officers. Local chapters channeled indispensable resources of money and human energy to the "higher" levels of federations. Constructed as intricate combinations of organizational authority and member engagement, America's great voluntary federations could help geographically mobile citizens create, coordinate, and sustain local voluntary groups as well as simultaneously generate sufficient clout to affect politics or societal mores beyond as well as within local communities. An example such as the Ku Klux Klan

shows that federated associations did not always weigh in on the side of the good. And they did not always hold together or grow very large; like the early United States itself, many broke apart in internecine battles. But voluntary federations that successfully managed their internal conflicts could recruit large numbers of members and exert great influence in national as well as local American life.

Our research not only substantiates an institutional and translocal conception of American voluntarism it also suggests that government and politics need to be integrated into research on the formation of civil societies. Commentators often assert that early Americans created and relied on purely local voluntary groups because the pre-New Deal state was too weak to accomplish collective tasks. This standard wisdom neither notices nor explains the development of nationally federated membership associations that so closely mirrored the representative and federal arrangements of U.S. government and proliferated in close relationship to key episodes of nation-state formation.

The American state was not the sort of bulky, authoritarian bureaucracy whose deleterious effects on civil society Alexis de Tocqueville feared. From the start, U.S. constitutional government was nevertheless pervasive and effective in many ways. The Bill of Rights broke the unity of state and church authority, which allowed citizen-run associations to compete freely with one another and with a plurality of evangelizing churches (Brown 1974; Mathews 1969). The U.S. Congress developed the world's most efficient postal service and a system of postal and transportation subsidies that allowed places far from the Eastern seaboard to become full participants in national life (John 1995). The U.S. state thus furthered both coordination and competition, even as it afforded opportunities for political leverage at the local, state, and national level. What is more, popular struggles to create and preserve the U.S. state taught Americans to use constitutions and representative-federal organizations in many kinds of associational endeavors. Modeling their efforts on those used in political and military mobilizations, civic organizers in the developing American republic assembled voluntary membership federations that tied leader-organizers to one another and linked local groups to larger undertakings.

Emergent U.S. national institutions afforded unique opportunities for citizen-run voluntary federations—in the space between competing, nonofficial churches, on the one hand, and representative government, on the other. Significantly, both translocal voluntary federations and mass political parties took shape in the United States well before national corporations emerged to dominate the market economy. Early in the modernization process, American association-builders synthesized routines borrowed from representative government with world views borrowed from religion (usually ideas blended from Protestant denominations). Remarkably, U.S. voluntary associations achieved this synthesis of representative governance and moral purpose without becoming captives of either church or state. In many complex societies, voluntary

¹² Of course, many classic federations were gender-specific, racially exclusive, and centered on Protestants. Yet, it is important to underline the surprising inclusiveness of some of the largest or most influential voluntary federations, even apart from labor groups. African Americans were at least partially included in the IOGT, the WCTU, and the military veterans' associations; women participated along with men in the IOGT and the Grange. American women also ran many of their own great federations, long before the modern feminist era. Although not willing to accept African Americans, a number of the largest fraternals did include Jewish lodges as well as ethnic-identified lodges that were allowed to conduct activities in many European languages. Surprisingly, the early Odd Fellows allowed Spanish-speaking lodges in Florida (see Stillson 1897, 331). What is more, most classic voluntary federations brought together people from various occupations and classes, a form of inclusiveness arguably not often found in U.S. associations today.

groups have been controlled by powerful states or religious hierarchies, but in America lay citizens took charge.

CIVIC AMERICA FROM PAST TO PRESENT

We have focused on the emergence and initial spread of popularly rooted U.S. membership federations. A more complete overview would note the struggles experienced by almost all dues-based federations during the Great Depression and would highlight the symbiotic partnerships between the federal government and large voluntary federations that helped the nation mobilize for World War I and II, culminating in remarkable voluntary membership spurts after each great conflict. Although particular groups rose and fell, chapter-based federations of the sort that first took shape between the mid-1800s and the 1920s continued to underpin U.S. civil society through the first twothirds of the twentieth century (Skocpol et al. 1999). Only after the mid-1960s did membership federations in general experience sharp decline, as new social movements and professionally run advocacy associations transformed civic life in unprecedented ways.

Today, many Americans are so disillusioned with national government and politics—indeed, with all authoritative institutions—that they are prepared to picture "Tocqueville's America" as a collection of spontaneous local efforts detached from government and politics. Similarly, many who worry that the United States is no longer a nation of voters and joiners hope that civic revitalization can occur apolitically and from the bottom up: Perhaps citizens can redress the nation's ills while organizing children's soccer games; perhaps foundations can solve national problems by dribbling tax-exempt grants to local community groups. But a more accurate picture of America's past suggests the need to think in new ways about contemporary civic dilemmas.

If the United States originally became a civic nation because translocal federations grew parallel to the institutions of national republican government, then Americans today need to worry about the decline of representative democracy as an arena and positive model for associational life. And if classic American membership groups were not just scattered local creations, but were linked into well-institutionalized national networks that used membership dues to support elected leaders who had authority and incentive to organize large numbers of fellow citizens, then perhaps revitalization of American civic life today will take more than efforts by self-appointed professional advocates operating out of Washington, D.C., or New York City, and more than the disconnected efforts of small groups operating apart from national politics and translocal movements. Perhaps the best aspects of America's civic past are not being perpetuated or replaced in today's civic world, where market models are displacing representative arrangements, and where

civic leadership no longer entails popular mobilization or the organization of interactive associations.¹³

An institutional approach to civic life suggests that state, politics, and society are—for better or worse inevitably intertwined. From this perspective, the key to civic health lies not in local face-to-face interactions alone but in the nature of connections between powerful supralocal institutions and local or particular endeavors (for further arguments to this effect, see Berman 1997; Eckstein 1961; Evans 1997; Foley and Edwards 1999; Tarrow 1996a). Democratic accountability and balances of power are also critical, for "when bad men combine, the good must associate, else they will fail, one by one, an unpitied sacrifice in a contemptible struggle" (Knights of Labor of North America 1883, title page). Institutional and power realities shaped America's rich civic heritage, and they remain relevant today. Consequently, Americans who better understand their civic past may need to reimagine their democratic future and look to revitalize shared and representative institutions not just in national politics but in associational life as well.

APPENDIX A: DATA ABOUT LARGE U.S. MEMBERSHIP ASSOCIATIONS

Multiple primary and secondary sources were used to understand the origins and organizational arrangements of each large U.S. voluntary association listed in Table 1. In addition to the scholarly studies and official associational histories listed below, we examined groups' constitutions, which are listed separately only when they establish points not documented elsewhere. For figures 1–3, the dates for state foundings and national membership and chapter trends in most cases were determined from associational records or convention proceedings. Otherwise, the secondary sources below that are marked with an asterisk (*) were used for such data, or the best available estimates.

In Figure 3, estimates for the Maccabees, Royal Arcanum, and German National Alliance were made from substantial but incomplete data on membership and state foundings. Figure 3 omits 4 of the 34 federal-state-local associations in Table 1. The American Anti-Slavery Society, the Colored Farmers' Alliance, and the American Protective Association were excluded from the figure because they were short-lived, regionally centered, and never reached 60% of states; for the American Automobile Association, we have not yet obtained founding dates for state associations.

American Anti-Slavery Society: American Anti-Slavery Society 1834; Myers 1961.

American Automobile Association: American Automobile Association 1952; Partridge 1952.

American Bowling Congress: Matzelle and Schneider 1995; American Bowling Congress 1999.

American Farm Bureau Federation: Hansen 1991; Kile 1921. American Federation of Labor: Taft 1957.

American Legion: Pencak 1989.

American Protective Association: *Desmond 1912; *Kinzer 1964.

¹³ For arguments along these lines, see Ganz 1994, Rosenstone and Hansen 1993, Schier 2000, Skocpol 1999, and Weir and Ganz 1997. For a variety of other perspectives on contemporary civic transformations, see Berry 1999, Minkoff 1997, Putnam 2000, Schudson 1998, and Wuthnow 1998.

American Red Cross: Davidson 1950a, 1950b.

American Temperance Society: Krout 1925.

Ancient and Accepted Free Masons: Stillson 1926.

Ancient Order of United Workmen [AOUW]: Stevens 1899, 128-30; Upchurch 1887.

Benevolent and Protective Order of Elks: Nicholson, Donaldson, and Dobson 1978.

Boy Scouts of America: Macleod 1983; Murray 1937.

Christian Endeavor. Clark 1906; United Society of Christian Endeavor 1892.

Colored Farmers' Alliance: *Abromowitz 1950; Dunning 1891; *Holmes 1975.

Congress of Industrial Organizations: Zieger 1995.

Farmers' Alliance: Dunning 1891; Hicks 1935; McMath 1975. Fraternal Order of Eagles: Fraternal Order of Eagles 1913; O'Reilly 1904.

General Federation of Women's Clubs: Croly 1898; Wells 1953.

General Union for Promoting the Observance of the Christina Sabbath: General Union 1828; *John 1995, chap. 5.

German National Alliance: *Child 1939; *Committee on the Judiciary, U.S. Senate 1918.

Grand Army of the Republic: Beath 1889; McConnell 1992. Improved Order of Red Men: Lichtman 1901.

Independent Order of Good Templars: Newton 1869; Turnbull 1901.

Independent Order of Odd Fellows: Independent Order 1844; Stillson (1897).

Junior Order of United American Mechanics: Deemer, Shanor, and Deily 1896.

Knights of Columbus: Kaufman 1982.

Knights of Labor of North America: Knights of Labor 1883; Voss 1993.

Knights of Pythias: Carnahan 1890.

Ku Klux Klan: *Chalmers 1965; *MacLean 1994.

Loyal Order of Moose: Fuller 1918; Loyal Order of Moose

Maccabees: Knights of the Maccabees 1894, *1901; *Stevens 1899, 151-4.

Modern Woodmen of America: Modern Woodmen 1999; Stevens 1899, 157-9.

National American Woman's Sufferage Association: Banaszak 1996; *Lemons 1973, 52-3. These sources disagree about NAWSA's size; we accept the latter's estimate that it briefly ballooned to about 2 million just as the constitutional amendment enshrining female suffrage was adopted in 1920.

National Congress of Mothers [PTA]: National Congress of Parents and Teachers 1947.

Nobles of the Mystic Shrine: Melish et al. 1919.

Old Age Revolving Pensions, Ltd. (Townsend movement): Holtzman 1963.

Order of the Eastern Star: Engle 1912; Stillson 1926, 857-68. Patrons of Husbandry (National Grange): Howard 1992; Nordin (1974).

Royal Arcanum: *Royal Arcanum 1901; *Stevens 1899, 186-7.

Sons of Temperance: Beattie 1966; Hodges 1877.

Washington Temperance Societies: Grosh 1842; *Maxwell 1950.

Woman's Christian Temperance Union: Bordin 1981; Mezvinsky 1959; Tyler 1949.

Woman's Missionary Union: Allen 1987.

Woodmen of the World: Larson and Cook 1991.

Young Men's Christian Association: Hopkins 1951.

APPENDIX B: CITY DIRECTORY DATA

Tables 3 and 4 are based on counts of membership-based associations listed in city directories for the dates specified in each table, and the definitions used to classify groups are given in the note to Table 3. Around 1900, U.S. city directories were compiled by local companies that came and went, which means they did a reasonable job of including groups of local interest but did not always cover exactly the same kinds of groups in equal detail. Although there was considerable imitation in formats from city to city and company to company, in order for valid comparisons to be made across time and place, directories must be scrutinized carefully to make sure they include the same kinds of groups. We have done this and taken some steps to correct for blatant omissions.

Labor groups were not always included in these directories. The Boston directory of 1910–11, for example, simply omitted most trade unions; we used instead a full list published by the Massachusetts Bureau of Statistics (1910). Scranton, Pennsylvania, directories included unions in 1900 but in 1910 referred readers elsewhere for a full listing. Without access to the other source, we estimated Scranton unions in 1910 as the 1900 number (which was very close to the 1920 number). We have not been able to correct for obvious omissions of unions in other directories, such as those for Des Moines, Iowa, and Troy, New York.

African American associations were erratically included in the directories for Rome, Georgia. Our counts for churches in Rome include African American congregations, which were consistently listed under a separate heading in every directory we used for the 1890 to 1920 period. But African American fraternal groups were listed in only one of the four directories used for Table 4, so we excluded them in order to maintain comparable coverage and to avoid overestimating unstable groups in this city.

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The Sovereignless State and Locke's Language of Obligation

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odern liberal states are founded on individual rights and popular sovereignty. These doctrines are conceptually and historically intertwined but are in theoretical and practical tension. Locke's political theory is a source for proponents of both doctrines, and the same tension that runs through modern liberal thought and practice can be found in his theory. Rather than define the state in terms of a single sovereign authority, Locke constructs a sovereignless commonwealth with several coexasting claimants to supreme authority. He rejects sovereignty as what unifies the state, and he wants to replace the discourse of sovereignty theory with a language of obligation that will help bind together the sovereignless state. This language permits its adherents to articulate the reasonable basis and limits of political power. An understanding of Locke's sovereignless state helps us better comprehend the tensions embodied in discourses about individual natural rights, popular sovereignty, and governmental authority heard in the liberal state.

odern liberal states are founded on doctrines of both individual rights and popular sovereignty. In the United States, citizens can appeal to the Declaration of Independence and its proclamation of inalienable natural rights as well as to the Constitution and the language of "we the people" forming a more perfect union. Although conceptually and historically intertwined (Tully 1983, 11–3), these two doctrines are in theoretical and practical tension. This tension is visible in attempts within the political and legal arenas to define and adjudicate the spheres of public and private as well as in debates within contemporary political theory over the relationship between the individual and the community.

John Locke's political theory is a source for proponents of both individual rights and popular sovereignty, and the same tension that runs through modern liberal thought and practice can be found in his theory and interpretations of it. Long celebrated as the source of the natural rights theory that undergirds the liberal state, Locke has more recently been criticized by communitarian thinkers and others for what is described as the excessive individualism of his theory. Bellah et al. (1985, 20, 143) identify Locke as "the key figure" in the development of the modern individualist strand that is the source of the "first language" of American politics, a dominant discourse that competes with biblical and republican traditions. Sandel (1996, esp. 22-3) speaks of a similar tension in the American political tradition between a republicanism concerned with the public good and a liberal theory of rights as "trumps" derived in part from Locke. Glendon (1991) describes Locke as a source of the dominant American political language of "rights talk." A contrasting interpretation views Locke as erring in the opposite direction and criticizes him for putting all the authority in the hands of the people and not sufficiently guaranteeing the rights of minorities and individuals. "Locke had no hesitation in allowing individuals to hand over to civil society all their natural rights and powers," writes Macpherson (1962, 256), and several prominent scholars agree (see Cox 1960, 120–2; Shapiro 1986, 116, 125–6; Strauss 1953, 233). What is the locus of supreme authority in Locke?

The tension in Locke's theory between the competing claims to ultimate or "sovereign" authority is not the result of confusion but the outcome of his axiomatic doctrine of the natural freedom of the individual. Rather than define the state in terms of a single sovereign authority, Locke constructs a sovereignless commonwealth with several coexisting claimants to supreme authority: the naturally free individual, the people or society, the legislative, and the executive. Locke rejects sovereignty as what unifies the state, and he wants to replace the discourse of sovereignty theory with a language of obligation that will help bind together the sovereignless state. The political reform he contemplates requires a conceptual and linguistic reform. In the Preface to Two Treatises Locke ([1689b] 1988, 138) asks his critics to reflect that "there cannot be done a greater mischief to prince and people, than the propagating wrong notions concerning government." An adequate political language will enable governors and governed to articulate the basis and limits of political power.

By propagating correct notions concerning government Locke hopes to encourage the legitimate exercise of political power. Furthermore, as Connolly (1974, 1) reminds us, "the language of politics is not a neutral medium that conveys ideas independently formed; it is an institutionalized structure of meanings that channels political thought and action in certain directions" (see also Pitkin 1967, 1). Institutional mechanisms check and channel that power, but the political machine does not run of itself. A shared language of obligation is a centripetal force that helps the political machine work without a single ultimate authority. An understanding of Locke's sovereignless state helps us better comprehend the theoretical and practical tensions embodied in the simultaneous discourses of individual natural rights, popular sovereignty, and governmental authority heard in the liberal state.

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LOCKE AND SOVEREIGNTY THEORY

Locke is widely believed to be a theorist of popular sovereignty. The editor of the standard edition of the Two Treatises explains that Locke's conception of the fiduciary nature of political power "secures the sovereignty of the people," for it is the people "who decide when a breach of trust has occurred" (Laslett 1988, 115; see also Tully 1983, 4-5; Wootton 1986, 44). Franklin (1978) agrees that Locke's doctrine of popular sovereignty is expressed as the right to resistance, and Gough (1973, 46-7) states that the community, or the people, is clearly sovereign in Locke's system, even though the philosopher does not expressly say so. Simmons (1993, 159 n.) is more reluctant to interpret from Locke's silence, writing that "there is a fairly clear sense in which he takes the people (society) to be sovereign."

In fact, Locke never calls the people "sovereign." He states that "there remains still in the people a supreme power to remove or alter the legislative, when they find the legislative act contrary to the trust reposed in them." Yet, he quickly assures us that while the people are latently "supreme," they are not actively such as long as government subsists: "The community may be said in this respect to be always the supreme power, but not as considered under any form of government, because this power of the people can never take place till the government be dissolved" (II, 149). The people is "supreme" only when government is dissolved; they are subordinate to the "one supreme government" (II, 89) while it exercises legitimate power. And if the dissolution of government often or even necessarily entails the dissolution of society and returns individuals to the state of nature, as Locke sometimes seems to argue (II, 121, 205, 219; see Simmons 1993, 167-72; Strauss 1953, 232 n.), then the people are in actuality perhaps never supreme. Thus, Polin (1960, 231, 160) wants to argue that in Locke "the people always remain—in the last analysis—the supreme power," but ultimately he concedes that "Locke participates in the myth of the sovereign people that will hold sway over the entire history of modern States."

Other interpreters continue to search for a Lockean sovereign within the state. While government endures, "supreme power" resides in the legislative (II, 149). Gough (1973, 118) therefore distinguishes between the normal "legal" sovereignty of parliament and the extraordinary extralegal or "political" sovereignty of the people in Locke's theory (see also Seliger 1968, 326). Yet, Locke also admits that the executive—who possesses the right of prerogative, may share in legislative power through the veto, and wields the power of government when the legislative is not in session—"in a very tolerable sense may also be called Supreme" (II,

151). Lamprecht (1962, 149) and Seliger (1968, 327) therefore contend that Locke develops a theory of shared or divided sovereignty, whereas Weston and Greenberg (1981) find in him a theory of "co-ordination in law-making" that means effective parliamentary sovereignty.

Who, then, is "sovereign" in the Lockean commonwealth? Is it the people? The legislative? Or even the executive? Aaron (1955, 281) notes the "considerable difficulty" of answering this question: "No doubt, the proper answer to the question is that the people are sovereign," he avers, but apparently there is some doubt, since he confesses that "Locke's political theory is devoid of any clear-cut theory of sovereignty." Barker (1962, xxv-xxvi) nominates the same candidates for Lockean sovereign and then complains that the theorist is simply unclear on the point. Finally, Lamprecht (1962, 148) laments that a serious flaw in Locke's theory is "his failure to grapple with the problem of sovereignty."

Locke's Rejection of Sovereignty

Rather than fail to grapple with sovereignty, Locke tacitly rejects the concept. He intentionally avoids the term in articulating his own theory as an implicit criticism of the theory of sovereignty he confronted. In particular, Locke rejects sovereignty theory's understanding of political power as the exclusive possession of certain ultimate powers. His thought does not accommodate a traditional sovereign, and much of the disagreement over his theory arises from trying to find a sovereign where one does not exist.

When writing in his own name about legitimate political power, Locke speaks of "supreme power." He restricts the term "sovereignty" to his critique of Filmer, using it throughout the First Treatise (see Grant 1987, 74–9; Kendall 1965, 95; Lamprecht 1962, 148).² On the rare occasions in the Second Treatise when the term is used, Locke is directly or indirectly disputing Filmer ([1675] 1991). For instance, Locke (II, 69, 83; see also II, 4, 61, 108, 115) argues that fathers do not possess the "sovereign power of commanding," "dominion" over their children, or "absolute sovereignty and power of life and death" over their wives or children, for which Filmer contends. When he returns

¹ All citations are to Two Treatises of Government (Locke [1689b] 1988), by treatise and section number, unless otherwise noted. References to Essay Concerning Human Understanding (Locke [1690] 1975), hereafter Essay, are by book, chapter, and section number. Spelling and capitalization are modernized, and original italics are omitted throughout, but original punctuation and usage are maintained.

² In his mature political writings, Locke adopts the same strategy as in Two Treatises. When he speaks in his own name, as in the Letter Concerning Toleration, Locke ([1689a] 1983, 48) defines the legislative as the "supreme power." He also uses the term "civil magistrate," which he also calls "supreme" (pp. 48-9; see Two Treatises, II, 89). In his defense of this work in the 1690 Second Letter on Toleration, Locke (1823, 6: 76) follows the usage of his opponent, for example, when he speaks of "the magistrates of the world, or civil sovereigns, as you think it more proper to call them." He does the same in the 1692 Third Letter on Toleration (pp. 158 ff.). The early unpublished Tracts on Government are more difficult to categorize (see Locke [c. 1660-62] 1967, 125, 187, 212-3), and it is unclear whether Locke accepts a Bodinian or other theory of sovereignty there. The Tracts are often described as "conservative" and in conflict with the theory of the Two Treatises (see Abrams 1967, Ashcraft 1986, 88-92; Dunn 1969, chap. 2; Simmons 1993, 123-36; Tully 1983).

to the exposition of his own theory of political power, however, Locke eschews the term.

Filmer's version of sovereignty theory is the immediate target, but Locke's reasons for avoiding "sovereignty" go beyond Filmer's patriarchalism to sovereignty theory in general. Locke (I, 9) summarizes Filmer's theory of political authority near the outset of the First Treatise: "This fatherly authority then, or right of fatherhood, in our a[uthor]'s sense is a divine unalterable right of sovereignty, whereby a father or a prince hath an absolute, arbitrary, unlimited, and unlimitable power, over the lives, liberties, and estates of his children and subjects."3 Although the most prominent claim of Filmer is that sovereignty is inherited from Adam, given the difficulties of locating the rightful heir of Adam, among other problems, he ultimately contends that we can identify rightful rulers by their possession of certain "sovereign" powers as a form of "dominion" (Tarcov 1984, 9-22). For example, Filmer ([1675] 1991, 7, see 12, 55) argues that the patriarchs were sovereigns since they possessed the power of life and death and of making war and peace, which "are the chiefest marks of sovereignty that are found in any monarch." Sovereigns are recognized not by their paternity but by their power.

By defining sovereignty in terms of the possession of certain powers, Filmer self-consciously follows Bodin ([1566] 1945, [1577] 1962; see Filmer [1675] 1991, 161-4). Locke (I, 8) recognizes that debt when he points out that Filmer discusses the power of kings "in Bodin's words." The concept of sovereignty gained prominence in English political thought largely through the introduction of Bodin's theory in the late seventeenth century (Allen 1928, chap. 10; Tuck 1993, 260-2). Bodin's theory of sovereignty represents the culmination of the modern natural law theory, which supplanted medieval theories of the state (Gierke 1957, 36–7). Bodin ([1577] 1962, 84) defines "majesty" or "sovereignty" as "the most high, absolute, and perpetual power over the citizens and subjects in a commonwealth." Sovereignty is defined and identified by the possession of five powers: creating magistrates, proclaiming laws, declaring war and peace, receiving final appeals, and wielding the power of life and death (Bodin [1566] 1945, 172-3). The sovereign is absolute and not subject to any (human) law and is the sole judge, not only of the civil law he himself makes but also even of the natural law by which he is bound and by which his own actions are to be judged. Finally, Bodin ([1577] 1962, 91-3) strenuously argues that sovereign power is indivisible. This notion caused perhaps the greatest difficulty when it was grafted onto English constitutional thought and its traditional, although highly contested, conception of shared or divided powers.4 Bodin's sovereignty theory was viewed by many, including Coke, as foreign to English constitutionalism (Burgess 1992, 165–6, 196–8). Nonetheless, the concept of sovereignty became increasingly common in the debates over political power from the Civil War to the Glorious Revolution, a period Locke ([1703] 1997b, 351–2) identifies as one in which the question of "the rise and extent of political power" had been "so bandied amongst us."

Locke's political theory has been widely interpreted as in part a reaction to the debate over the locus in sovereignty, but confusion about his position arises largely from trying to read him in terms of the sovereignty theory he rejects. Figgis (1896, 240) points in the right direction: "Locke's treatise is expressly directed against the notion that there is any sovereign power in the state.... The more closely Locke's treatise is studied, the more clearly it will be seen that it is an attack directed far more against the idea of sovereignty than against the claims of monarchy." Yet, Locke does not "expressly" argue against sovereignty. He makes a tacit argument. We must begin from his silence.

Several scholars have attempted to interpret Locke's silence and conclude that he avoids the term "sovereignty" because of its Hobbesian association, but contradictory interpretations have been offered about his avoidance of the term. Numerous writers in the midseventeenth century either contended against the Leviathan or avoided the dangers that came from referring to Hobbes or using his terminology (Ashcraft 1986, esp. 570-1; Kenyon 1977, 16-7). Although Locke does not directly discuss Hobbes,5 there is at least an indirect link to his discussion of sovereignty, since Filmer examines Hobbes's theory at length in a writing to which Locke (I, 14) refers as containing the sum of Filmer's argument "for Adam's sovereignty, and against natural freedom." Filmer ([1675] 1991, 184, and see 191-4) criticizes Hobbes's argument for sovereignty from consent but has no objection to his absolutism, prefacing his observations on Hobbes's theory by commenting that he read his works "with no small content, ... consenting with him about the rights of exercising government."6

We know that Locke does not consent with Filmer about the origin or rights of political power, but his relation to Hobbes is less clear. On the one hand, Gough (1973, 47) argues that Locke establishes the sovereignty of the people and explains that he does not actually call the people "sovereign" in order to steer clear of Hobbes. On the other hand, Cox (1960, 108–10, 120) contends that the individual is as wholly

³ In his early "An Essay on Toleration," Locke ([1667] 1997a, 135–6) describes the claim to monarchy by divine right in similar terms: "that the sole, supreme, arbitrary power and disposal of all things is and ought to be by divine right in a single person."

⁴ Bodin ([1577] 1962, 96-8) argues that the monarch is the sole sovereign in the English system. His theory was initially used in England by the royalists (see Tuck 1993, 260-2).

⁵ Locke (II, 98) appears to allude to Hobbes when he writes that the requirement of unanimity "would make the *mighty Levlathan* of shorter duration, than the feeblest creatures." Hobbes ([1651] 1994b, 210) explains his use of "leviathan" just before claiming that a properly designed state could be immortal. Compare Laslett 1988, 71. Locke also refers to Hobbes and his Leviathan in the *Ersay* (I.3.5). I will return to Locke's rejection of sovereignty in an apparently Hobbesian version when I discuss the contrast he makes between the state of nature and state of war.

⁶ Bodin is a source of the sovereignty theory of both Filmer and Hobbes. Hobbes ([1650] 1994a, 166-8; [1651] 1994b, 115-6) enumerates basically the same powers of sovereignty as Bodin and explicitly follows him in arguing against its divisibility.

absorbed into the sovereign state in Locke's theory as in that of Hobbes, and he therefore conjectures that Locke avoids the term because he is in fact a Hobbesian. In contrast, Vaughn (1925, vol. 1, 134) agrees that Locke's abstention is in part a reaction to Hobbes, but, like Figgis, he contends that his work is "in fact, an assault not only upon the sovereignty of *Leviathan*, but upon the very idea of sovereignty.... At bottom, he is as much against the 'sovereignty of the people' as against that of the oligarchy or the tyrant."

Examination of what Locke does say about sovereignty reveals why he rejects the concept in any of its versions. His break with the core idea of sovereignty theory, that sovereignty is defined and identified by the exclusive possession of certain ultimate powers, can be seen in a passage in the First Treatise in which he juxtaposes the terms "sovereignty" and "supreme power" in a way that Cox (1960, 109) takes to indicate their synonymy. Against Filmer's contention that political authorities are recognized by their possession of the "absolute and sovereign authority" of life and death and other powers, Locke (I, 129) argues that the possession of such powers "is not a certain mark of sovereignty." In the passage in question, he takes up the claim that the possession of the power of war and peace is an unerring "mark" of sovereignty in a way that undercuts the entire theory of sovereignty.

But making war and peace are marks of sovereignty. Let it be so in politic societies. May not therefore a man in the West-Indies... make war and peace... without being a sovereign, an absolute king over those who went with him?... War and peace cannot be made for politic societies, but by the supreme power of such societies.... In voluntary societies for the time, he that has such a power by consent, may make war and peace, and so may a single man for himself, the state of war not consisting in the number of partisans, but the enmity of the parties, where they have no superior to appeal to (I, 131; emphasis added).

At first glance, Locke may appear to use "sovereign" and "supreme power" interchangeably. He does indeed attribute to the "supreme power" of his positive theory some of the same powers traditionally ascribed to a "sovereign," the most important of which is the power of life and death (see II, 90, 93, 139). Even his stipulated definition of "political power" is phrased in terms of that power: "Political power then I take to be a right of making laws with penalties of death, and consequently of all less penalties" (Π , 3). Along with Cox, Seliger (1968, 326) concludes that in Locke's "usage, 'supreme power' means exactly the same as 'sovereign power.' He did not shun the term 'sovereignty." Laslett reaches a similar conclusion (see editor's notes to Locke [1689b] 1988: I, 129, 131; II, 11, 88). Yet, closer examination reveals that Locke himself shuns the term. In the passage in question, as elsewhere, he uses "sovereignty" solely when repeating Filmer's argument, and he uses "supreme power" when speaking in his own name. The purport of his change in usage in the above passage is intimated by his pairing of "sovereign" and "absolute king" in the Filmerian sentence and "supreme power" and "politic societies"

in the Lockean statement. Filmer's "sovereign" or "absolute king" has "an absolute, arbitrary, unlimited, and unlimitable power" as "lord or proprietor of every thing" (I, 9). In contrast, Locke's "supreme power" holds power in trust from a "politic society" based on "consent."

Although Locke allows that the supreme power exercises powers traditionally ascribed to the sovereign, he points to a completely different basis for that power, one which will limit it and make it reasonable. He argues against Filmer that the powers of life and death and of war and peace, and other powers understood as the exclusive possession of sovereigns, are exercised by inferior magistrates and others (I, 129-31). More important, he claims that these powers are rightly exercised by individuals. Not only may a man in the West Indies make war and peace without being a political authority, but also "so may a single man for himself" exercise the powers of war and peace where he has "no superior to appeal to" (I, 131). Locke attacks sovereignty theory at its root: "The actual making of war or peace is no proof of any other power...and this power in many cases any one may have without any politic supremacy" (I, 132). All individuals possess "sovereign" powers where there is no recognized supreme power. Locke's switch of terms represents a different conception of the very nature and basis of political authority.

The "Sovereign" Individual

Locke rejects the traditional sovereign because for him political power derives from the rights and powers of naturally free individuals who cannot reasonably be conceived to have relinquished their rights to a sovereign. "To understand political power right, and derive it from its original, we must consider what state all men are naturally in" $(\Pi, 4)$. The state of nature reveals the moral autonomy of the individual. The natural state of mankind is "a state of perfect freedom" and also a state of equality, "wherein all the power and jurisdiction is reciprocal" (Π , 4; see Π , 54). Locke suggests his rejection of traditional sovereignty in this context when he claims it is evident that individuals are equally born "without subordination or subjection" unless there is a manifest declaration by "the Lord and master of them all" that one person has "an undoubted right to dominion and sovereignty" (II, 4). No earthly being possesses exclusive dominion and sovereignty by nature; thus, everyone possesses reciprocal power and jurisdiction. If the individual possesses "sovereign"

⁷ Locke's explanation in the First Treatise for why "War and peace cannot be made for politick societies, but by the supreme power of such societies" foreshadows his argument that the supreme power of a political society is the majority or its representative: "because war and peace, giving a different motion to the force of such a politic body, none can make war or peace, but that which has the direction of the force of the whole body, and that in politic societies is only the supreme power" (I, 131). In the Second Treatise, Locke describes the majority as the "greater force" that moves the political "body" (II, 96)

powers by nature, then there is no "sovereign" in the traditional sense.

Locke's most radical break from sovereignty theory is to argue that individuals by nature possess the powers of judging and executing the law of naturepowers traditionally ascribed to the sovereign alone. By nature individuals have a right to self-preservation and a right to preserve all mankind (II, 6). Invasions of those rights are contrary to the law of nature, and Locke maintains there must be an executor of the law of nature lest that law "be in vain." Since "naturally there is no superiority or jurisdiction of one, over another," he argues that "every one has a right to punish the transgressors of that law to such a degree, as may hinder its violation" (Π , 7). Every individual is the ultimate judge of when to execute the law of nature and to what degree, even with death (II, 12). When entertaining the objection that "it is unreasonable for men to be judges in their own cases," Locke admits the inconvenience but reiterates his position that every individual has "liberty to be judge in his own case" $(\Pi, 13).$

The right to exercise the natural power to judge and execute is based on the "property" Locke states every individual has in his person: "Every man has a property in his own person. This no body has any right to but himself" (Π , 27; see Π , 173). With the right comes power. The individual has "by nature a power, not only to preserve his property, that is, his life, liberty and estate, against the injuries and attempts of other men; but to judge of, and punish the breaches of that law in others" (II, 87). The same conclusion concerning the ultimate right of the individual to judge follows from Locke's argument that human beings are God's property: "For men being all the workmanship of one omnipotent and infinitely wise maker; all the servants of one sovereign master, sent into the world by his order and about his business, they are all his property whose workmanship they are, made to last during his, not one another's pleasure" (II, 6; see I, 39). How Locke can maintain simultaneously that individuals have a property in their persons and that God owns that property is unclear, and some scholars maintain that the "workmanship argument" precludes a doctrine of inalienable rights (Glenn 1984, 80-2; Kendall 1965, 68; Polin 1960, 211; Simmons 1993, 109). Perhaps Locke's position on the ultimate right to judge is derived from both his divine workmanship argument and his consideration of the natural right to selfpreservation (Grant 1987, 66–72). Whether or not God is considered as our "sovereign master," no one possesses by nature an "undoubted right to dominion and sovereignty" over others (II, 4). In either case, Locke directs his argument against sovereignty theory.

The doctrine of rights as property in one's person further undermines sovereignty theory by reconfiguring the relationship between property rights and political power. In sovereignty theory, political power is viewed as a form of property. As Locke explains in the *First Treatise*, the tradition of primogeniture reinforces the mistaken opinion of the inheritance of "both estate and power; and that the inheritance of both rule over men

and property in things, sprang from the same original" (I, 91; Resnick 1992, 110). Locke's opposing argument about individuals having property in their person depends upon a distinction between transferable and nontransferable property that he uses to defeat both Filmer's patriarchalism and sovereignty theory more generally. "Locke's positive argument in the Second Treatise also turns on what kinds of rights and powers are transferable and by what means, rather than on the traditional catalog of powers considered to be marks of sovereignty" (Grant 1988, 58; see also Glenn 1984, esp. 94-5). Neither paternal nor political power is a form of property, Locke argues: "A father cannot alien[ate] the power he has over his child" (I, 100), and he cannot transfer any political power he may have through inheritance or otherwise (I, 93). The basis of all political authority is consent, not inheritance (I, 94). If individuals possess their rights as property in their persons, then all political power must be derived from the natural power of the individual and held through consent and in trust (see Π , 127–8).

In entering civil society, the individual agrees to "give up" a portion of his natural powers. The power of acting for the preservation of self and of mankind "he gives up to be regulated by the laws made by the society, so far forth as the preservation of himself, and the rest of that society shall require" (II, 129). The natural executive power of punishing infractions of the law of nature "he wholly gives up" (II, 130). Locke uses "gives up" in the sense of renouncing the exercise of a power and yielding to someone else (see Oxford English Dictionary, s.v. "give up"). The underlying power cannot be transferred or alienated. "For no man, or society of men, having a power to deliver up their preservation, or consequently the means of it, to the absolute will and arbitrary dominion of another . . . they will always have a right to preserve what they have not a power to part with; and to rid themselves of those who invade this fundamental, sacred, and unalterable law of selfpreservation" (II, 149; emphasis added). The ultimate right to judge and execute is what we would term an inalienable right.8 "This freedom from absolute, arbitrary power, is so necessary to and closely joined with a man's preservation, that he cannot part with it, but by what forfeits his preservation and life together" (II, 23). The right of the individual to judge the "supreme power" of government ensures that the individual remains "sovereign."

The theory of the natural authority of the individual "will seem a very strange doctrine to some men" (II, 9), Locke admits. And indeed it is. Although he claims to recur to "the old way" of making governments "by contrivance, and the consent of men" (I, 4, 6), his theory of the natural powers of the individual is a break from previous consent theory in both its English con-

The attribution of a doctrine of "inalienable" rights to Locke is problematic, in part because he does not use the term in his political theory (Glenn 1984, 80-2; Kendall 1965, 68; Polin 1960, 211; Simmons 1993, 109). Nonetheless, based on what he does argue, Locke must at least be committed to the position that individuals cannot alienate the right to judge when their trust is violated and to resume the exercise of their natural powers.

stitutionalist and natural law versions (see Ashcraft 1986; Höpfl and Thompson 1979; Pocock 1957; Resnick 1984; Zuckert 1994, esp. 64-5). In both versions, political power was conceived as originating in a compact between the people and the ruler, the ruler and his heirs then held perpetual authority over his subjects and their descendants (see Gierke 1957, 44-61; Kantorowicz 1957, esp. 223-31). Where an active right by the people to resist tyranny was admitted, for example, in the populist tract Vindiciae contra tyrannos, it was held to belong to the representatives of the people and not the people itself (Mornay [?] [1579] 1969, 189ff.). Lawson was the most prominent theorist prior to Locke to find in the people a right to resist, and Franklin (1978) argues that Locke follows Lawson (see also Skinner 1978, 247-8, 338-9). Yet, Locke takes a decisive step beyond Lawson by arguing that not only the people but also every individual possesses the right to resist.

Locke ultimately grounds the right to resist on the individual's right to exercise the natural powers of judgment and execution. He frames his doctrine of resistance in answer to the "old question... But who shall be judge?" posed by Hunton, a defender of monarchical sovereignty who preached passive resistance for want of an earthly judge above the sovereign (see Hunton [1643] 1986, 187–8). The first time Locke poses Hunton's question he answers in his own name: "Of that I my self can only be judge in my own conscience" (Π , 21). We glimpse here the revolutionary Locke revealed by Ashcraft's (1986) biography. "And where the body of the people, or any single man, is deprived of their right . . . and have no appeal on earth. there they have a liberty to appeal to heaven, whenever they judge the cause of sufficient moment" (II, 168; emphasis added). The right to resist "is an inalienable right; without it a man would be a slave subject to absolute power" (Grant 1987, 173; see also Andrew 1988; Glenn 1984).

Locke's reason for usually speaking of popular instead of individual resistance has more to do with prudence than right. Thus, although he insists upon the right of an individual or a minority to resist a majority they judge to be unjust, he admits that they may not act because the exercise of their right is not likely to succeed "where the body of the people do not think themselves concerned in it" (II, 208). Resistance is more likely to occur when the mischiefs of illegitimate power "have extended to the majority of the people" or at least threaten everyone (Π , 209; see Π , 223–6, 242), that is, when it becomes reasonably prudent to act upon that right. Locke's doctrine that the powers of judging and executing the law of nature ultimately reside in the individual distinguishes his theory from even his most radical predecessors. William Penn foresaw the implication of such a theory when he wrote during the exclusion crisis of the fundamental "right and title to your own lives, liberties, and estates: in this every man is a sort of little Sovereign to himself" (quoted in Ashcraft 1986, 208).

THE SOVEREIGNLESS STATE

The sovereignless state of nature produces a sovereignless state. Rather than speak in terms of an ultimate, absolute, and incorrigible sovereign, Locke organizes the civil state so that several claimants to ultimate power can coexist without a common authority in theoretical harmony, if in practical tension. These claimants are the same candidates that Locke's interpreters have nominated for sovereign: the naturally free individual, society or the people, the legislative, and the executive. Yet, none of them are sovereign in the traditional sense of being a single, absolute, and incorrigible authority; they are contingently "supreme" in possessing power that is continually open to judgment in terms of its reasonableness for the naturally free individuals who consent to join society and entrust it with their power. Locke's conceptual vocabulary of the legal or moral conditions in which individuals can understand their relations to one another provides the means for the theoretical coexistence of several supreme powers and the simultaneous affirmation of the ultimate "sovereignty" of the individual and the supremacy of society and its government. The legal conditions Locke stipulates—the state of nature, state of war, and civil society—are not static denominators of sovereign rulers and their subjects; they are contextual and corrigible relationships among claimants to supreme authority. Each of the potentially supreme powers within the sovereignless state can be judged to be supreme according to the context and can be considered differently as the context changes. Before turning to the language of obligation that Locke provides for making such judgments, I will explore the legal vocabulary he offers to allow the theoretical coexistence of supreme powers and the structure of the sovereignless state.

The State of Nature and Civil Society

In order to understand how a sovereignless state with coexisting claims to supreme power is theoretically possible, one must grasp the primary legal and relational character of Locke's terms, especially the "state of nature" and "civil society." The three conditions Locke identifies are relational conditions defined by the legal or moral relations between two or more individuals or groups. The transition from one condition to another is likewise a change in legal or moral relationships. The tendency to interpret Locke's discussion of the move from the state of nature into civil society in particular as a historical or temporal change (e.g., Wolin 1961, 306) has obscured the primary relational character of his terms and their bearing on the structure of the Lockean commonwealth.

The relational character of the legal conditions Locke identifies has been noted by several scholars (e.g., Ashcraft 1968, 904–5; Dunn 1969, 111) but is explored most thoroughly by Simmons (1993, esp. 16–7). As Simmons notes, an individual can be in two or even all three states simultaneously in relation to different individuals. For example, Locke speaks of a

member of a commonwealth who is being robbed as being in a state of war in relation to the robber but remaining in civil society in relation to his fellow citizens (II, 18). He states that all members of a community remain in civil society in relation to one another while being in a state of war in relation to a tyrannical executive or legislative (II, 211–2, 222). Finally, his own example as proof against those who ask about the existence of a state of nature confirms the primary relational character of both civil society and the state of nature: Independent political communities, whose own members are in civil society with one another, are in a state of nature in relation to each other (II, 14; see II, 145, 183).9

The state of nature and civil society are distinguished by the absence or presence of a recognized common judge between individuals. The primary relational character of the state of nature is brought out by Locke's stipulated definition: "Men living together according to reason, without a common superior on earth, with authority to judge between them, is properly the state of nature" (Π , 19). The state of nature is "properly" a transhistorical moral or legal concept that defines the relationship among any individuals or groups without a recognized terrestrial superior. Naturally free and independent individuals leave the state of nature and put themselves into civil society in order to avoid a condition in which the only appeal is to heaven and in which differences are therefore apt to lead to a state of war (II, 21). A civil society is created when individuals agree to "make one people, one body politic under one supreme government" by establishing a common terrestrial superior (Π , 89).

A common judge is established when individuals give up the exercise of their natural powers of judgment and execution. "Wherever therefore any number of men are so united into one society, as to quit every one his executive power of the law of nature, and to resign it to the public, there and there only is a political, or civil society" (II, 89; see II, 87). A recognized common judge is the definitive difference between civil society and the state of nature. "Whereby it is easy to discern who are, and who are not, in political society together. Those who are united into one body, and have a common established law and judicature to appeal to, with authority to decide controversies between them, and punish offenders, are in civil society one with another: but those who have no such common appeal, I mean on earth, are still in the state of nature, each being, where there is no other, judge for himself, and executioner; which is, as I have before showed it, the perfect state of nature" (II, 87). The relational character of both civil society and the state of nature is clear here. Individuals are in civil society "one with another" when they have a common law and judge to which they can appeal. Those who have no such common terrestrial appeal are "still" in the state of nature in relation to one another.

Locke appears to draw an absolute distinction between civil society and the state of nature, but the relational character of his terms means that they are not as exclusive as he initially implies. Aside from the "perfect" state of nature wholly lacking any political associations and thus any common judge, there are less "perfect" examples. "And wherever there are any number of men, however associated, that have no such decisive power to appeal to, there they are still in the state of nature" (II, 89; see also II, 91). A state of nature exists where there is no decisive common judge among "any number of men, however associated." The state of nature may conceivably be "perfect," that is, perfectly devoid of any ultimate authority; but every human association—including the commonwealth—is necessary "imperfect," since Locke rejects sovereignty.

The absence of a single common authority might even seem to make civil society indistinguishable from the state of nature. Locke in fact takes up this issue.

No man in civil society can be exempted from the laws of it. For if any man may do, what he thinks fit, and there be no appeal on earth, for redress . . . I ask, whether he be not perfectly still in the state of nature, and so can be no part or member of that civil society: unless any one will say, the state of nature and civil society are one and the same thing, which I have never yet found any one so great a patron of anarchy as to affirm (II, 94).

Does Locke make so bold? No. The civil state is not a state of nature, but because of their relational character the two conditions are not mutually exclusive (see Pangle 1988, 248; cf. Ashcraft 1968, 914). As Locke admits in granting the possibility that government abuses its authority: "This is an inconvenience, I confess, that attends all governments whatsoever" (II, 209). He evokes the "inconveniences" of the state of nature and admits that they endure in the commonwealth. The state of nature necessarily remains a constant legal possibility where there is no single sovereign authority.

The Coexistence of Supreme Powers in the Sovereignless State

The Lockean state contains several claimants to supreme power that coexist without any sovereign judge among them. Locke explains that the supreme political power of a commonwealth is initially exercised by the society as a whole, through majority rule (II, 96–9). Civil society or the people itself is therefore supreme in this sense as the original legislative power. He observes, however, that the legislative power is usually entrusted to a separate governmental body that then holds the "supreme power" (II, 132, 134). He insists several times upon the irrevocable nature of this transfer of supreme power to the legislative (e.g., II, 243). Yet, he also says that the people simultaneously retain supreme power in some regard: Although "there can

⁹ Although he has been interpreted in this way, what Locke actually writes is that "since all princes and rulers of independent governments all through the world, are in a state of nature, 'tis plain the world never was, nor ever will be, without numbers of men in that state' (II, 14). He therefore only asserts that rulers are in the state of nature, but he does not specify what sort of rulers (existing "princes" or legitimate executives?), and he does not specify with whom they are in this relationship (their fellow rulers or everyone, including the members of their own state?).

be but one supreme power, which is the legislative, to which all the rest are and must be subordinate, yet the legislative being only a fiduciary power to act for certain ends, there remains still in the people a supreme power to remove or alter the legislative, when they find the legislative act contrary to the trust reposed in them" (II, 149).

well-constituted commonwealths, legislative power is put into the hands of individuals who assemble periodically and "when they have done, being separated again, they are themselves subject to the laws, they have made" (II, 143-4). Even so, when they are exercising this power, the representatives are not then subject to the laws they make, and there is thus no common judge between the legislative and the people at that point or, indeed, ever. The coexistence of these two supreme powers has led some scholars to distinguish between the normal "legal" sovereignty of parliament and the "political" sovereignty of the people in Locke's theory or to put forward similar interpretations of divided, coordinated, or shared sovereignty. Yet, the people and the legislative in Locke's theory are "supreme," not "sovereign." Each power might be recognized by the other as its superior in certain situations, and the smooth functioning of the Lockean commonwealth requires such recognition. Where that recognition is not granted, a common superior cannot be said

The same relationship of uneasy coexistence with its fellow supreme powers exists for the executive in the Lockean state. In well-constituted commonwealths, legislative and executive power are separated. Although executive power is subordinate to the supreme legislative power, since the legislative power is not always in being but the executive power must be, the executive "in a very tolerable sense may also be called supreme." Furthermore, if the executive also shares in legislative power through the veto, for example, "he is properly enough in this sense supreme" (II, 151). Finally, the power of prerogative possessed by the executive makes him "supreme" in another, more radical sense. "This power to act according to discretion, for the public good, without the prescription of the law, and sometimes even against it, is that which is called prerogative" (II, 160). Since the executive is above the law and is in a sense outside the society and wields the executive power of the law of nature (II, 159), he must be said to hold the "supreme power" in this sense as well.

Locke's most revealing remarks concerning the tension among the supreme powers of the commonwealth come in his discussion of executive prerogative. "The old question will be asked in this matter of prerogative, but who shall be judge when this power is made a right use of? I answer: between an executive power in being, with such a prerogative, and a legislative that depends upon his will for their convening, there can be no judge on earth" (II, 168). The difficulty is not limited to executive prerogative, however, and extends to the structure of the commonwealth as a whole: "As there can be none, between the legislative, and the people, should either the executive, or the legislative, when

they have got the power in their hands, design, or go about to enslave, or destroy them. The people have no other remedy in this, as in all other cases where they have no judge on earth, but to appeal to heaven" (II, 168). Locke avows this consequence in Letter Concerning Toleration ([1689a] 1983, 49): "For there is no judge upon earth between the supreme magistrate and the people. God, I say, is the only judge in this case." He adds demurely: "I only know what actually happens where controversies arise, without a judge to determine them." The specter of the state of nature is not limited to the outbreak of a state of war; the lack of appeal which is discovered with the judgment that tyranny exists merely reveals there was never any judge on earth among these parties. The state of nature endures as a necessary possibility, and the state of war looms as a constant threat for the Lockean commonwealth.

The State of Nature and State of War

The distinction Locke draws between the state of nature and the state of war allows him to create a sovereignless state wherein the absence of a common judge among the claimants to supreme power need not necessarily be a condition of war. Against those who have confounded the state of nature and state of war, apparently Hobbes, Locke insists that the two states must be distinguished. "Men living together according to reason, without a common superior on earth, with authority to judge between them, is properly the state of nature. But force, or a declared design of force upon the person of another, where there is no common superior on earth to appeal to for relief, is the state of war" (II, 19). Like the state of nature, the state of war is a relational term that defines the legal or moral relationship between individuals where there is no common terrestrial appeal. The state of war therefore can be understood as a subset of the state of nature (see II, 90-1, 93-4; Simmons 1993; Tarcov 1981, 202; cf. Ashcraft 1968; Dunn 1969, 180-1). What distinguishes the two conditions is the use of unauthorized force: an individual "using force, where he has no right" (II, 18; see II, 181, 207, 227, 232). This is precisely the point Locke makes immediately before stating: "And here we have the plain difference between the state of nature, and the state of war" (II, 19, emphasis added; Ashcraft 1968, 904).

Most interpreters miss the relevance of the distinction Locke draws between the state of war and state of nature because they either deny the distinction or misunderstand it. Despite Locke's explicit statement, Strauss (1953, 224–30) denies that there is ultimately a distinction between the two conditions; he argues that Locke gradually reveals the irenic natural condition to be a Hobbesian war of all against all (see also Cox 1960, esp. 75–9; Pangle 1988, 244–51). Alternatively, Ashcraft (1968, 900–8) argues that Locke's description of the state of nature as "men living together according to reason, without a common superior on earth" (II, 19) is meant to prove that humans are capable of living in peace (see also McClure 1996; Wolin 1961, 306–7). Yet, living "according to reason" means living without

any other rule than the law of reason of nature, that is, without a common authority, which is Locke's explicit definition of the state of nature (see Tarcov 1981, 202-4). Individuals living without a common superior may or may not act reasonably. In the state of nature, the "inconveniences" that arise when naturally autonomous individuals exercise their "sovereign" powers with the partiality of self-love make a common authority necessary (II, 13; see II, 123). As for civil society, the distinction Locke draws between the state of nature and state of war allows the possibility of the peaceful coexistence of supreme powers within the state.

Comparison with Hobbes further illuminates Locke's attempt to construct a sovereignless state in which several supreme powers can coexist. Like Locke, Hobbes defines the natural condition of mankind as a legal condition of naturally free human beings without a common terrestrial superior. But unlike Locke, for Hobbes ([1651] 1994b, 75-7, 106) the natural condition is equally a descriptive account of human nature: an "inference made from the passions," a condition that "is necessarily consequent . . . to the natural passions of men, when there is no visible power to keep them in awe" (see Pangle 1988, 246). Hobbes's natural condition of mankind is a war of all against all in both the legal and descriptive senses. This dual role of the state of nature in Hobbes's theory poses a problem for his theory of the state, since Hobbes's sovereign remains in a state of nature in relation to his subjects. Hobbes ([1651] 1994b, 111) explains that there is no contract between the sovereign and the subjects, because the sovereign legislator and judge cannot be subject to the rule of justice and injustice he provides through law, and also because a terrestrial judge above the sovereign and subject would entail an infinite regress of sovereigns. But if the state of nature is equally a description of human nature, then how can the relationship between sovereign and subject not be one of war? Hobbes (p. 120) argues that the sovereign will be restrained from violence because it is in his interest to promote the good of the appeal. He ultimately acknowledges the "incommodities" of his absolute sovereign, however, and answers with the trump card of civil war (p. 117).

Locke calls the Hobbesian bluff: "Absolute monarchs are but men, and if government is to be the remedy of those evils, which necessarily follow from men's being judges in their own cases, and the state of nature is therefore not to be endured, I desire to know what kind of government that is, and how much better it is than the state of nature, where one man commanding a multitude has the liberty to be judge in his own case" (II, 13; see II, 93)? In Locke's view, a Hobbesian sovereign is in not merely a state of nature, but is in a state of war in relation to his subjects (II, 222; see II, 91, 176, 181, 207, 227). Naturally free and rational individuals "cannot be supposed" to agree to such "an absolute arbitrary power" (II, 127). Locke's distinction between the state of nature and state of war permits, at least in theory, the peaceful coexistence between government and society, and Locke thereby aims at Hobbes's goal of peace with arguably more theoretical consistency than Hobbes himself.

Finally, we may wonder whether Locke's sovereignless state is not a more logical conclusion from the doctrine of individual natural rights than is Hobbes's attempt to erect the sovereign upon a natural rights foundation. Filmer was among the first to point to this difficulty. Hobbes wants to retain and even strengthen the sovereignty of Bodin, but beginning with the natural authority of the individual does not produce a truly absolute sovereign, given Hobbes's ([1651] 1994b, 144) assertion of inalienable rights and his admission of the right to revolt when the subjects judge that the sovereign power is effectively dissolved. Hobbes's sovereign is in this respect not the single ultimate judge that sovereignty theory seems to demand, including his own, untraditional, version. Locke does not attempt to erect a single, absolute political authority and instead articulates a sovereignless state that permits several simultaneous claimants to supreme power and continued recognition of the natural rights of the individual.

The Tensions in the Sovereignless State

Locke's relational vocabulary permits the theoretical coexistence of several claimants to supreme authority, but jettisoning sovereign authority leaves a practical tension. Vaughn (1925, vol. 1, 133, 171, 193-4) complains that a sovereign who "is not absolute, a sovereign which can only claim second honors, is, in the strictest sense, no sovereign at all." He worries that "the sovereignty of the individual" makes the Lockean commonwealth an unstable aggregation of individuals instead of a true corporate body. Vaughn seeks the sovereign authority Locke rejects, but he does leave one asking what binds together and stabilizes the Lockean commonwealth.

Grant (1988, 75–6) suggests that Locke replaces the sovereignty of a ruler with the sovereignty of law. Locke (II, 3) defines political power as the right to make and execute laws for the public good of a civil society, and he insists on the rule of standing law (II, 87, 94, 131, 136, 142). Civil society is therefore characterized by the legitimate exercise of power through law, whereas the illegitimate exercise of power is tyranny, which occurs when a governor makes "not the law, but his will, the rule" (II, 199). When the members of the commonwealth judge that the law is made and executed for the public good and when governors as well as governed consider themselves bound by the law, then there can be said to be a common judge—the law. Nonetheless, there must be a maker and executor of the law. Although Locke counsels representative government in which the legislators become subject to the laws they have made once the assembly is dissolved (II, 138), in the act of legislating they are strictly speaking not bound by the law, nor is the executive, at least in his use of prerogative. More important, the sovereignty of law depends upon the judgment of its legitimacy by governors and governed alike. The legitimacy and stability of the Lockean state requires a shared conception of the basis and limits of political power.

THE LANGUAGE OF OBLIGATION

A shared language of obligation is a centripetal force that helps the political machine work in practice despite the tension of numerous claimants to supreme authority. Locke offers an alternative political language as a replacement for sovereignty theory, especially in its divine right version, and for the political discourse of his time more generally. In the Preface to Two Treatises he explains "there cannot be done a greater mischief to prince and people, than the propagating [of] wrong notions concerning government." Although Filmer's doctrine is "glib nonsense put together in well sounding English," it is nonetheless dangerous nonsense since "the pulpit, of late years, publicly owning his doctrine" has "made it the current divinity of the times" (Locke [1689b] 1988, 137-8). Locke means to reform the political vocabulary of his time, correcting mistaken notions and propagating a more reasonable alternative.

Linguistic Reform and Political Reform

Replacing mistaken notions concerning government entails a conceptual or linguistic reform. Locke's project in the Essay ([1690] 1975) is to labor on existing philosophical ideas and language to make them more adequate, but that reform extends to political and moral issues as well. In the Essay, Locke argues that a common language is "the great bond that holds society together." In the Second Treatise, he defines a political community in terms of a common political authority (II, 212). A political society is therefore defined ideally, at least—by a common authority and by common notions concerning political authority. Languages are "proportioned to the notions men have," and Locke observes that those notions and the language used to express them vary from nation to nation (Essay II.28.2, III.5.8); furthermore, languages change with changing notions (Essay II.2.7). The replacement of mistaken notions of government with correct notions that Locke hopes to achieve through his Two Treatises is an extension of his project in the Essay.

The connection between linguistic and political reform is suggested by parallels between the Second Treatise and the Essay. Chapter 7 of the Second Treatise begins with a discussion of natural sociability and the limitations of the associations that arise "naturally."

God having made man such a creature, that, in his own judgment, it was not good for him to be alone, put him under strong obligation of necessity, convenience, and inclination to drive him into society, as well as fitted him with understanding and language to continue and enjoy it. The first society was between man and wife, which gave beginning to that between parents and children; to which, in time, that between master and servant came to be added: And though all these might, and commonly did meet together, and make up but one family, ... each of these, or all together came short of political society, as we shall see, if we consider the different ends, ties, and bounds of each of these (II, 77).

Locke indicates a link between natural sociability and the capacity for reason and language, and his discussion in the *Essay* of this capacity and its shortcomings is strikingly similar:

God having designed man for a sociable creature, made him not only with an inclination, and under a necessity to have fellowship with those of his own kind; but furnished him also with language, which was to be the great instrument, and common tie of society. Man therefore had by nature his organs so fashioned, as to be fit to frame articulate sounds, which we call words. But this was not enough to produce language (III.1.1).

Both political society and language have a natural basis, but that basis is limited and fallible; both society and language must be supplemented by human conventions (compare Tully 1980, 48–9). The chapters in the *Treatise* and the *Essay* that immediately follow the two chapters cited above establish this point in a parallel manner. Chapter 8 of the *Treatise* begins:

Men being, as has been said, by nature, all free, equal and independent, no one can be put out of this estate, and subjected to the political power of another, without his own consent. The only way whereby any one divests himself of his natural liberty, and puts on the bonds of civil society is by agreeing with other men to join and unite into a community (II, 95).

Locke argues in the *Essay* that a linguistic community is likewise established through voluntary agreement:

Man, though he have a great variety of thoughts... yet they are all within his own breast, invisible, and hidden from others, nor can of themselves be made to appear. The comfort, and advantage of society, not being to be had without communication of thoughts, it was necessary that man should find out some external sensible signs, whereby those invisible ideas, which his thoughts are made up of, might be made known to others.... Thus we may conceive how words, which were by nature so well adapted to that purpose, come to be made use of by men, as the signs of their ideas; not by any natural connection... but by a voluntary imposition (III.2.1).

Human contrivance must reform the fallible social and linguistic endowments of nature.

Locke's philosophical reform in the Essay provides a framework in which to understand his reformation of political language in the Two Treatises. The natural capacity for speech requires convention, the "voluntary imposition" of words that serve as signs of our ideas, for it to be of any utility. Because there are no innate ideas and no natural correspondence between the objects of our experience and our ideas, or between those ideas and the words we use to express them, human understanding and language are liable to imperfection. Humans often have inadequate or mistaken ideas, misuse the words that stand for them, or mistakenly assume that others agree about the significance of the words. Particularly subject to imperfection and abuse are the "complex ideas" and "general words" that stand for them from which "arise the obligations

¹⁰ A detailed comparison of Locke to Aristotle concerning the relationship between language and sociability and the roles of nature and artifice in the rise of political society would be highly instructive but goes beyond the scope of this article. Compare generally Locke, I, 98, II.2, 77–89, 169–74, to Aristotle, *Politics*, I 1–2.1252a1–1253a39 (1985, 35–8).

and several duties amongst men" (Essay II.28.2). Unlike "simple ideas," which refer to an external object (the "archetype") and are therefore less liable to abuse or easier to amend, "complex ideas" have no immediate referent and are themselves "archetypes" (Essay III.5.3). Since these ideas exist only in the mind, it is easy for an individual to assume that others attach the same meaning to the "general words" that stand for them (Essay II.28.19; see II.32.5, 9–12; III.2.4; III.9.6).

Moral and political language is thus particularly liable to be confused, inadequate, and even fantastical. Locke nonetheless finds the remedy for the disease of moral and political language in its very cause. The complex ideas of relations are "the workmanship of the mind" (Essay III.5.3). Just as human labor must be added to the spontaneous productions of nature for them to be worth anything (esp. II, 36; see Locke [1689a] 1983, 47), so human contrivance must "perfect" language to make it truly useful to man (Essay III.1.3; III.3.4).¹¹ The "general terms" used in moral and political discourse, being of human construction, can be known perfectly. Locke therefore envisions a moral science capable of demonstration (Essay III.9.16; see III.11.15-7; IV.3.18-20). As a part of moral science, the part of political science treating "the original of societies, and the rise and extent of political power" (Locke [1703] 1997b, 351-2) is capable of the improvement Locke undertakes in the Two Treatises.

The philosophical aspect of the Two Treatises can be seen in Locke's concern with correct definitions of axiomatic terms (Grant 1988, chap. 1; see also Danford 1978, 48-69; Tully 1980, 10-1). The Second Treatise begins by stipulating a definition of "political power" (II, 2-3) and proceeds with definitions of the state of nature, state of war, and civil society. As I argued above, all these definitions are offered by Locke as alternatives to the terms of sovereignty theory. He therefore defines "political power" in terms of the powers traditionally described as "sovereign" but without using that term, and he does so because of his basic doctrine of the natural freedom of the individual as revealed by the "state of nature," where there is no "right to dominion and sovereignty" by nature $(\Pi, 4)$. He then uses the basic terms of this theory to show the fallibility of the patriarchal sovereignty that tends to arise from natural human sociability and offers an alternative conception of the basis and limits of political authority founded upon the consent of naturally free and equal individuals. Even when Locke uses old terms, such as "political power" or the "state of nature," he gives them specific meanings. The Two Treatises is effectively an attempt to replace one political language with another.

Like the fallible linguistic endowment of nature, the associations that develop from natural human sociability require reform. Locke is particularly concerned that these associations have led to mistaken notions about the basis and limits of political obligation. He explores this fallible development in the chapters from the Second Treatise quoted at length above. The family grows to the point that resembles "a little commonwealth" (II, 86). The habit of living under patriarchal authority leads us "naturally . . . into that form of government" (II, 107), and the innocent people of "that poor but virtuous age" therefore erected unrestrained monarchies (II, 110). They later became aware of the dangers of paternal prerogative and "found it necessary to examine more carefully the original and rights of government" (Π , 111).

The "natural" forms of association stemming from the family suggest the mistaken notion that serves as the cornerstone of the patriarchal argument (and perhaps sovereignty theory more generally): men... are born under governments" (II, 114). Locke therefore offers an alternative account of the true origin of government through consent. Although he concedes the likely patriarchal roots of civil society (see II, 54, 105, 117, 175; Schochet 1969; Waldron 1989, 7), he argues that whatever resemblance the family may have to "a little commonwealth," it is very far from it, "both in its constitution, power and end" (II, 86). The subpolitical relationships of the family must be reformed along more reasonable grounds (see II, 78-82, 85, 116-22; see Pangle 1988, chap. 18; Resnick 1992). Since individuals are all equally born "with a title to perfect freedom," human associations in general must be conceived of—or rather reconceived of—as beginning in consent (Π , 87).

The Reasonableness of Consent

The doctrine of the origin of political power in consent and the language used to express it are meant to be reasonable: philosophically adequate as well as practically salutary. In order to answer critics of consent theory, including Filmer, who argued that founding political power on consent makes government unstable and ineffective. Locke wants to reform previous consent theory in order to make it reasonable. This effort can be likened to his project in the Reasonableness of Christianity ([1695] 1965), wherein he claims to show that Christianity is reasonable in its doctrines and demands based on what a "righteous God" could be "supposed" to mean and require, given a reasonable divinity who made man a rational creature and requires him to live by "the law of reason, or, as it is called, of nature" (§§1-4, 14; see Rabieh 1991). He faces a similar task in the First Treatise, where he seeks a more "reasonable" interpretation of the scriptural texts upon which Filmer and the divines have relied. Locke therefore reinterprets the divine donation to Adam in terms of rights and right reason. The divine grant "gave him not private dominion over the inferior creatures, but right in common with all mankind" (I, 24). And "it is more reasonable to think, that God who bid mankind

¹¹ The parallel between Locke's discussion of the conventions of language and of property, including money, could be developed. The value of money is introduced by a "tacit agreement" (II, 36), and the meaning of words is likewise based upon a "tacit consent" (Essay I.3.22; III.2.8). Similarly, both can be perfected by making the "tacit" conventions clear and express through institutions, through philosophy in the case of language and through monetary regulation in the case of money. Locke's writings on comage and monetary policy should be recalled in this context.

increase and multiply, should rather himself give them all a right" to make use of the animals for food and clothing even before the explicit grant to Noah (I, 41, emphasis added; see I, 39). Man is given a "strong desire of preserving his life and being" and "reason, which is the voice of God in him," and those endowments reveal to him his rights and the rights of "each individual person" (I, 86, 88). Locke thus clears the ground for his own theory and establishes that theory on a moral claim rooted in human nature (see II, 1).

Overarching Locke's philosophy as a whole is the idea of human rationality (Schouls 1992, 42-3). The criterion of reasonableness likewise spans his political teaching. As he states in explaining why naturally free individuals will give up natural authority to be held in trust by a society limited in its use of political power by the common good: "No rational creature can be supposed to change his condition with an intention to the worse" (II, 131). The criterion of reasonableness justifies both the beginnings and the ends or limits of the commonwealth. First, he shows why it is reasonable for individuals to consent to give up their natural liberty and subject themselves to political authority through his account of the "inconveniences" of the state of nature (see Grant 1988, 53-7). Second, he derives the limits to political power, and thus the limits of our obligation, from the same basis as the reasons for consent. For example, in the Letter Concerning Toleration, Locke ([1689a] 1983, 26) argues that the jurisdiction of civil magistrates is "bounded and confined" by the aims of civil government and cannot extend to matters of religious salvation, "nor can such power be vested in the magistrate by the consent of the people."

The act of consent itself is subject to the correction of reflection or reason. Reasonableness serves as the criterion for the end of government through justified rebellion. Here Locke offers people a language to articulate the feelings that contradict the mistaken notions they have been proffered. For example, he observes: "But whatever flatterers may talk to amuse people's understandings, it hinders not men, from feeling." He then articulates this feeling in the terms of his theory: "And when they perceive that any man, in what station soever, is out of the bounds of the civil society which they are of; and that they have no appeal on earth against any harm they may receive from him, they are apt to think themselves in the state of nature, in respect of him, whom they find to be so" (II, 94). Similarly, people "cannot but feel" the "long train of abuses" that justify revolution (II, 225; see II, 208-9), yet they "are more disposed to suffer, than right themselves by resistance," even when trespasses of authority are "sensible to the greater part" (II, 230). The people need to be exhorted in a language that translates their just feelings into a language of reasonable obligation. For the philosophical reform of political language to be effective in practice it must change common opinion and usage, or what we might even term "ordinary language." Locke is aware of both the "civil" and the "philosophical" use of words (Essay III.9.3). Like the Two Treatises itself, a combination of philosophy and political rhetoric, the language of obligation Locke offers within the work is both philosophical and common.

Locke's rearticulation of political power in terms of a language of reasonable consent and obligation can be seen in the Preface to the Two Treatises. There he declares his hope that his discourse will "establish the throne of our great restorer, our present King William; to make good his title, in the consent of the people, which being the only one of all lawful governments, he has more fully and clearly than any prince in Christendom; And to justify to the world, the people of England, whose love of their just and natural rights, with their resolution to preserve them, saved the nation when it was on the very brink of slavery and ruin" ([1689b] 1988, 137). William may have come to his throne as a "restorer" through inheritance (that of his wife), but he owes any title to legitimate rule to the consent of the people. By stipulating a language of obligation, Locke counsels the ruler by alerting him to the true basis of his authority. He also instructs the ruled by declaring to them their "just and natural rights." Locke rejects the constitutional justifications being given for the Glorious Revolution and offers his theory of consent instead (see letter to Edward Charles, cited in Ashcraft 1986, 597-600; see also Den Hartogh 1990, 112-3). He does not legitimize existing rule so much as provide a language through which authority may be justified.

Interpreting Locke's theory in terms of a language of obligation that appeals to the criterion of reasonableness may reveal a middle way through the complex scholarly debate over consent in his theory. On the one hand, reasonableness is central to those who interpret Locke as a "hypothetical" consent theorist insofar as they argue that he offers a theory of what people might be supposed to have consented to (see Pitkin 1965, 990-9; Pateman 1985, esp. 66; Steinberg 1978). On the other hand, the embodiment of this doctrine in a language of obligation spoken by a political community eventually allows for more express forms of consent, forms that Locke himself seems to have desired (see Ashcraft 1992; Dunn 1967). Whether Locke offers a theory of hypothetical consent or actual consent, the central issue is his argument that to conceptualize and articulate political relations in terms of consent is to be able to offer an account of their reasonableness (see Simmons 1992).

The Limits of the Language of Obligation

Although Locke offers a language of reasonable obligation, he is nonetheless keenly aware of the limits of speech and reason in politics. Naturally free individuals are born subject to the law of nature (II, 6), which is

¹² For "ordinary language" approaches to political theory, including Locke, see in particular the work of Pitkin (1967, 1972), Flathman (1972), and Danford (1978).

"the law of reason" (II, 57). The law of reason governs the state of nature as well as civil society, and it stands as a rule for governors and governed alike in their exercise of authority. Yet, political authority is necessary in large measure because of the fallibility of human reason. "For though the law of nature be plain and intelligible to all rational creatures; yet men being biased by their interest, as well as ignorant for want of study of it, are not apt to allow of it as a law binding to them" (II, 124). Educating individuals in the law of reason and a language of obligation based upon it, as Locke does, may help promote the responsible exercise of natural and political power; but human reason cannot succeed alone. Those who renounce reason and adopt the way of the beasts may be justly punished (II, 11, 171, 181). The use of force without authority may justly be opposed by force (II, 155). In the last analysis, however, naturally free individuals must judge when force is authorized. The criterion of reasonableness stands as a rule, but although sometimes corrigible, judgment is fallible. The need for force in politics and against it is itself a reflection of the limits of human reason and of Locke's language of obligation.

THE CONSTITUTION OF THE LIBERAL STATE

Locke's sovereignless state necessarily produces coexistent claimants to supreme authority, but a shared political language provides its speakers with the means to express and assess their mutual obligations as well as their competing claims. The tensions in the liberal tradition revealed by the simultaneous discourses of individual natural rights, popular sovereignty, and governmental authority are likewise negotiated in political speech and action. If government in a liberal state founded upon the doctrine of individual natural rights is to be of the people, by the people, and for the people, then the institutional mechanisms that are so often the focus of analysis are insufficient for maintaining the perceived legitimacy of the state. Understanding the relationship between the structure of Locke's commonwealth and the language of obligation he offers to help bind it together should help us better appreciate the constitution of the liberal state.

The importance of shared political concepts for the proper working of the liberal commonwealth is evident in the American regime. 13 The major documents of the American founding period are declarations of principle that articulate a lexicon of political language to which its speakers can refer (see White 1984). The Declaration of Independence not only proclaims the "selfevident truths" of the rights of naturally equal individuals but also, through the very act of proclaiming them, establishes a "people" apart. The Constitution is both an institutional framework and a declaration of the "people" forming a more perfect union in order to realize the promise of a government designed to protect our rights. An opponent of the proposed Constitution, the "Federal Farmer" (1787, in Storing 1981, 2, 324-5) articulated the importance of declaring the basis of obligations and their limitations in his call for a bill of rights:

We do not by declarations change the nature of things, or create new truths, but we give existence, or at least establish in the minds of the people truths and principles which they might never otherwise have thought of, or soon forgot.... What is the usefulness of a truth in theory, unless it exists constantly in the minds of the people, and has their assent:-we discern certain rights . . . which the people of England and of America of course believe to be sacred, and essential to their political happiness . . . while the people of some other countries hear these rights mentioned with utmost indifference. . . . The reason of the difference is obvious—it is the effect of education, a series of notions impressed upon the minds of the people by examples, precepts and declarations.

A proponent of the Constitution, Madison, came to see a similar use of constitutional declarations of principle through a bill of rights: "The political truths declared in that solemn manner acquire by degrees the character of the fundamental maxims of free Government, and as they become incorporated with the National sentiment, counteract the impulses of interest and passion" (letter to Jefferson of October 17, 1788, in Madison 1981, 158). The tensions in modern liberal states over competing claims of individual and governmental authority endure, but the very viability of the commonwealth may rest upon its members being able to speak a common language.

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¹³ I do not want to argue that Locke's works exercised an influence on the American Revolution, only that, like Locke, the revolutionaries and founders appreciated the importance of declaring principles as a guide to behavior. For recent considerations of Locke's possible influence in America and the debate over that question, see Dworetz 1990, Huyler 1995. For a discussion of a similar appreciation of declaration of principle in the covenant tradition, see Lutz 1988

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The Possibility of Self-Government

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any have suggested that the findings of social choice theory demonstrate that there can be no "will of the people." This has subversive implications for our intuitive concept of self-government. I explore the relation between the notion of a "social will," that of self-government, and the impossibility theorems of social choice theory. I conclude that although the concept of the social will is essential to that of self-government, the findings of social choice theory do not cast doubt upon the possibility of either. Unlike many attempts to respond to the threat posed by social choice theory, my argument does not require any appeal to the problematic notion of the common good.

o speak meaningfully of an organization or political community as self-governing requires that it have (or implies that it has) the property of having, knowing, and consciously pursuing its own "will." As an abstract proposition, this claim is not especially controversial, but exactly how the standard it sets might actually be met remains a matter of deep dispute. I will address a particularly strong form of skepticism about the possibility of satisfying this requirement. This is the claim made by certain social choice theorists that it is not merely difficult, but logically incoherent, to believe there are social procedures under which collectivities can learn their own will. I hope to show that, whatever other problems theories of the "social will" face, this particularly strong objection can be answered, and in a way that opens up fruitful avenues for further inquiry. I begin by briefly setting out the objection that I aim to defuse.

THE SOCIAL CHOICE OBJECTION

Starting with Kenneth Arrow's ([1951] 1963) seminal monograph, social choice theorists have sought to identify procedures under which a collectivity might identify the preferences that could reasonably count as its own. Taking for granted relatively uncontroversial liberal democratic assumptions, the social choice theorists asked a simple question: How can one devise procedures to "process" or "aggregate" the impartially weighted preferences of individual members of a collectivity into a preference that can stand for the collectivity as a whole? The answer Arrow and his successors repeatedly gave was that the only procedures capable of identifying a coherent and unambiguous collective preference or "will" involve either deferring to the judgment of a dictator or narrowing in advance the range of preferences that citizens are permitted to express. Neither of these ways of generating a coherent collective preference seems to satisfy our intuitive sense of what it means for something to have its own will. No one would be satisfied by the suggestion that a society whose decisions are determined for it by a dictator is genuinely following its own will. Similarly, to impose a blanket prohibition on society choosing a certain preference ordering, even though all its members would otherwise do so, may also seem inconsistent with the notion of society following its own will. Arrow's terminology is telling here: To violate this second proviso is to violate a norm of "citizen sovereignty."

In the years since Arrow's initial contribution, social choice theorists have both confirmed and deepened the general point (McKelvey 1979; Plott 1976; Riker 1982; Schofield 1985; Sen 1970). As a result, they have been increasingly confident in their claims about the implications of social choice theory. It is now standard for these theorists to claim that the impossibility theorems, because they establish there can be "no such thing as the will of the people" (Maclean 1991, 509), mandate a certain kind of methodological individualism. Willing, preferring, and choosing are not possible predicates of societies, and to think otherwise is to engage in a primitive kind of anthropomorphism (Riker 1982, 18; Schelling 1984, 93). The comments of Charles Plott (1976, 525) are representative:

The concept of social preference involves an illegitimate transfer of the properties of an individual to the properties of a collection of individuals.... The Arrow theorem demonstrates that the concept of social preference involves the classic fallacy of composition, and it is shocking only because the thoughts of social philosophers from which we have developed our intuitions about such matters are subject to the same fallacy.

I seek to refute this kind of claim and to show how having a will might be a possible property of both societies and individuals. Before I tackle these claims, I want to set the technical issues raised by social choice theory within the broader context of discourse about self-government. This is essential if we are to understand the nature of the difficulty identified by social choice theory and its relation to the larger theoretical issues that it often claims to resolve.

SELF-GOVERNMENT: SOME PRELIMINARIES

Very roughly, to say that a political community or organization is self-governing is to say that in some sense or senses the actions taken or controls imposed

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by its governing institutions can be thought of as originating from within that community or organization. There is no essential or canonical specification of the senses in which self-government demands that authoritative action be internally "sourced" in this way. To set the stage for my argument, however, I will identify three salient ways in which one might claim that authoritative action originates from within a self-governing political community or organization. I will then use these three senses of self-government to make several preliminary points about self-government, democracy, and social choice theory.

A first and relatively undemanding sense in which a group can be self-governing operates at a basic institutional level. A society can be self-governing simply to the extent that the apparatus of government and administration is operated exclusively by officials and participants drawn from within, and authorized by, the citizen body, not by outsiders. In this institutional sense, imperial domination is the antithesis of self-government.

A second and more demanding interpretation of the idea that public action originates from within makes use of our organizing idea of a social will. In this view of self-government, public decisions must be plausibly understood by members of the collectivity as reflecting, expressing, or revealing a will that is authentically their own, and there must at least be social consensus on procedures for determining or verifying the content of this will, such that one can in principle assess the extent to which public action fulfills or deviates from it. Self-government in this sense requires that the political community itself be the subject of political action at least in the following minimal way: Both governors and governed must recognize accepted "will-revealing" procedures and view them as communicating instructions, which governing agencies are then expected to execute as a matter of course. If this condition were satisfied, we could meaningfully speak of a society or organization being the author of its own fate, at least insofar as the actions it takes successfully fulfill a will that has been appropriately certified as its own. It is the possibility of ever satisfying this condition that social choice theory calls into question.

A third and still more stringent sense in which public action might have an internal source results in a highly moralized (and I believe marginal) conception of selfgovernment. In this account, society is self-governing to the extent that its government acts only in the light of a plausible conception of citizens' common good. The idea is that officials take the whole community's welfare (the "public interest") to be the main object of public action, and they appeal to a plausible account of this common good as the grounds for public action. In this sense, a community is self-governing insofar as public action is oriented only to those goals and ends identifiable as proper, rather than alien, to the whole community. This rules out justifications for public action that either appeal to goals and values that go beyond the welfare of the relevant community (the progress of mankind in general, fulfilling God's providential design, and so on) or privilege the interests of narrower sectional or partisan groups within the community. For instance, juries might seem to be self-governing in a variety of other senses (e.g., juries do act in accordance with their will), but they are not self-governing in this third sense, for a jury does not derive justification for a verdict on a particular occasion from a set of claims about its own well-being but, rather, looks to something beyond itself—the fate of a third party.

Similarly, a society whose ruling class justifies the exploitation of the rest of the population by reference to its own partisan interests, rather than to those of the whole society impartially considered, is not self-governing in this third sense. In such a case, justifications for public action are not grounded in claims about the best interests of society as a whole but in claims about the interests of a partisan subset of the community. Here, it seems more accurate to say that one social group is simply subjugated to another in that the former's interests are not given due weight in the (purported) justifications for public action offered by the ruling class. One can reasonably claim that this sort of subjugation is inconsistent with at least ideal forms of self-government. Certainly, the history of modern concepts of self-government, from Rousseau on, is replete with examples of theorists and commentators who have understood the concept in this way.1

To set the stage for my argument, I now make several connected observations about these three senses of self-government.

Self-Government and Democracy

The concept of self-government has an elective affinity with the idea of democratic rule. But if democracy requires simply that all citizens in a given state (have a right to) participate in the political process, then strictly speaking it is not true (1) that any of these three senses of self-government requires democracy or (2) that democracy entails self-government in any of the three senses. Hobbes's political theory illustrates the first point: Citizens can will their collective security and self-preservation by authorizing highly undemocratic forms of government; even a monarchical Leviathan instantiates Hobbesian self-government.

The second point is illustrated by those empirical theories of democracy inspired by the work of Joseph Schumpeter (1942) and his antecedents (Michels 1916). These modern theorists of democracy regarded the notion of self-government as obsolete and prescientific, in part because in their view it tends to obscure the systematically oligarchical reality of political organizations. Despite this, they believed that a theoretically useful theory of democracy could be retrieved from the conceptual swamp left behind by the philosophers of self-government.² The findings of social

¹ This sort of claim is normally brought in under the aegis of claims about the virtues of citizenship. Such virtues require that citizens be motivated by a concern for the public good rather than by their self-interest (their "particular wills," in Rousseau's terminology). Self-government, in this view, requires a virtuous citizenry. For a recent statement of the idea, see Barber 1993, 66.

² For a superb illustration of this tendency to dissociate democracy

choice theory tend to reinforce the intuitions of those in the Schumpeterian tradition. In this view, of which William Riker (1982) was the most vociferous proponent, the social choice objection demonstrates that the only viable conceptions of democracy available to us are ones that drop the idea that democratic votes express a popular will. We are left with a much less ambitious—in Riker's terms, "liberal"—conception of democracy. It demands merely that all citizens (have the right to) participate in a set of political processes that provide some kind of external check on the activities of oligarchical political elites. The ability of democratic political systems to institutionalize this sort of check does not, however, imply that when the check is invoked, "society" as a whole is concertedly acting in accordance with something called "its own will" (Riker 1982, 243-5). Rather, the democratic system can be thought of as a rather crude, although at least inclusive, arbitration device. According to Riker and his followers, social choice theory provides a cast-iron justification for severing the connection between democracy and the problematic idea of self-government. This conclusion is apt to seem shocking: If authentic democracy requires anything, it is surely that public action track the popular will. But it is just this conception of democratic self-government that social choice theory calls into question. In order to retrieve that conception, we must defuse the social choice objection and show how there can in principle be such a thing as the popular will.

Self-Government and the Common Good

Self-government in the third and most demanding sense shades into, and becomes difficult to distinguish from, some more general account of political justice and the social good. For this reason, we should regard this third sense as lying at the very periphery, rather than near the core, of the concept of self-government. This is supported by the intuitively plausible reflection that a self-governing society need not be an especially good society.3 The first two senses of self-government preserve that intuition, but the third sense overturns it. A society could be governed only by its own members (first sense), and it could know and pursue its own will (second sense), but from neither supposition does it automatically follow that this society acts in accordance with its common good. The social will might be misguided or even evil, and those citizens who wield power might be entirely corrupt. The third sense, however, precludes these possibilities, because when policy runs counter to the correct appreciation of the common

and self-government in modern empirical research, see Duverger 1954, 424: "No people has ever been known to govern itself and none ever will. All government is oligarchic: it necessarily implies the domination of the many by the few.... All government implies discipline... by definition constraint is external to the constrained. A people does not constrain itself, it is constrained. It does not govern itself, it is governed."

good and its imperatives, the relevant society is not fully self-governing. But this seems far too strong. It might make sense to say that such a society falls short of the moral ideal of a perfectly self-governing community, but it is implausible to say that it is violating conditions that are either strictly necessary or sufficient for us to be able to describe it as genuinely self-governing.

The Central Sense of Self-Government

If the third sense seems too strong, the first seems too weak by itself to count as a sufficient condition for self-government. Yet, the first sense does appear to embody a necessary condition for regarding a community or organization as authentically self-governing. Although the absence of (say) imperial domination does not automatically guarantee that a society is self-governing (there have been plenty of nonimperial autocracies), it is clear enough that any form of imperial domination is strictly incompatible with self-government. By contrast, the second sense looks very much like a sufficient condition for self-government: If we can show that a society is acting upon a will that it knows to be its own, it is unlikely that we need to establish anything more in order to convince ourselves that this society is self-governing. There might be a further question about whether this society is good, but we have already seen that a self-governing society need not be good: The mere fact that a certain society is pursuing policies inconsistent with the common good or social justice, or that are in some other way ethically problematic, does not automatically establish that it is not self-governing. As long as public policy fulfills the social will (for good or ill), that seems sufficient to show that this society is self-governing.

The Independence of the Second and Third Senses

These observations suggest not only that the second sense of self-government is the central one but also that it is largely independent of the third. This is a pivotal point in my argument. One of the main claims I advance is that many of the difficulties involved in the notion of self-government can be removed by maintaining a clear distinction between the second and third senses. That is, we should separate the question of how society can determine its own will from that of how we might discern the public interest or society's common good. Both classical and contemporary discussions of self-government have systematically confused these two issues. Two characteristic forms of this confusion have particularly plagued these discussions.

Confusion 1. Sometimes it is assumed that there is ultimately no way to specify the common good apart from somehow amalgamating citizens' judgments on some occasion about what the common good requires. The effect of this move is to collapse the third sense of self-government into the second: In this view, the notion of the common good involved in the third sense

³ Note, however, that this is perfectly compatible with the claim that any authentically good society would have to be to a high degree self-governing.

is identified with whatever society contingently nominates as its common good through the kind of "willrevealing procedures" envisaged in the second sense. Social choice theorists are particularly prone to making this move, mainly because they subscribe to an attenuated version of utilitarianism that views the public interest or common good as an aggregative function of the contingently expressed preferences of a society's constituent individuals. Therefore, they are tempted to claim that answers to questions about society's common good or the public interest stand or fall on the results of voting procedures that aggregate citizens' various views on the subject. Consider in this regard Riker's (1982, 137) claim that social choice theory "even leads us to suspect that no such thing as the 'public interest' exists, aside from the subjective (and hence dubious) claims of self-proclaimed saviors" (see also Plott 1976, 526-7). Riker seems to think this follows from the fact that the impossibility theorems show that outcomes of votes will systematically be ambiguous or meaningless.

But this inference is surely invalid. We would not normally conclude from the fact that individuals make contradictory or ambiguous claims about their own interests that there is no longer any basis on which to make credible assessments of their best interests. Still less does it show there is "no such thing" as an individual's best interests. Perhaps the individual in question is simply confused or ill informed. The same point applies with equal force in the case of claims about the best interests of a collectivity (i.e., the common good). The fact that citizens' amalgamated judgments about their common good are (even systematically) ambiguous or incoherent is not a reason to suppose that no such thing as the common good or public interest exists or can be determined. The salient question in this context is: What is the common good, and what does it require under circumstances A, B, C? But a claim about the ability or inability of a particular agent or agency to enunciate a clear, unambiguous answer to this question does not amount to any sort of answer to that question itself. We would at the very least need some additional argument to show that this particular agency's opinion on the subject carries special authority, so that its failure (say) to arrive at a clear, coherent answer definitively establishes that the question itself just is finally unanswerable. After all, the fact that some agent manages to enunciate a coherent, unambiguous view of its own best interest does not in any way imply that that view is correct or even plausi-

To my mind, these reflections highlight the fundamental gap between two issues raised, respectively, by the second and third senses of self-government. The second sense raises the question of how we can establish that a particular judgment about the common good can genuinely be regarded as society's own will. The third sense raises the different issue of how we can determine a correct or plausible account of society's common good. Perhaps social choice theory puts in doubt the possibility of ever resolving the first issue, although that is the claim I seek to refute. But even if

my argument fails, it still does not follow that social choice theory puts in doubt the possibility of resolving the second issue; determining the common good is a different problem, and there is little reason to believe the apparatus of social choice theory is equipped to address it. Note, finally, that one can admit all this without in the least casting doubt on the proviso that, properly understood, the common good is a function of the welfare of the individuals who make up the political community. This unobjectionable proviso must be distinguished from the very different and more dubious claim that the common good is whatever citizens contingently judge it to be on some occasion.

Confusion 2. A second way in which the second and third senses of self-government are often conflated moves in just the opposite direction, by effectively collapsing the second sense into the third. This occurs when one succumbs to the temptation to stipulate that some putatively valid account of the common good (and its correct application to contingently arising social and political problems) embodies the "real will" of the people. On this sort of view, in order to disclose the people's will on some occasion, we need only grasp the correct account of their common good and determine what it recommends under the relevant circumstances. This proposal derives such plausibility as it has from the Socratic intuition that agents, to the extent they are rational, will their own good. When individuals or groups will what is counter to their best interests, they must have failed to understand or grasp what they "really" will. This assumption undoubtedly underlies a good deal of the mistrust of popular rule voiced over the centuries. If we suppose that the average citizen is not competent to determine the common good, then the "real will" of the people may be better served by taking power out of their hands and giving it to "the experts."

Some political theorists have tried to defend popular rule even within the terms of the Socratic assumption. Rousseau (according to some influential interpretations) is responsible for the most historically significant version of this argument, and for that reason I refer to it as the General Will argument. It confuses the social will and the common good in exactly the way I am trying to identify. In order to diagnose this second confusion and highlight its shortcomings, I will briefly discuss some salient features of the General Will argument.

THE GENERAL WILL ARGUMENT

The common good requires that we give fair consideration to the best interests of all citizens, and the General Will argument suggests that the citizenry in full assembly is the appropriate "expert" on the common good. To the extent that we entrust power to groups smaller than the whole citizenry, we increase the likelihood that government will unfairly privilege the interests of some partisan sector of the community. This argument need not imply that the whole citizenry is infallible, merely that a virtuous citizenry is more

likely to identify correctly the general will than are any of the less inclusive alternative candidate groups. The latter are more likely to be unduly influenced by their narrow sectarian interests and so are likely to offer correspondingly less reliable guidance as to the content of the general will or common good.

Today, political theorists appeal to versions of this argument in order to respond to the difficulties raised by social choice theory. But if social choice theory calls into question the idea that we can identify a will that is society's own, it is not clear that the General Will argument is capable of supplying an adequate response. The reason is that this argument implausibly reduces the concept of the social will to that of the common good. Consider a contemporary variant of the General Will argument that has been defended by a number of theorists who sometimes describe themselves as "epistemic populists" (Cohen 1986; Coleman and Ferejohn 1986; Grofman and Feld 1988; Radcliff 1992).

Epistemic populists respond to social choice theory by defining the general will as an "independent standard of correct decisions—that is, an account of justice or of the common good that is independent of current consensus and the outcomes of votes" (Cohen 1986, 34). This is exactly the reduction of the social will to the common good to which I am objecting. Epistemic populists then require that voters express their sincere beliefs about the content of the general will in specific instances. Under this requirement, voters do not merely assert whatever unreflective preferences happen to pop into their heads, so epistemic populists believe that "public deliberation is guided by the principles that define ... [the general] will" (p. 34). This transforms the context and purpose of voting. Citizens are now expected to vote on the basis of good faith attempts to discern the general will. Clearly, this obviates social choice theory's search for an all-purpose aggregation function that can reveal a social preference in the absence of any restrictions on the preferences individuals are permitted to express. Moreover, under these conditions, the outcomes of votes can constitute compelling evidence about the content of the general will or common good, or so the argument goes.4

As will become clear, my own argument shares many features of this approach. But as it stands, this epistemic populist proposal does not satisfactorily explain why the outcomes of these sorts of votes should automatically count as the social will. Epistemic populists simply recruit their specially configured voting procedures to the project of determining what some antecedently specified and independently valid account of the common good requires under specific circum-

stances. Even if we concede that epistemic populist voting procedures do indeed provide reliable evidence about the common good and its imperatives, is it obvious that we establish the content of the popular will by applying them? Suppose a society conducts a vote along the lines recommended by the epistemic populist. Citizens now have good reasons to believe that the victorious policy conforms to, or is dictated by, the common good. But are they entitled to infer from this fact alone that the winning policy constitutes their own will? Only if they already know that the epistemic populists' voting procedures are an appropriate barometer of their collective will. Yet, the mere fact that those procedures are built around a plausible view of the common good does not establish that this is so: Perhaps there is a conflict between what the common good actually requires of a society and what that society might—on further reflection—choose to will for itself.

Suppose, for example, that putting a man on the moon is a complete waste of valuable social resources from the point of view of the common good: The money would be far better spent improving the condition of disadvantaged citizens. This fact alone hardly establishes that a society has collectively decided to stop funding moonshots. After all, members of society might choose to sacrifice their common good to the goal of putting a man on the moon. Perhaps they see themselves as responsible not just for their own common good but for the progress of mankind in general. In such cases of a conflict between the common good and the social will, the content of that will cannot simply be read off the best available account of society's common good, even if we determine the latter through some sort of voting procedure.

At best, then, epistemic populism justifies a kind of social decision procedure that can be expected to (1) generate meaningful results and (2) provide reliable guidance about what the common good requires on specific occasions. But these do not constitute answers to the question which I am considering and which social choice theory claims is unanswerable. That question is not: What is the common good, and what does it require? The question is: What is it that we, as a society, actually want to do? To be sure, this latter question requires that we identify some clear, unambiguous result, but it does not require that the result be mandated by the common good. Instead, it requires that in arriving at a certain result, society must "know" that it is being true to itself or must authentically express its own view (right or wrong) of what ought to happen. If we are to respond to the social choice objection, then we have to show how this can be so, and how citizens can come to know that this is so.

To the extent that epistemic populists and other proponents of the General Will argument offer any answer to this question, it comes in the form of the Socratic stipulation that the social will and the common good must ultimately be one and the same. Defining the general will as the common good properly understood effects just this equation. But as an answer to our question about the possibility of self-government, this stipulation has at least two decisive drawbacks. First,

⁴ Arrow ([1951] 1963, 85) had already anticipated this proposal in his original monograph, suggesting that it "has much in common with the statistical problem of pooling the opinions of a group of experts to arrive at a best judgment, here individuals are considered experts at detecting the moral imperative." If one assumes the existence of a "right answer," and individuals are likely to arrive at the correct decision on their own more than 50% of the time, then the probability that a majority will be right increases as the size of the relevant group increases. See Barry 1990, 292–3.

notions of the common good or the public interest are not exactly unproblematic. If we are seeking to dissolve the ambiguities and problems of the notion of a social will, then an appeal for aid to the equally vexed notion of a "correct account of the common good" is like throwing out an anchor to save a drowning swimmer. There is every reason to demand that unravelling the mystery of the social will not await a solution to the riddle of the common good.

Second, stipulating an equivalence between the social will and the common good radically distorts the relation between the idea of self-government and the notion of freedom. Intuitively, the concept of selfgovernment commands our moral attention in large part because it seems to be a condition for a certain kind of social freedom. A society that is not selfgoverning is in some way subjugated, enslaved, subordinated to alien forces, and so on, and in these senses is unfree. By contrast, a society that acts on a will that it knows to be its own is (to that extent) free in the sense relevant to the concept of self-government. But such a society is surely no less free in this sense if its will is misguided or runs counter to its common good. To establish that a society is free in this sense, we need only establish that social action is loyal to society's own view (right or wrong) of what ought to be done. If we follow the General Will argument, however, and stipulate that society's real will or general will is whatever the common good properly requires, then we completely invert these perfectly sound intuitions about freedom. If freedom is a function of acting in accordance with the social will, and the social will is really the common good, then whenever a society opts for a course of action inconsistent with the common good it is acting unfreely, because it is not (really) acting in accordance with its (real) will. On this view, assuming for the sake of argument that he understood the common good plausibly, Plato's Republic becomes the ideal of a free, self-governing political community, whereas the Athenian democracy that put Socrates to death becomes its antithesis. This is surely absurd, quite apart from Plato's idiosyncratic view of the common good. We may agree with Plato that Athens made a catastrophic mistake when it condemned Socrates, but it seems bizarre to claim that for this reason Athens was acting unfreely or was somehow subjugated to "alien forces" when it chose to convict Socrates. But by stipulating an equivalence between the social will and the common good, proponents of the General Will argument are forced into this deeply counterintuitive account of social freedom.5 The price of reducing the social will to the common good is the sacrifice of the concept of freedom implicit in our intuitive concept of self-government.

Ideally, of course, we might hope that members of a fully self-governing society both organize themselves so as to secure their common good and know that in doing so they are enacting a will that is authentically theirs. When both conditions are met, we can say that society is freely and willingly pursuing its true common good. But the means by which we determine that social practices and policies conform to the common good and that they fulfill society's will are, and should be, independent. If we conflate them in the manner of the General Will argument, we risk losing our grip on the form of social freedom that lends moral weight to the ideal of self-government.

The lesson, then, is that the second and third senses of self-government should be kept apart. It is a mistake to suppose that the absence of a social will implies that there is no way to determine the content of the common good (the first confusion), but it is also wrong to think that appealing directly to some conception of the common good and defining it as society's "real will" (the second confusion) allows us to supply a satisfactory answer to the social choice skeptic. Such a response requires a direct and free-standing account of how a political community might identify a will as its own. Below I attempt to show that the results of social choice theory at least do not shortcircuit this project.

THE ANALOGY BETWEEN INDIVIDUAL AND SOCIAL WILL

The concept of collective self-government suggests an analogy between individual agents and collectivities. According to this analogy, a collectivity can be an actor in its own right, capable of formulating and determining its own will, and thereby responsible for its own choices and self-determination. Social choice theorists typically claim that their results "demonstrate" that any such analogy "involves an illegitimate transfer of the properties of an individual to the properties of a collection of individuals" (Plott 1976, 525). The social choice theorems largely derive their shock value from their claim to expose such analogies as logically confused.

This claim is mistaken, however. Understanding why is the key to my strategy in this paper. To see this, however, it is first important to grasp the overall structure of the argument that persuades social choice theorists of this claim. This argument has four steps: (1) We can formulate a number of conditions that any social decision procedure capable of generating clear, unambiguous results would have to satisfy in order for us to certify its ability to enunciate a "will" that is authentically society's own. (2) It is logically impossible to satisfy all these conditions in a way that permits meaningful, coherent results to emerge. (3) Therefore, there is no possible decision procedure for identifying a social will since there is no way to satisfy the authenticity criteria and at the same time produce clear results. To say that no such procedure is logically possible is tantamount to showing that there is no such thing as the popular will. (4) Because we routinely assume that when individuals decide on a course of

⁵ Just this line of argument has led a host of critics to demur at Rousseau's notorious claim about forcing citizens to be free. They think that Rousseau gave the game away when he claimed that the "General Will is never wrong" and that popular participation in expressing it causes "the public to learn to know what it wants." Social choice theory tends to reinforce the suspicion that Rousseau's democratic and voluntarist terminology disguises an essentially Platonic dogmatism.

action or preference, those decisions do or at least may reflect their own will, it follows that collectivities are to this extent fundamentally unlike individuals. They are not agents in the way that individuals are because it is meaningless to think of them as having their own will.

This argument is decisive only if we are sure that the social choice theorist's conditions are reasonable constraints on purportedly will-revealing procedures. If they are unreasonable or nonessential, then the argument collapses, because the violation of those conditions will no longer automatically imply that a coherent result does not authentically represent the social will. I suggest that the conditions imposed by social choice theory are not necessarily essential or even reasonable in the context of attempts to identify a social will. If so, then will-revealing procedures may legitimately violate these conditions, and the idea that society can identify a will of its own is not threatened by the results of social choice theory.

We need to understand, therefore, the conditions imposed by social choice theorists and the implicit expectations they make of reasonable will-revealing procedures. I will focus on five conditions.⁶ The first embodies the expectation that these procedures generate transitive results:

T (Transitivity): For any alternatives x, y, and z, if x is preferred to y and y is preferred to z, then x is preferred to z.

For our purposes, T is the only relevant expectation of decisions. Preference orderings that violate T are said to be "cyclical." The problem with cyclical orderings is that they fail to communicate a clear, unambiguous preference. The relevance of this condition to the present argument is clear. If the public is to be in a position to communicate a will to governments expected to execute it, then acceptable will-revealing procedures must satisfy T. Otherwise, the procedures will enunciate cyclical judgments and thus fail to identify any clear instructions or decisions to the institutions authorized to execute the social will.

The remaining four conditions embody expectations of the amalgamation procedure that is supposed to process the inputs of each citizen into a final judgment representative of society's own will.

- P (Weak Pareto Principle): For any alternatives x and y, if all individuals prefer x to y, then society prefers x to y.
- D (Nondictatorship): There is no individual such that, for every set of individual orderings, for all alternatives x and y, if that individual prefers x to y, then society prefers x and y.
- U (Unrestricted Domain): For any set of alternatives and any set of individuals, the domain of the social welfare function includes all orderings of alternatives by individuals.
- I (Independence of Irrelevant Alternatives): For any alternatives x and y, if the preferences of all individuals as between x and y remain the same, then the

preference of society as between x and y depends only on the preferences of individuals as between x and y.

In what follows, I will refer to this set of conditions on both decision and aggregation as the *TUPID* conditions

The social choice theorems show that we cannot satisfy all the *TUPID* conditions at once: We must choose between T and one or other of the remaining four. This claim is the second of the four steps in the argument I set out above. But can this second step establish the later claims in the sequence? Can it show that there is no such thing as the popular will and that the analogy between individual and collective agency must be spurious?

To see that the answer is "no," consider the following two claims: (A1) Societies are potentially like individuals because they can have their own will, and (A2) societies are potentially like individuals because both are able to determine the content of their will in analogous ways.

Note that A1 does not imply A2. It is possible that both individuals and collectivities can have a will of their own, but that there is no resemblance whatsoever between the respective procedures under which these two kinds of agencies determine the content of their will. But A2 assumes A1: If we know that societies are able to determine their will in the same way that individuals do, then we presuppose that societies can have a will.

The difficulty facing the social choice argument is this. Social choice theorists claim to falsify A1 (there is no such thing as the popular will), but to do so they deploy an argument compatible with the truth of A2. Their argument cannot decisively falsify A1, since it is presupposed by A2.

How is the social choice argument compatible with the truth of A2? The answer is that social choice theorists (rightly) acknowledge the possibility that the TUPID conditions might also apply to the case of individual decision making (Hurley 1989, 226f; May 1954; Roemer 1996, 25-6; Steedman and Krause 1986). The standard way of making the case points out that when individuals are called upon to express preference rankings over defined alternatives (say, candidates for a professorship), they very rarely have the luxury of being swayed by a single dimension. Rather, they must rank candidates relative to a range of emergent preferences along different dimensions: teaching ability, reputation, quality of written work, affability, compatibility of research interests with members of the department, and so on. Moreover, these dimensions will very often generate widely divergent rankings. In principle, an individual seeking to arrive at a final transitive ranking must find some way to process these emergent lower dimensional rankings into an overall (all things considered) ranking, much as a society must find some way to amalgamate individuals' preferences into a social preference. If the TUPID conditions define reasonable will-revealing procedures in both these cases, then A2 has been conceded.

⁶ These conditions are taken from Hurley 1989, 229.

It is relatively easy to stipulate equivalent *TUPID* conditions for the case of intrapersonal deliberation. I will follow Hurley's (1989, 231) extremely helpful stipulation of *TUPID* analogues for individuals seeking a "coherence function" capable of processing several criterial rankings of alternatives into an "all-things-considered" ranking. Apart from *T* itself, which remains unchanged, Hurley amends the other four conditions as follows.

P* (Weak Pareto Principle): For all alternatives x and y, if all criteria rank x above y, then x ranks above y all things considered.

D* (Nondictatorship): A coherence function must not give so much weight to one criterion that it outweighs any criterion that conflicts with it under any circumstances. That is, it must not be the case that there is one criterion such that any one alternative's superiority over any other according to this criterion would always result in its superiority all things considered, regardless of how other criteria might rank those alternatives.

U* (Unrestricted Domain): For any set of alternatives and any set of criteria, the domain of the coherence function includes ordering of all alternatives by criteria.

I* (Independence of Irrelevant Alternatives): For all alternatives x and y, if the rankings of x and y by all criteria remain the same, then the ranking of x and y all things considered remains the same; that is, a deliberator's ranking of a pair of alternatives all things considered depends only on the ranking of those alternatives by all the relevant criteria, not on the rankings of other alternatives.

If it is plausible to claim that these conditions apply to individuals trying to process several emergent preferences over given alternatives, then it would seem to follow that transitive decisions can be reached only by violating one or other of the TUPID* conditions. Roemer (1996, 26) suggests that this conclusion is an "unexpected bonus" of Arrow's theorem. But it seems to me that it gives the game away. If these conditions apply in the individual case, then this undermines the ability of social choice theory to adjudicate the question of whether there is such a thing as the popular will. Once we allow this possibility, we concede the possible validity of A2, and we can no longer claim to be establishing that A1 is false.

To see that this is right, suppose it is plausible to depict the individual will along these lines. The violation of one or other of the *TUPID* conditions then becomes a generic condition of all aggregation that results in transitive outcomes, whether individual or collective. If we assume that this form of aggregation captures essential features of any will-revealing procedure (whether individual or social), then this would imply either (1) that neither individuals nor collectivities have a will or (2) that in both instances the violation of the *TUPID* conditions is a precondition for identifying such a will. Clearly, neither claim is capa-

ble of establishing that the ability to will belongs to individuals but not to collectivities.

Alternatively, the social choice theorist could simply drop the idea that the TUPID conditions apply in the individual case and say that they are appropriate only in the collective case. This at least would allow one to claim that individuals can have wills but collectivities cannot. There are two difficulties, however. First, this cannot serve as an argument to the proposition that it is futile to model collective decision on the analogy of a single individual. The argument rests on the assumption that this analogy is misguided. This simply begs the question against the claim that A2 is false. Even if we certify independently that A2 is false, this still will not falsify A1, since A1 does not require A2. This helps us understand why the social choice framework is in no position to test the claim its practitioners often see it as proving: that the analogy between collective and individual agency is flawed because collectivities cannot be said to share the individual property of having a will. Plott's claim that Arrow's theorem "demonstrates" the fallacy of treating collectivities like individuals cannot be sustained.

Second, adopting this line of argument results in a crucial shift in the burden of proof. If one concedes that the TUPID conditions need not apply to individual will-revealing procedures, then the case for their salience in the collective case is weakened. If it is permissible for individuals to override one or other of the TUPID conditions when they determine the content of their will, then why should it be impermissible for a collectivity to do so? We have no choice but to take the individual will as the basic primitive unit of comparison—there are no other places to look for accounts of how the will operates. But if it turns out that in determining and pursuing their own wills individuals routinely and legitimately violate the TUPID* conditions, then the case for expecting collectivities to satisfy them might be undermined. I will now explore this possibility, although the findings are neither conclusive nor complete.8

conception of the will: Many philosophers and theorists have defended views of the will that seem consistent with this one. Hobbes, for example, can be interpreted as suggesting that individuals are only able to arrive at decisions at all by violating the nondictatorship condition. In Hobbes's view, deliberation is nothing other than the process by which a sequence of values (embodied by a series of emergent appetites and aversions) compete for dominance within the mind of an agent. An individual finally wills an action when one or other of these competing values wins out. In this way, the victorious appetite can be said to dictate the decision. That "dictated" decision represents the individual's will, just as the arbitrary decisions of the sovereign represent the will of the collectivity after the social contract has instituted a Leviathan. So it is perfectly possible to defend a notion of the will that violates one or other of Arrow's conditions. On this point generally, see Mayo 1956.

⁸ I am not the first to suggest this general strategy of analysis. Hurley (1989, 234-41) explores a similar line of argument. But I find her argument unsatisfactory. Hurley accepts P^* and D^* as appropriate conditions on individual deliberation but rejects U^* and I^* . To my mind, U^* is the one condition that does make sense in the individual case (see Steedman and Krause 1986, 208), so my own arguments will

target P^* , I^* , and D^* .

⁷ The point here is precisely not that this results in an indefensible

RECONSTRUCTING THE INDIVIDUAL WILL

Thus far I have argued that in order to be self-governing, societies and groups need some procedure for certifying that some policy fulfills or conforms to its own view of what should happen. I maintained that such a procedure must be independent of procedures designed to determine the content of the common good. Correctly discerning the common good is one thing, and fidelity to society's own judgment is another. The latter matters more in the context of self-government. To be self-governing, a group's will must authentically be its own. I will call this the requirement of reflexive authenticity.

Social choice theorists believe that Arrow's theorem undermines the possibility of such a will because they take the *TUPID* conditions to define a minimal, but essential, standard of reflexive authenticity for collective will-revealing procedures. This is clear from the justification social choice theorists normally offer for the critical *P*, *I*, and *D* conditions. They argue that when these conditions are violated, social choice is not determined by voters' own judgments but is instead an artifact of the aggregation method (Riker 1982, 117–9). This is a technical way of expressing the demand that procedures for determining social choice should not preempt citizens' own choices. *P*, *D*, and *I* comprise social choice theory's test of reflexive authenticity.

But do these conditions represent the only reasonable standard of reflexive authenticity in the context of self-government? Perhaps there exist other reasonable interpretations of reflexive authenticity that are inconsistent with P, D, and I. I have suggested that we might seek such alternative conceptions by questioning the claim that analogous conditions $(P^+, D^+, \text{ and } I^+)$ are essential constraints on individual will-revealing procedures, although many theorists have considered this claim (Roemer 1996, 25-6; Steedman and Krause 1986, 207-10). But if it is wrong, understanding why might allow us to reconstruct a notion of a popular will that is immunized against the threat posed by Arrow's theorem. We might learn why violating one or other of these conditions is consistent with an appropriate test of reflexive authenticity, thus undermining the basis for the social choice objection to the idea of a social or popular will.

How might one call P^* , D^* , and I^* into question? It is tempting to focus on I^* . As is well known, I^* prohibits (among other things) cardinal rankings of alternatives and thus rules out claims about the relative intensity of several emergent preferences. But surely individuals should be permitted to take such information into account when they deliberate about their choices? This is certainly right, but in the present context the objection is insufficient. Relaxing the requirement of ordinality is unlikely to help us reconstruct a viable account of will formation for two reasons.

First, intensity of preference may predict behavior, but the content of one's will is another matter. Prima facie, there is no reason to expect that the content of one's will ought to be considered a simple function of the intensity of preferences, or of behavior. Our intuitive concept of the will allows, for example, that it is often weak relative to intense desires, even that it can be "betrayed" by intense temptations and actions prompted by them. This suggests that even if we had a viable measure of intensity (itself doubtful), that variable would not predict, or stand in any determinate relation to, the content of one's judgments about one's will.

Second, we must remember that we are investigating the individual will not for its own sake but for the purposes of illuminating possible collective will-revealing procedures. Taking into consideration the intensity of citizens' preferences will only be useful in the latter context if we have some interpersonally applicable standard of intensity. But there is overwhelming agreement among economists and social choice theorists that interpersonal cardinal comparisons of utility are meaningless (indeed, many think the same is true of intrapersonal comparisons). This consensus might be misguided, but relaxing I^* 's requirement of ordinality will help us reconstruct an account of the collective will only if we can defeat the argument that interpersonal comparisons of intensity are meaningless. This route seems unpromising, and it is certainly unlikely to persuade a social choice theorist.

In what follows, I sketch a more promising line of argument against P, D, and I. It hinges on the idea that practical deliberation about how to be true to oneself has a characteristic depth structure that is incompatible with the TUPID model of the will. I follow a number of philosophers in using the hierarchical metaphor of orders of desires, preferences, volitions, and reasons to explicate this important dimension of will formation. To elaborate the point, I make extensive reference to Frankfurt's (1988, 11–26) classic, "Freedom of the Will and the Concept of a Person." I use Frankfurt largely for reasons of convenience and ease of exposition; I do not necessarily endorse his views on the definition of persons or the subject of free will.

Consider the case of the tempted spouse, who finds herself contemplating infidelity because of three distinct emergent desires. First, her husband no longer satisfies her appetite for sexual experimentation and variety; sex within her marriage has become routine and has lost its earlier sparkle. Moreover, she is strongly attracted to a colleague at work, who has given strong indications of a reciprocal interest. Second, her husband cheated on her three years ago, and she has harbored a latent desire for revenge ever since. Third, the colleague is the tempted spouse's boss, and she has reason to believe that a sexual relationship with him will advance her career. Although the tempted spouse experienced emergent desires in the past (e.g., overwhelming sexual interest in her husband) that would rank fidelity over infidelity, no such desires are present now. Her emergent desires unanimously recommend infidelity, but the tempted spouse still might decide, in the face even of this unanimous recommendation, to refrain. The feature in virtue of which this is possible is the capacity for forming what Frankfurt calls "second order volitions."

Frankfurt (1988, 16) claims that we do not merely experience emergent first-order desires of the sort that dominate the mind of the tempted spouse. We also have the ability to form second-order desires, that is, desires to have or not to have certain desires. A second-order desire becomes a second-order volition when a person desires that a certain (first-order) desire move, or not move, them to act. A second order volition is, therefore, a desire to be moved or not to be moved to action by a certain desire or set of desires.9 By virtue of this capacity, the tempted spouse may choose not to identify her will with her emergent first-order desires. It is important to emphasize that this does not imply she will successfully battle these temptations; she may give in to them. The point, however, is that even if her unanimous first-order desires cause an act of infidelity, this action does not necessarily receive the blessing of her will. It is more accurate to say that temptation caused her to act against her will, which will be reflected in the selfrecrimination, shame, and guilt likely to follow her

What sort of reasons might agents have for countermanding or even disowning their first-order desires by refusing to identify them with their will? Very roughly, the most obvious can be collected under the general category of the personal ideal. A personal ideal here refers to any sustained intention, aspiration, hope, or plan to become a certain kind of person or to live up to a certain exemplar. The tempted spouse may refuse to identify her will with her emergent desires because she is antecedently committed to "being a good spouse" or "being a good Christian." If those ideals demand that agents not be moved by lust, vengeance, greed, and so on, then people who identify with those ideals may try to renounce the disqualified desires. One reason to affiliate such ideals with one's "will" is to encourage vigilance toward motivations that might deflect or undermine one's resolve to be a certain kind of person. By refusing to identify her first-order desires with her will, the tempted spouse is trying to do just this; and her will to realize whichever personal ideal is operative in her case is instantiated in the formation and pursuit of certain second-order volitions about emergent desires.

Frankfurt's account of second-order volitions allows one to distinguish between two different ways in which agents may fail to generate or be in a position to pursue their own will. First, they may fail to form second-order volitions at all; Frankfurt calls them "wantons." It is vital to note that wantons may have second-order desires, and they may have the capacity to deliberate. The distinctive feature of a wanton is "mindless indifference to the enterprise of evaluating his own desires and motives"; the wanton may experience the second-order desire to suppress his first-order desire for sex or drugs, but he is indifferent as to which should win out. It follows that a wanton could not be self-governing in Frankfurt's sense because he could not care about

fulfilling any personal ideals of the sort sketched above. Second, as illustrated by the case of the tempted spouse or Frankfurt's (1988, 16–9) example of an unwilling addict, an agent may be capable of forming a will, but it does not follow that the will is effective in generating action.

The cornerstone of a Frankfurtian will-revealing procedure, then, is a higher order review of currently emergent first-order preferences. Agents seeking to arrive at an all-things-considered judgment as to the content of their will, given a variety of rankings based on emergent first-order desires, must ask a version of the following question: If I allow my will to be determined by emergent preferences of this particular character, will I be able to think of myself as living up to some personal ideal with which I identify? If the answer is "no," the agent may disregard the recommendation. There are two things to note about this test. First, it can legitimately claim to be one of reflexive authenticity: It reveals whether agents are being true to themselves by following one preference or another. Second, it does not evaluate emergent preferences by directly assessing their recommended rankings. Instead, it assesses the character of the values generating these rankings and their relation to some operative personal ideal. Agents can disregard an emergent preference on this basis without thinking that they should ultimately repudiate the ranking it recommends.

This reconstruction of a Frankfurtian will-revealing procedure raises basic questions about the credentials of P^* , I^* , and D^* as necessary conditions for such a procedure. These conditions conflict with the Frankfurt model because they interact in a way that forces one to treat emergent preferences as social choice theorists propose to treat voters: Each is entitled to equal voice and weight in the amalgamation process. But the Frankfurt model reveals difficulties with this assimilation. Consider again the predicament of the tempted spouse. If she believes that P^* and I^* are appropriate expectations to place on the process by which she determines the content of her will, she presumably will have to identify with the course of infidelity, since this is what her emergent desires unanimously recommend.

Moreover, strictly speaking, considering second-order assessments of the desirability of living in accord with currently emergent desires is ruled out by I^* . The reason is that when the tempted spouse invokes her second-order volition as a reason for disregarding her first-order rankings, she is choosing between fidelity and infidelity precisely on the basis of a preference between alternatives that I* deems "irrelevant." I* requires that agents' final orderings be independent of information concerning alternatives not in the set of alternatives under direct consideration. In the Frankfurtian procedure, the tempted spouse reaches a final judgment about fidelity because of the way she ranks two quite different alternatives: a life led in accordance with greed, lust, and vengeance versus a life organized around different motivations. But these two possible modes of life are not themselves members of the set of

⁹ It seems possible in principle that second-order volitions may be capable of producing, or causing one to have, particular first-order desires. See, for example, Raz 1986, 32–3.

alternatives under direct consideration, a set that includes only fidelity and infidelity on this specific occasion

It is tempting to object that the tempted spouse's second-order volition to disregard her first-order desires can itself simply be counted as an emergent preference for fidelity over infidelity, on a par with the other first-order emergent preferences. On this supposition, it would no longer be true that the tempted spouse's emergent desires unanimously recommend infidelity over fidelity, since there is now an additional preference for fidelity. This objection misfires in two ways, however. First, stipulating that the second-order volition counts as an emergent preference for fidelity may allow the Frankfurtian procedure to satisfy P^* , but only by raising a new question about D^* . D^* requires that there be no criterion or emergent preference dimension such that it determines the final ranking regardless of the rankings recommended by other emergent preferences (see Riker 1982, 118). But if we count the second-order volition as a direct preference for fidelity over infidelity and the tempted spouse opts for fidelity over the objections of all her other preferences, this requirement is violated. Given its special importance in the tempted spouse's self-image, the personal ideal of "spouseworthiness" functions in just such a "dictatorial" way in the formation of her will. But in combination with I^* and P^* , D^* denies this sort of special privilege to particular preferences. Instead, these conditions hold will formation hostage to a principle of "one desire, one vote." As the tempted spouse illustrates, however, the process of will formation is not necessarily well captured within the terms of this principle. Deliberation about the content of one's will and about how to be true to oneself often is precisely and legitimately about which preferences to privilege over others and about which alternatives are relevant in a particular instance. P^* , D^* , and I^* take these issues off the deliberative agenda by deciding them in advance according to an arguably inappropriate principle of equal consideration.

This hints at the second way in which the objection misfires. The objection simply stipulates that we assimilate second-order volitions and first-order emergent rankings of the alternatives. It may well be possible to retrieve a rationale for the TUPID* conditions once we make this stipulation, but it begs the critical question of whether we must view second-order volitions in this way. If this stipulation is independently implausible (because it fails to detect relevant differences between these two kinds of emergent preference), then so much the worse for the TUPID* model of the will. And the stipulation is implausible for this reason. Second-order volitions cannot simply be assimilated without qualification to other first-order rankings of the alternatives. A second-order volition not to be moved to X (where Y is some alternative) when prompted by first-order desires A, B, and C is not adequately described as a preference for Y over X. There may be other first-order desires (P, Q, R) that, if emergent, also would recommend X but to which the operative second-order volition has, as it were, no objection. This is hard to see

in the tempted spouse example, because it is difficult to imagine "spouseworthy" motivations for infidelity, but not in other cases. John may choose not to date Sarah because he thinks his desires for doing so are suspect (lust, a desire to get back at her current husband, wanting to access Sarah's millions), but he remains open to the possibility that other desires for going out with Sarah (e.g., true love), if they emerge, could be allowed to determine his will.

This does not mean that the tempted spouse's eventual will cannot be seen as expressing a certain kind of desire to refrain from infidelity. That description remains available, subject to an important qualification: The tempted spouse's eventual desire to refrain from infidelity is the result of a deliberative assessment of her emergent desires on this occasion, not one of the initially emergent desires for or against fidelity. Appealing to the ideal of spouseworthiness and associated second-order volitions is a method for arriving at a final judgment, and the outcome of this appeal can legitimately be described as a desire to refrain from infidelity. But this does not mean that in the first instance the second-order desire is itself a desire for or against fidelity. Rather, it is essentially a desire not to be the kind of person who acts on the sorts of motivations implicated in the first-order rankings of fidelity and infidelity.

This is not to deny that the desire to live up to the operative ideal is itself an emergent desire, but it only ranks fidelity over infidelity indirectly, by rejecting the motives on which the initially unanimous first-order recommendation is based on this occasion. Assessing the relation between one's currently emergent firstorder rankings and some privileged personal ideal is surely a perfectly legitimate way to process one's initially emergent preferences into a final judgment about one's will on a given occasion. As long as we take P^* , I^* , and D^* seriously, however, we must regard such a procedure with suspicion, for these conditions prohibit agents from assigning any greater weight to second-order volitions than to any other ranking. Yet, flattening out the will in this fashion arguably misrepresents the special character and legitimate role of privileged second-order volitions in the formation of the will. Substantive doubts of this sort cannot be removed simply by stipulating a priori that this compressed model of the will is the correct one.

RECONSTRUCTING THE COLLECTIVE WILL

I have called into question the appropriateness of P^{\bullet} , I^{\star} , and D^{\star} as conditions under which individual agents determine the content of their will, given emergent desires or criterial rankings of the alternatives under consideration. Can the lessons of this analysis be extrapolated to the case of collective self-government and, in particular, democratic self-government? At first sight, this seems unlikely, because I have claimed that it is problematic to treat agents' emergent preferences as if they were voters expressing judgments entitled to equal consideration. But this is not so problematic in the collective case, in which we are dealing precisely

with voters' judgments, and to which the democratic principle of equal consideration seems entirely appropriate. Perhaps the argument I have given simply shows that the individual psyche is typically not organized according to democratic principles. But if a democratic polity is just a group of individuals whose own views about public policy must be weighted fairly, then a presumption in favor of equal consideration may seem to follow very naturally, and with it a cogent rationale for *P*, *D*, and *I*. It is because these conditions seem to express a norm of *democratic* reflexive authenticity that social choice theory is so disturbing to conceptions of democratic self-government.

I will now show how the Frankfurtian argument offered above helps us see why P, D, and I may not be essential requirements for any reasonable standard of democratic authenticity. The discussion of second-order volitions questioned the requirement that the individual will depend exclusively on the profile of currently emergent preferences without any assessment of their character in relation to operative ideals. Does this proviso carry over to the collective case?

Consider the case of a jury in a criminal trial, as that institution is currently understood in common law jurisdictions. Suppose that initially the jurors are unanimously in favor of conviction, but upon deliberation it emerges that this unanimity is largely an artifact of prejudice, tendentious interpretation of the evidence, and disregard for the reasonable doubt standard. Under these (hardly exotic) conditions, the jurors might reasonably decide that to permit their joint will to be determined by these initially emergent preferences would fall short of the ideal of juryhood that defines their association. In that sense, the verdict implied in the initial consensus could not represent their will qua jury. But at this point the jurors need not conclude that they will eventually acquit the defendant. Their rejection of the original recommendation to convict was not based on a direct judgment of the proper verdict but on an indirect assessment of the way it was reached. No definitive conclusion about the proper verdict can be inferred at this stage. The jury simply resolves to continue the discussion until it reaches a verdict on terms compatible with a certain ideal of how juries ought to make decisions. Suppose that eventually it votes unanimously for acquittal after a sincere effort to apply the reasonable doubt test.

Keeping this example in mind, suppose that we must determine rules and conditions under which the votes of a jury can genuinely be described as expressing a jury's will. Must we accept P, D, and I as appropriate conditions? If so, we could not make any clear distinction between the two votes reached by the jury in the example. Both the initial vote for conviction and the later vote for acquittal are unanimous, and accepting either is compatible with P, D, and I. From the viewpoint of the TUPID model, the only salient difference between the two votes is the time at which they occurred and the fact that at those moments the pattern of emergent preferences was different. But P, D, and I by themselves give us no basis on which to prefer one to the other, apart from the Hobbesian

ground that the later unanimity immediately preceded the moment of resolution and so was contingently decisive. But the jury need not have changed its mind. Had it not done so, by the lights of P, D, and I, the earlier decision could have stood equally well as the jury's will.

It is not obvious that this is the only conceivable or reasonable standard of reflexive authenticity for juries. From another point of view the first unanimous verdict represents the jury's will less successfully than the second, but this perspective will elude us as long as we think of a jury as nothing more than a collection of separate individuals free to use whatever criteria they wish to determine their own view of the appropriate verdict. If this is what a jury is, then P, D, and I will seem perfectly appropriate, and the failure of these conditions to discriminate between the two verdicts becomes unproblematic. For on this understanding, a jury is simply an empty vessel whose character is determined entirely by the preferences of the individual jurors who happen to make it up at a given moment. Since both unanimous verdicts in the example represent them equally well (at different times), there is no reason to expect that one verdict represents the jury's will more successfully than the other.

Clearly, this is not the only available conception of a jury. Another defines it as a form of association that demands that its members be moved by certain considerations rather than others. A group could completely disregard the appropriate considerations and perhaps reach joint decisions, but it could not reasonably regard them as reflecting a will that is theirs qua jury. It would be basing decisions on considerations that are, in effect, unworthy of a jury. Indeed, in a sense they are failing to be a jury, or at least failing to behave like one. Adopting this more restrictive conception does not in any way show that the ideal of juryhood is itself worthy; jurors might have their own excellent reasons for wanting to subvert conventional legal practice. But as long as that ideal is partly definitive of a jury (for good or ill), a standard of reflexive authenticity for juries can reasonably demand that jurors take that ideal seriously. Moreover, it seems clear that the jury in the example fulfills this expectation. When they rejected the initial verdict, jurors did so because they reflected upon their initially emergent preferences and found their character is inappropriate given their self-image as a jury. Insofar as they aspired to juryhood, they had to conclude that their first verdict could never count as their will and must be revised, even though unanimous. Although P, D, and I do not disallow this revision, they also do not require it; these conditions could allow the first verdict to count as the jury's will. But a standard of reflexive authenticity based on the alternative conception of juryhood might well require the jurors to disallow the first verdict, so that alternative view must exclude P, D, and I.¹⁰ It follows that if this alternative

¹⁰ Suppose, for example, that after the first unanimous vote, the jury holds a second vote on whether to discuss the verdict further or let the recommendation stand. In this second vote, all but one agree to let the conviction stand, because they want their jury duty to be over

conception is reasonable, then we have found at least one collective will-revealing procedure in which relaxing the *TUPID* conditions is innocent.

One might object that there are fundamental differences between a jury and the sovereign electorate of a purportedly self-governing democracy. There is some truth to this: Directing democratic debate about public policy along the narrow lines defined by a judge who instructs a jury to disregard all criteria apart from the reasonable doubt standard would surely impinge unduly upon democratic freedoms. Moreover, in the jury example constraints are imposed externally by the judge and the wider legal apparatus in which the jurors find themselves immersed, but the idea of democratic self-government seems to require that any relevant restrictions be imposed from within.

The force of these points is easily exaggerated, however, particularly if we draw an unnaturally sharp line between procedure and substance in the context of democratic practice. Stressing the way in which democracy asserts the priority of certain decision procedures over substantive political viewpoints can obscure the way in which democratic procedure can itself function as a substantive social ideal. That social ideal need not specify any particular view about the goals of public policy. But, as in the jury example, it unavoidably defines a certain ideal about the terms on which certain policy alternatives are to be identified as the will of a democratic public. To the extent that citizens represent themselves as members of a collectivity with the distinctive character of a democratic public, they may reasonably impose restrictions on the range of firstorder values and motivations that could in principle determine their joint will. Moreover, the justification for such restrictions need not be based upon an elaborated and defensible theory of the public's best interests, or of the common good. To establish that such a democratic public is self-governing, it would be necessary only to justify restrictions on the following more slender ground: They are a necessary condition for plausibly ascribing to the authors of the social will the distinctive characteristics of a democratic public.¹¹ If citizens see themselves as members of such a public, they would be untrue to their collective character if they were to ignore such restrictions.

These restrictions will vary according to the content of the relevant democratic ideal, but it is not difficult to imagine likely candidates. Excluded motivations might include, minimally, and formally: determining votes by all consulting the same oracle and following its directives or by tossing a coin. More substantively, they might include ranking alternatives entirely on the basis of judgments about irrelevant features of the people

and done with. In our example the lone holdout is right to think that the initial vote, albeit unanimous, may not represent a will that is theirs qua jury, so a will-revealing procedure based on our alternative understanding of juryhood might require that his single vote determine their will as a jury at that moment—in this case to continue deliberation. But this seems to require relaxing D.

who advocate them (e.g., skin color, ethnic origin, and religious affiliation) and determining one's vote on the basis of criteria that give absolutely no weight to, or completely discount, the values of free and open debate, compromise, accountability, equal consideration, and transparency. These restrictions neither amount to nor necessarily rest on a particular view of the common good or the substantive goals of public policy, but a society that ignores these desiderata clearly fails to meet expectations implicit in the idea of democratic agency. As in the jury example, citizens might reasonably conclude that when a society acts on the basis of such motivations and values it fails to live up to the ideal of an open, inclusive democratic society.¹² Unlike the jury example, it is not obvious that these standards are necessarily "externally imposed" upon citizens. They simply may be embedded deeply in the institutional history and discursive practice of a given polity, such that it would seem very unnatural to regard citizens who apply them as appealing to anything alien or extraneous.13

The central point is that practices of compromise, open debate and inclusion, and the associated deliberative commitments to values such as fairness, consideration, and freedom are partly constitutive of democratic agency. To the extent that restrictions essential to these practices fail to operate, one may legitimately doubt whether the results of the process can qualify as the will of a group with the distinctive character of a democratic public. By contrast, P, D, and I involve an interpretation of democratic authenticity that pays no attention to the character of the motivations behind individual votes and preferences. The only information

Or they are characteristic of some polity that aspires to some more substantive social ideal that is nested within and compatible with democratic ideals and procedures (e.g., "the American way").

¹² The relevant restrictions would therefore be what Goodin (1986, 78) helpfully calls "input filters."

¹³ This may imply a certain conservative bias, in the sense that embedded social ideals (such as democracy) require citizens to acknowledge an overriding allegiance to the extant practices, cultures, and institutions in which such ideals are latent. But perhaps that is necessary in order for a political group to have a persisting identity over time such that it can be in principle the subject of its own destiny. This tends to vindicate the Aristotelian idea that a society's constitution is both the source of a local consensus on certam social values and a political society's essential identifying attribute (Aristotle 1981, 176). Perhaps the real lesson of the impossibility theorems is that a social will cannot emerge in a social vacuum: There must be at least some nonarbitrary constraints informing citizens' joint self-understanding as a society organized around certain ideals and values. Only then can they see themselves as participants in an overriding political identity to which their joint decisions and actions may (or may not) be loyal. But note that constraints of this kind are crucially different from those imposed by the various forms of the General Will argument we considered earlier. Under that argument, the constraints are imposed by some independently authoritative account of the common good so as to increase the likelihood that the right answer will emerge from appropriately designed decision procedures.

¹⁴ This does not mean that democratic publics must consciously take these values as their concrete goals, only that consciously or not, these restrictions must in fact shape the character of the political process. The property of acting like a democratic public may, for example, be what Elster (1986, 122–7) calls a by-product. This highlights another difference between the line of argument developed here and epistemic populism. In the latter view, citizens are required to embrace consciously a particular collective goal (some substantive view of the common good) as part of the political process. In my view, this need not be so.

that is allowed to count concerns patterns in the list of individual rankings offered by citizens. This is the same "empty vessel" interpretation of reflexive authenticity discussed earlier. The character of a democratic public can be whatever the preferences of its members make it. From a Frankurtian viewpoint, democratic authenticity requires second-order restrictions on the sorts of motivations and values upon which voters' judgments are based. The standard objection to such restrictions is that when they are applied, "social choice is based as much on the restriction[s] as it is on individual judgments" (Riker 1982, 117). The response is to insist that although such restrictions may not be based on individual judgments on specific occasions, observing them may nevertheless be a necessary condition for certifying that the individuals expressing those judgments comprise a group with the distinctive character of a democratic public. The criteria for attributing to a collectivity a democratic character automatically entail various restrictions on the range of motivations allowed to constitute its will. Once we see this, we can dismiss the charge that such restrictions necessarily preempt democratic authenticity because they are "imposed upon" a democratic public as if from outside. Frankfurtian restrictions are presupposed in the very concept of a democratic public itself. In a sense, they cannot be imposed; if they are not operative, there simply will be no democratic public on which to impose. That is why they can legitimately be regarded as essential to an appropriate test of reflexive authenticity for democratic publics.

Given this, citizens who think of themselves as members of a democratic public have reasons to reject P, D, and I as criteria of democratic authenticity. Frankfurt's terminology helps illuminate what is at stake: For a democratic public to endorse P, D, and I as its criteria of reflexive authenticity would institutionalize wantonness. In virtue of its accepted self-image, such a public would have a second-order desire to fulfill its aspiration to be genuinely democratic. But the effect of endorsing P, D, and I would be to deny any special privilege to that ideal in the formation of its will. Such an authorization would instantiate wantonness by undermining the basis for any plausible discrimination between the fulfillment of society's aspiration to be democratic and those first-order preferences or emergent desires that might signify its betrayal.

Replacing P, D, and I with a more restrictive criterion of democratic authenticity does not itself guarantee that such betrayals will not occur (although it might imply institutional—perhaps in particular constitutional—mechanisms to disable or impede them). But this does not affect the key theoretical point that from the perspective of an ideal of democratic self-government it is irrational to institutionalize wantonness. Surely, there is a fundamental difference between a society whose aspiration to be genuinely democratic has been betrayed in practice and a society that has institutionalized a license for this kind of betrayal. It is the difference between Frankfurt's wanton and Frankfurt's unwilling addict (or the tempted spouse). From the Frankfurtian perspective, Arrow's theorem becomes

much less disturbing to the idea of democratic selfgovernment. The claim that the *TUPID* conditions cannot result in a coherent will can now be interpreted simply as a formalization of the claim that wanton collectivities, like wanton individuals, are systematically unable to form a will.

CONCLUSION

Clearly, these arguments are not intended as a solution to Arrow's theorem on its own terms, if by that one means an all-purpose algorithm for guaranteeing that voting procedures will not result in cyclical preferences. My argument also is not meant as a refutation of the theorem; I do not believe that Arrow's theorem can be refuted. Rather, I have attempted to put the impossibility theorem in proper perspective within the context of self-government. Social choice theorists have made strong claims that Arrow's theorem establishes the logical absurdity of the notion of a popular democratic will. That is only true if the expectations imposed by social choice theory are necessary conditions for any reasonable will-revealing procedure. I have suggested that this assumption is dubious. We may have been looking in the wrong place for democratic will-revealing procedures and tacitly relying on an unreasonably stringent criterion of collective self-government.

The specification and application of my broader understanding of will formation are complex matters and face many difficulties. The result may not be an understanding of the social will with the formal elegance and rigor offered by the social choice model. That would be a real loss. But there is also the potential for compensating gains. A wider notion of the social will may illuminate a number of issues that the social choice model leaves obscure. In closing, I offer three thoughts along these lines.

First, many insights are offered by social choice theorists. I merely claim that the model of will formation presupposed by Arrow and his successors is not uniquely reasonable and appropriate to all choice situations. Furthermore, some choice situations may not be governed by relevant second-order volitions. In such cases, the TUPID conditions may yet be appropriate, and it will follow that there is no way to determine the content of the social will with regard to the specific alternatives under consideration. Conversely, the presence of second-order volitions may cause preferences over alternatives to have the property of single-peakedness (Riker 1982, 128). It has long been understood that where this property exists, the Arrow problem does not arise. Integrating social choice theory into the broader perspective proposed here may allow us to define more sharply the preconditions for the formation of a social will.

Second, the perspective offered here brings into the foreground questions about the interface between second-order volitions that define citizenship and those that operate in other arenas. For example, individuals have personal ideals, and the ideals of democratic self-government also make demands on them. Different personal second-order volitions might threaten or

facilitate the organization of a democratic will, and it is important to know which. Moreover, semipublic institutions such as political parties form wills using their own characteristic second-order volitions. Perhaps personal identification with such ideals being a "loyal Republican" or a "faithful trade unionist," and the sanctions imposed by such institutions for disloyalty, may help reduce or increase conflicts between personal ideals and ideals of citizenship. Understanding the role of semipublic institutions might tell us much about the conditions under which a public will can be organized. There is also the related question of the range of motivations affiliated with particular parties or groups that a given ideal of democratic citizenship will tolerate: Unfortunately, the people of Austria and Northern Ireland have recently found themselves embroiled in just this issue. Arguments to the effect that certain kinds of parties, and the values that identify them, are "beyond the pale" or inimical to the conduct of democratic decision making may be essential to the practice of democratic self-government.

Third, the Frankfurtian position preserves the idea, deeply embedded in ordinary usage, that wills, once identified, may nevertheless be betrayed in practice. Drawing on Aristotle, Hurley (1989, 136-7) gives a fascinating illustration of how this idea can have collective applications. Members of a democratic public may "will" an end to racial discrimination (perhaps because they take this to be mandated by democracy itself), yet find that their own will is frequently betrayed in their interactions as private citizens (de facto discrimination, powerful groups using their favored position to circumvent or exploit loopholes in relevant statutes). The line of argument opened here provides a framework for analyzing this kind of phenomenon and can help us perceive a politics of the will in the characteristic discourse that develops around the attempt to impose legal and social controls on citizens'

These suggestions are sketchy and present serious difficulties, but there is no reason to suppose a priori that these difficulties are impossible to overcome. The social choice theorists suggested that the notion of a collective will is indeed an a priori impossibility. If the argument of this article has succeeded in dispelling these alleged impossibilities and in opening up a viable line of argument, the progress, though modest and incomplete, will nevertheless be real.

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The Fear of Death and the Longing for Immortality: Hobbes and Thucydides on Human Nature and the Problem of Anarchy

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Recent challenges to the modern secular state invite us to reexamine the arguments made by its theoretical founders, especially Hobbes. Hobbes argues that the desire for security is the most reliable and rational desire of our nature, and the state based on satisfying that desire is fully in harmony with human nature and therefore fully capable of solving the problem of anarchy. I will examine his argument that anarchy, although in some sense the natural human condition, can be overcome once and for all through political institutions that ensure the rational fear of death will control humans' destabilizing hopes and longings for immortality. I then turn to Thucydides, the classical thinker whom Hobbes admired most and who seems closest to Hobbes in outlook, and consider his more somber thesis: Because human hopes for immortality are more powerful than the fear of violent death, anarchy will return over and over again.

uring the past quarter-century, powerful challenges have emerged to that hitherto mighty Leviathan, the modern secular state. In the Islamic world, India, and Israel, religious movements have weakened and in some cases overthrown modernizing, secular governments.1 In the former Soviet Union and former Yugoslavia, ancient ethnic and religious loyalties and hatreds have crippled and in some cases shattered state power.2 More generally, the modern state, which Nietzsche (1967, 160) described as "the coldest of all cold monsters," seems to be losing its hold over the hearts and imaginations of postmodern human beings. Throughout much of the world there seems to be an antipolitical mood, an anarchism of the heart, which manifests itself in different ways but is recognizable to all.

During the past few years, observers in America and Europe have written of "The Coming Anarchy" (Kaplan 1994), "Ethnarchy and Ethnoanarchism" (Tamas 1996), Pandaemonium (Moynihan 1993), and The Balkanization of the West (Mestrovic 1994). They wonder whether the age of the modern state is over and whether we are returning to a premodern epoch (Hassner 1995, 338–9), a world "in which the classificatory grid of nation-states is going to be replaced by a jagged pattern of city-states, shanty-states, nebulous and anarchic regionalisms" (Kaplan 1994, 72), a world in which the primary problem of security "is not the desire for power or expansion, but rather the breakdown of States" (Delmas 1996, 7).³

These challenges invite us to reexamine the arguments made by such theoretical founders of the modern state as Locke, Montesquieu, and above all Hob-

bes.4 The modern state was based not on humans' hopes for salvation or their desire to fulfill their political natures, but on their fear of death and desire for self-preservation. The thesis of Hobbes in particular is that this desire for security is the most reliable and rational desire of our nature, and any state based on satisfying that desire is, unlike premodern forms of political organization, fully in harmony with human nature and hence fully capable of solving the problem of anarchy. I will explore Hobbes's argument that anarchy can be overcome once and for all through political institutions that ensure the rational fear of death will control destabilizing hopes and longings.5 Then, for a critical perspective on this argument, I will turn to the classical thinker Hobbes admired most, Thucydides, who believed that individual statesmen and regimes can effectively address the problem of anarchy to a considerable extent, but anarchy will nevertheless return repeatedly as long as human nature remains the same.6 I conclude that the fundamental reason Thucydides does not share Hobbes's hope that the problem of anarchy can be solved once and for all is that he does not share Hobbes's hope that the fear of violent death can lead humans to master their destabilizing hopes. Thucydides argues instead that human hopes, especially for immortality, tend to overwhelm human fears, even of violent death. Hobbes is hopeful precisely because he believes that the power of hope can be tamed by fear, whereas Thucydides is not

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¹ Consider, for example, Huntington 1996; Juergensmeyer 1993; Kepel 1994.

² See especially Fairbanks 1995, 1997; Ignatieff 1993, 3-56; Tamas 1994, 1996.

³ See also Hardin 1995, 226-8, Hassner 1995, 23-61, 335-54, 371-91; Huntington 1996, 33-6; Kaplan 1996.

⁴ See Mansfield 1971, esp. 109–10, 1989, esp. 153–8; 1996, 281–94, esp. 294; Skinner 1978, 2:353; 1989, 120–3; Tuck 1993, xvii; xiv, 348; 1996a, ix, xiv. On the relation between Hobbes and such liberals as Locke and Montesquieu, consider MacPherson 1970, 1–4, 256–7, 270; 1982, 24–5, Tuck 1990; 1993, 345, 333; 1996a, xxxiv See also Skinner 1965, 171–8; 1966, 295–303 Consider as well Emenach 1981, Oakeshott 1960, lvii; Pitkin 1967; Ryan 1996, 234, 237; Strauss 1952, 157; 1953, 165–6, 202–51; Tarcov 1984, 34–66, 245–7.

⁵ Regarding Hobbes's hopefulness, consider Tuck 1996a, xxxix; see also xxvi; and Strauss 1952, 138. Consider as well Kateb 1989, 367; Kraynak 1990, 6, 102–3, 189–90, 207, Strauss 1953, 194.

⁶ For the influence of Thucydides on Hobbes, see Hobbes's Thucydides, 1975, 6-27, as well as 580, n. 5 (all references are to the Schlatter edition). See also Bull 1981; Johnson 1993; Klosko and Rice 1985; Orwin 1988, 839-41; Pouncey 1980, 151-7; Ryan 1996, 209-10; Skinner 1996, 229-30, 235, 242, 244-9, 282-3, Strauss 1952, 44, 59. But consider Kraynak 1990, 23 n. 15 on Hobbes's admiration for Diodorus Siculus.

hopeful precisely because he believes hope is invincible.

A number of scholars overlook this fundamental difference between Hobbes and Thucydides. Cogan (1981, 1987) contends that "the two share an almost identical view of the nature of mankind, of its motives, of the origin of those motives, and of the usual result when this nature is allowed a free rein to operate."7 Other scholars, who have noted that Thucydides is less hopeful than Hobbes in his view of the problem of anarchy and human nature, agree with Pouncey (1980, 43): "The principal difference between Hobbes and Thucydides on this point is the difference between a political theorist and a historian."8 Yet, by claiming that his work will be useful for those who seek clarity about not only the past but also the future and is therefore "a possession for all time," Thucydides explicitly makes the theoretical claim on behalf of his book that it will reveal the permanent and comprehensive truth about human affairs and human nature (Thucydides 1963, History, 1.22.4).9 He thereby indicates that he is, in a crucial sense, a political theorist.

I maintain that the principal difference between Hobbes and Thucydides is their theoretical disagreement over whether the fear of violent death or the desire for immortality is the stronger element within human nature. Orwin and Slomp offer especially helpful observations concerning this matter. Orwin (1988, 845) remarks that "Thucydides knows better than to anticipate Hobbes in hoping for too much from fear," and Slomp (1990, 574; see 581, 586) points out that "Hobbes refuses to share the pessimistic Thucydidean position according to which, as a rule, hope of success prevails over fear of failure." I argue, however, that the reason Hobbes and Thucydides disagree over whether the problem of anarchy can be solved lies in their disagreement over whether the destabilizing hope for immortality in particular—that is, the hope that one can somehow overcome one's mortal nature and live on after death—can be controlled by the fear of violent death.

HOBBES: THE FEAR OF VIOLENT DEATH AND THE ESCAPE FROM ANARCHY

Even though Hobbes believes the natural condition of human beings is the war of all against all, he also believes it is possible for humans to escape that state of "meer" nature once and for all. "If the moral philosophers had as happily discharged their duty" as have the geometricians,

I know not what could have been added by human industry to the completion of that happiness, which is consistent with human life. For were the nature of human actions as distinctly known as the nature of quantity in geometrical figures, the strength of avance and ambition, which is sustained by the erroneous opinions of the vulgar as touching the nature of nght and wrong, would presently faint and languish; and mankind should enjoy such an immortal peace, that... there would hardly be left any pretense for war (Hobbes 1972a, Epistle Dedicatory, 91, emphases in original).¹⁰

Hobbes suggests here that the problem of anarchy, at least on the domestic plane, can be solved definitively. According to him, anarchy is not a necessary consequence of human nature but, rather, an accidental consequence of the "erroneous opinions" of particular human beings.

Though nothing can be immortall, which mortals make; yet, if men had the use of reason they pretend to, their Common-wealths might be secured, at least, from perishing by internall diseases.... Therefore when they come to be dissolved, not by externall violence, but intestine disorder, the fault is not in men, as they are the *Matter*; but as they are the *Makers*, and orderers of them (Hobbes 1982, xxix, 363, emphases in original).¹¹

Although the state may not, perhaps, ever be free of the threat of foreign war, 12 it can "be secured" from the threat of civil war if only it is properly ordered by human beings who have, thanks to Hobbes, surpassed the moral philosophers in their understanding of human nature. Insofar as "at least" civil war is avoidable through human efforts, then, it is indeed possible, according to Hobbes, for mortals to create an "immortal peace." In order to understand how this is possible, let us turn to Hobbes's account of human nature.

What distinguishes man from the other animals, according to Hobbes, is foresight and hence the awareness of death. Man is the mortal being who is aware that he is such. The consequence of this awareness is constant anxiety, "a perpetual solicitude of the time to come," and hence a tendency to live, as it were, in the future (Leviathan, xii, 168-9; cf. Strauss 1959, 176, n. 2). Just as "man is famished even by future hunger," so is he tormented by future sufferings, that is, by the possibility of future sufferings (Hobbes 1972b 10.3).13 Accordingly, man not only fears death and seeks to avoid it in the here and now but also seeks to "secure himself against the evill he feares" for the future (Leviathan, xii, 169, emphasis added). He seeks not only to be secure but also to feel secure. He desires specifically to feel assured that he will continue to be secure in the future so that he may be free, not only

⁷ Brown (1987, 34; see 40) maintains that "virtually every part" of Hobbee's argument in chapters 11 and 13 of *Levathan* "finds its parallel in Thucydides." Even Schlatter (1945, 357, see 358-60, 362) suggests that "Hobbes found in Thucydides concrete examples of how human nature performs: the descriptions in the *Levathan* of how men act read like generalizations from these examples." Consider as well Connor 1984, 99.

See Pouncey 1980, 156-7. Consider Johnson 1993, 70, but see 69, 199-200; and Schlatter 1945, 357-62.

All references to Thucydides are to the Jones and Powell edition (1963). All translations are my own. See also 3.82.2, as well as 1.76-7, 2.50-3, 3.45, 4.61, 4.108.4, 5.89, 103, 105. Consider Hobbes 1975, 6-7; Bolotin 1987, 7, Orwin 1994, 3-8.

¹⁰ Hereafter cited as De Cive; all references are to the Gert edition (Hobbes 1972a).

¹¹ Hereafter cited as *Leviathan*; all references are to the Macpherson edition (Hobbes 1982).

¹² But consider Kraynak 1990, 113, and see also 29, 31, 95-6, 170, 186, Pangle 1976, 339-40; Tuck 1996a, xxvl. See as well Kateb 1989, 379; Strauss 1953, 194.

¹³ Hereafter cited as *De Homme*; all references are to the Gert edition (Hobbes 1972b).

from death but also from the gnawing fear of death (xii, 169; consider xiii, 186). Consequently, since "the object of mans desire, is ... to assure for ever, the way of his desire," he must desire an immortal security in order to be secured absolutely and "for ever" against future evil and future death (xi, 160-1, emphasis added; see xix, 247-8). The fundamental desire of the human animal is the unlimited desire for self-preservation.

Yet, even though Hobbes sometimes goes so far as to speak of this desire as a "necessity of Nature,"14 he also acknowledges that human beings are at times willing to sacrifice their life, especially for the sake of honor or revenge.¹⁵ There, are, then, two sides of human nature: the anxious, death-fearing side and the spirited or vain, honor-seeking side (see Strauss 1952, 18; 1959, 192). Both sides lead to a state of war. Just as the proud desire for honor—namely, that others honor you more than they honor themselves-leads humans to attempt "to extort" honor from others by force, so the anxious desire for security leads humans to subdue or kill preemptively anyone who might possibly threaten their life (see Leviathan, xiii, 184-5; Kavka 1983, 309). Hobbes does not contend that these desires always lead to actual fighting, only that they always lead to the danger of fighting: "The nature of War consisteth not in actuall fighting; but in the known disposition thereto, during all the time there is no assurance to the contrary. All other time is PEACE" (Leviathan, xiii, 186; see Kavka 1983, 292). Because no society up until now has successfully "assured" or guaranteed that fighting will not arise, that is, because all societies hitherto have laid their foundations "on sand," all societies hitherto have existed, according to Hobbes's strictest definition, in a state of war (Leviathan, xviii, 233; xx, 260-1; xxiv, 296; see Johnston 1986, 213; Mansfield 1971, 104-5).

But if the threat of anarchy is rooted in human nature, then how can that threat ever be overcome once and for all, to be replaced by the security of an "immortal peace" (see *Leviathan*, xxxi, 395)? The answer, of course, is the Leviathan, that artificial state whose goal is only peace, which subordinates all other possible goals—such as justice or virtue or salvation or truth itself—to this goal, and which aims therefore at peace at any price. This state must possess absolute power as a necessary means to achieve the limited aim of peace, above all, in order to "bridle men's ambition, avarice, anger, and other Passions" through the threat of punishment. Hobbes emphasizes that the absolute power of the state cannot, by itself, establish peace, since the threat of punishment alone will not

always deter people from rebellion. Accordingly, the state must also enjoy the enlightened, moral support of the people. The people must believe, in other words, that the state has right, as well as might, on its side (see *Leviathan* xxx, 376–85).¹⁸

Yet, how can we human beings, whose vain and anxious nature leads us to anarchy, be led to embrace the artificial state, whose goal is to avoid anarchy at all costs? We must first experience the horrors of the state of war (Leviathan xviii, 236-7; xlvi, 699). According to Hobbes, the two principal natural passions of human beings are the desire for security and the desire for honor, and both passions are equally natural, but they do not both equally lead to the natural state of war. Your anxious desire to preserve yourself may lead you to kill others, but your fundamental desire is to be left alone, in peace, and this desire does not necessarily lead you into conflict with others who feel that desire. In contrast, your desire for honor does necessarily lead you into conflict with others, since in seeking honor from others you demand that other human beings, who are just as self-regarding as you are, honor you more than they honor themselves (see xiii, 184-5).19 In order to induce humans to quit the state of war, the natural desire to preserve oneself, or the fear of death, must be inflamed and instructed, while the natural desire for honor must be weakened and controlled. How is this conquest of honor-loving human nature to be achieved?

The solution to the natural state of war lies precisely in the natural state of war. For it is primarily those who enjoy security and consequently forget their primary, natural insecurity who long for honor. "All men naturally strive for honour and preferment; but chiefly they, who are least troubled with caring for necessary things And therefore it is no marvel, if with greedy appetites they seek for occasions of innovations" (De Cive 12.10). The love of honor is based on the splendid illusion, born of plenty and safety, that we should not trouble ourselves with mere life but should seek honor from others and from future generations. The long experience of comfort and security leads humans to forget their vulnerable nature, to unlearn their natural anxiety, and breeds in them that confidence and "excessive self-esteem [that] impedes reason" and threatens peace (De Homine 12.9). Hence, cruelty proceeds "from Security of their own fortune" and "Man is ... most troublesome when he is most at ease" (Leviathan, vi, 126; xvii, 226; see xxvii, 341-2). Hobbes suggests that the experience of security stifles our natural fear of death, strengthens our natural vanity and love of honor, and eventually leads to the state of war and the destruction of our security.

But precisely insofar as the blinding love of honor and, more generally, the vain, confident, noble stance toward life lead to war and insecurity, these ultimately

¹⁴ See Lenathan, rv, 209–10; De Cive, Epistle Deducatory, 90; 1.7, 2.3, 2.18, 3.9; Hobbes 1975, 577, n. 5.

¹⁵ See Levathan, xv, 210-1; xxvii, 341-2; De Cive 3.12. See also Gert 1996, 163, 165, 169-70; Johnston 1986, 50-1; Kraynak 1990, 197-200; Tuck 1996a, xxviii—xxx; 1996b, 188.

¹⁶ See Levathan, xv, 215-6; xvm, 232-3; xxiv, 297; xxxvi, 468-9, xl, 500-1; xlii, 550-1, 591; xlv, 670-1; xlvi, 691, 703; De Cive 2.1-2, 3.19. Hobbes admits that his claim that all men are equal is based primarily on the goal of peace rather than on the truth. Compare Levathan, xin, 183-4, with xv, 211 and De Cive 3.13.

¹⁷ Leviathan, xiv, 195-8, esp. 196. Consider xvii, 224-5; xviii, 232-3; Manxfield 1971, 101-3.

¹⁸ On the importance of enlightenment for Hobbes, see Blits 1989, 426-9; Johnston 1986, 91-2, 101, 120-33, 213; Kraynak 1990, esp. 112-4; Trainor 1985, esp. 356-7.

¹⁹ On this point, see Manent 1977, 55, 81; Ryan 1996, 219-22 But consider also Kavka 1983, 309.

lead to enlightenment and salvation. Hobbes maintains that "continuall feare, and danger of violent death" is a most valuable learning experience. With a gun at your head, you suddenly see clearly what counts and what does not. In a flash, you grasp the essential human situation. You see the folly of vainly disdaining mere life in favor of fame after death (De Homine 12.8).20 You see that all speech about Right and Wrong, Justice and Injustice, and especially about Finis ultimus and Summum Bonum, to be found most notably "in the Books of the old Morall Philosophers," is just talk, just "uselesse" cant (Leviathan, xiii, 188; xi, 160-1; xxxi, 407-8). And you see that the one real thing in human life—our only star and compass—is the evil of death, "the chiefest of natural evils," "the terrible enemy of nature" (De Cive 1.7; Hobbes 1889c, De Corpore Politico I.1.6).21 The state of war is for Hobbes truly a "most violent master" or teacher; by inflicting violence upon us, it teaches us to fear death most of all, and such eye-opening and mind-clearing fear is the beginning and the end of human wisdom (see Hobbes 1975, 3.82). The experience of the "calamity of a warre with every other man," precisely insofar as it "is the greatest evill that can happen in this life," "instruct[s]" human beings on the necessity of establishing a state aimed at avoiding such war at all costs (Leviathan, xxx, 376; xviii, 236-7). The state of war proves to be a blessing as well as a curse, a blessing because it is a curse. That "which is worst of all" about it is the "continualle feare, and danger of violent death." But the intense and overwhelming fear of death overpowers the vainglorious passion of humans, inclines them to peace, and leads them to consent to, and thereby embrace, the sovereign (xiii, 186, 188; xx, 251-2; xlii, 599).22 Such fear of death is the rational and the saving passion of humankind, for it makes human beings reasonable, sober, and peaceable.23 Death is the chiefest of natural evils, but the fear of death turns out to be the chiefest of natural goods. It is that passion upon which Hobbes reckons, and builds, to attach human beings to the artificial state, the Leviathan, so that they might thereby escape once and for all from the natural state of war (compare Leviathan, xiv 200 with xx, 260-1 and xxix, 363).

To be sure, Hobbes does not believe that the state should appeal exclusively to its subjects' fear of death in order to save them from anarchy. "The Passions that encline men to Peace" also include the "desire of such things as are necessary for commodious living" and "a Hope by their industry to obtain them" (Leviathan, xiii, 188). Accordingly, when arguing that the end for which the sovereign is entrusted with his power is "procuration of the safety of the people," Hobbes explains that "by safety here is not meant a bare Preservation, but also all other Contentments of life, which every man by

 20 As Manent (1977, 87) puts it: "Seule la peur de la mort delivre Narcame de ses songes."

lawful Industry, without danger, or hurt to the Commonwealth, shall acquire to himself" (xxx, 376, emphasis in original; see xxiv). Hobbes clearly encourages the sovereign to appeal to the desire for comfortable self-preservation of his subjects, but he also clearly argues that the sovereign should appeal primarily to their fear of death. "Of things held in propriety, those that are dearest to a man are his own life, & limbs; and in the next degree, (in most men,) those that concern conjugall affection; and after them riches and the means of living" (xxx, 382-3, emphasis added). Consequently, "the Passion to be reckoned upon, is Feare" of that which is "worst of all," namely, "violent death" (xiv, 200; xiii, 186; see xii, 169; xiv, 202; xvii, 223; xxvii, 343).

Hobbes's account of human nature might seem to imply that the state of war can never be escaped once and for all but will always alternate with periods of peace. It seems there is no natural order to the human soul, that neither fear of death nor vanity naturally rule, but each predominates or gives way depending on external circumstances. If the terrible experience of war strengthens our fears, weakens our vanity, and leads us to embrace an artificial state aimed at peace at all costs, then will not the comfortable experience of peace inevitably lead us to forget the horrors of anarchy, inflate our vanity, and eventually undermine the state and return us to the state of war?

Hobbes suggests that human history has been marked by cyclical war and peace but he reveals how this cycle can be broken.

If there had not first been an opinion received of the greatest part of England, that these Powers were divided between the King, and the Lords, and the House of Commons, the people had never been divided, and fallen into this Civill Warre . . . which have so instructed men in this point of Soveraign Right, that there be few now (in England,) that do not see, that these Rights are inseparable, and will be so generally acknowledged, at the next return of Peace; and so continue, till their miseries are forgotten; and no longer, except the vulgar be better taught than they have hetherto been For all men are by nature provided of notable multiplying glasses, (that is, their Passions and Self-Love,) through which, every little payment appeareth a great grievance; but are destitute of those prospective glasses, (namely Morall and Civill Science,) to see a farre off the miseries that hang over them. and cannot without such payments be avoyded (Leviathan xviii, 236-8; see xxvi, 320; xlvi, 699, emphases in original).

Hobbes indicates that the seemingly inevitable cycle between war and peace, vanity and fear, can be overcome through an instruction that artificially gives people who are at peace the lessons they would "naturally" learn from the bitter and bloody experience of war. An education enlightened by "Morall and Civill Science" can correct the natural tendency of human beings who are at peace to succumb to vanity by equipping them with the foresight—"those prospective glasses"—to see and to feel the terrible consequences of such vanity—namely, anarchy and war—without having to experience them directly. The purpose of enlightenment is to make human beings who are secure feel insecure, so that they may properly appreciate their security and

²¹ Hereafter cited as De Corpore Politico, all references are to the Molesworth edition (Hobbes 1889c).

²² See Johnston 1986, 45-6; Manent 1977, 81; Manafield 1971, 100-1.

²³ As Oakeshott (1960, xxxvi) states, "man is a creature civilized by the fear of death."

thereby continue to be secure.²⁴ "To regain a good is better than not to have lost it. For it is more rightly esteemed because of the memory of evil" (*De Homine* 11.14).

The price of objective security is subjective insecurity. The more insecure you feel, the more secure you will be. If you "enjoy" your security, then you will take it for granted, you will hope and strive for goods greater than mere security—such as honor—and eventually will lose your security. But if you feel insecure, if you feel in your bones "the continualle feare, and danger of violent death," then you will continually cherish peace and the state that provides it. "Men profit more by looking on adverse events, than on prosperity" and "men's miseries do better instruct, than their good success" (Hobbes 1975, 20). Such events and miseries teach us sober fear rather than lofty, vain hope.

Since "those imbued with no matter what opinions from boyhood retain them even in old age," and since the majority of people receive their moral opinions from the learned class, the education Hobbes proposes as the solution to the problem of anarchy must focus "wholly, on the right teaching of youth in the Universities" (De Homine 13.3; Leviathan, xxx, 384; see 376-85 as a whole). This will entail broad and deep university reform, because it will "root out" books, such as those "by all the philosophers, Plato, Aristotle, Cicero, Seneca, Plutarch," that encourage vanity, rebellion, and anarchy; in their place will be books, such as works by Hobbes and perhaps Thucydides, that promote rational fear, obedience to the state, and hence peace.25 Such an education will impose an artificial order on the naturally chaotic human soul by enabling the rational fear of death to triumph once and for all over the vain love of honor. It will thereby enable the artificial state to triumph once and for all over the natural state of war.

One may still object that a certain fear could undermine the state whose goal is peace at any price and whose premise is that death is the greatest evil: the fear of eternal damnation (compare Leviathan, xv, 200, with xxix, 371; see Johnston 1986, 98–101, 112–3). Hobbes acknowledges that "it is impossible a Commonwealth should stand, where any other than the Soveraign, hath a power of giving greater rewards than Life; and of inflicting greater punishments, than Death" and it is therefore "a thing worthy to be well considered, of all men that desire (by obeying Authority) to avoid the calamities of Confusion, and Civill war, what is meant in holy Scripture, by Life Eternall, and Torment Eternall" (Leviathan, xxxviii, 478, emphases in original). He also admits that "no man can serve two masters: nor is he less, but rather more a master, whom we believe we are to obey for fear of damnation, than he whom we obey for fear of temporal death" (De Cive 6.11). Here, too, however, Hobbes finds a solution to the problem

of anarchy and rebellion (in this case, in the name of God and the afterlife) in education.

Hobbes contends that ignorance is the primary cause of that "Feare of things invisible [that] is the naturall Seed" of religion. He proposes to remove that cause by enlightening the people, by teaching what he claims to be the truth about God and the afterlife: "The will of God is not known save through the state"; the head of state, or sovereign, alone properly determines what constitutes "Sinne"; the sovereign alone is "God's Prophet" and the interpreter of Scripture; most important, the soul is not immortal.26 At the same time, Hobbes proposes to weaken the irrational fear of divine punishment after death by enhancing, again through education, the rational fear of death, especially violent death. Insofar as education reproduces the experience of the state of nature, we will feel "the continualle feare, and danger of violent death," we will grasp the truth that death is the greatest evil, and we will worry about the afterlife as little as human beings do in the state of nature.27 In this way, Hobbes hopes that enlightenment will arm the minds and hearts of human beings against those religious beliefs and passions that threaten the state and, therewith, the peace.

Hobbes's belief that the threat of anarchy can be overcome once and for all is based, then, on his contention that the experience of insecurity inspires in humans a rational fear of death, weakens the vanity and love of honor that can lead them to despise death, and teaches them to support the state whose primary aim is to avoid anarchy at all costs. If education can artificially reproduce for human beings who are at peace the effects and lessons that ensue from the natural state of war, then it should be possible for humans always to support the state and avoid civil war. In this way, the state, that "Artificiall Man," can enjoy "an Artificiall Eternity of Life," and through it human beings can obtain that "perpetuall . . . security" they seek (Leviathan, xix, 247–8; see xxi, 272; xxix, 363; De Cive, Epistle Dedicatory 91).

SOME QUESTIONS CONCERNING HOBBES'S SOLUTION TO THE PROBLEM OF ANARCHY

A number of features of Hobbes's account of human nature and anarchy may reasonably lead one to wonder about his belief that the state is the definitive solution to the problem of anarchy. In the first place, it seems that the security provided by the state can only be "perpetuall" as long as the members of the state recognize that the threat of anarchy is perpetual. If the fear of anarchy fades, then vain hopes and longings for honor will burst forth, attachment to the state will be

²⁴ On this point see also Johnston 1986, 92-3, 101, 120-33, 213. ²⁵ De Cive 13.9, 12.3; see also 12.12; De Homine 13.7; Leviathan, 1, 87; xxi, 267-8; xxix, 369-70; xxxi, 407-8; xivi, 685-703; and Review and Conclusion, 727-8.

²⁶ Leviathan, xi, 167-8 (see xii; xxx; De Homine 14.13); De Homine 15.3 (see 14.4); Leviathan, xxvii, 337-8 (compare with xiii, 187-8; xv, 216); xxxvi, 467-9; xl, esp. 502-5 (see xxvi, 330-4; xxxli-xxxlii; xlii, 604-5; xliii); xxxvlli; xliv, 636-57; xlvi, 691-2; xxxlv, 428 (but consider xv, 206).

²⁷ See Lenathan, xiii, esp. 186; xv, 200. But consider xi, 168; xil, 169-70. Consider also Kraynak 1990, 109-11, 152, 165-6; Manent 1977, 86; Strauss 1953, 198-200.

undermined, and people eventually will return to the miseries of anarchy. But precisely insofar as the state is successful in rescuing its members from those miseries, will not their fears of anarchy inevitably fade, notwithstanding the efforts of Hobbesian educators to keep those fears strong? Will not students of Hobbes's works be especially tempted to believe that they have progressed irreversibly beyond the danger of savage civil war, given his promises of "immortal peace" and an "everlasting" Commonwealth?²⁸

One may also wonder, on the basis of Hobbes's account of human nature, whether the fundamental longing of human beings will or can ever be satisfied by the Hobbesian state. That state is based on the natural human desire for security. But humans flee the state of nature not only to avoid the danger of violent death but also to be free of the fear of violent death. The feature of the state of nature "which is worst of all" is the "continual feare, and danger of violent death" (Leviathan, xiii, 186). Tuck (1996a, xliii; see xxvi) characterizes Hobbes's political philosophy as "the grand Hobbesian enterprise of liberating men from terror." Hobbes maintains that humans turn to the state in order to escape their natural anxiety, in order to feel secure as well as to be secure (see Leviathan, xiv, esp. 192). Precisely because they long to be free from fear, and hence from the very possibility of evil and death, humans long not only to preserve themselves but to do so "for ever" (xi, 160-1). Humans establish the state and subject themselves to it "for their perpetuall, and not temporary, security" (xix, 248; see De Cive 1.15).

Yet, insofar as humans long for a complete security, insofar as they long not only to avoid violent death but also to be free of the fear of death, they clearly cannot satisfy that longing while living under the Hobbesian state. The state aims to liberate its members from fear but is itself based primarily on fear. "The Passion to be reckoned upon, is Fear," and fear "is the onely thing, (when there is appearance of profit, or pleasure by breaking the Lawes,) that makes men keep them" (Leviathan, xiv, 200; xxvii, 343). Leviathan, "that Mortall God," must inspire in its members a terrible fear of punishment (xvii, 227-8; see De Cive 5.8; Johnston 1986, 92–3). But furthermore, and more important, the state must continually inspire in its members an enlightening memory and fear of the instructive miseries of the state of nature so they will never forget that no good is greater than peace, that anarchy is worse even than tyranny, and that death is the greatest evil; hence they will always obey the state.²⁹ As observed above, the price of being secure is feeling insecure. But insofar as humans long for security in its complete sense, both objective and subjective, how can they be satisfied with a state that requires them to feel continuously inse-

We may pursue this question one step farther.

Hobbes suggests that the deepest desire of human beings—that is, of beings burdened by the awareness of their mortality and hence "gnawed on by the fear of death"—is to be free of the fear of death, and hence to enjoy a "perpetuall... security." That would seem to lead to the conclusion that human beings can never be "contented" under any political order or, indeed, in any circumstance in this life but only, if at all, in an afterlife (Leviathan, xii, 169; xix, 248; xvii, 223). "There is no such thing as perpetuall Tranquillity of mind, while we live here; because Life itself is but Motion, and can never be without Desire, nor without Feare, nor without Sense" (vii, 129-30, emphasis added; see Hobbes 1889b, Human Nature. 7.5-6). He goes so far as to state in De Homine that "the greatest good, or as it is called, felicity and the final end, cannot be attained in the present life" (11.15; see Leviathan, xi, 160).

Indeed, Hobbes might appear to echo St. Augustine's argument that, precisely because human beings long for a complete and perpetual security, we cannot be happy in this life, we can only be happy if there is an afterlife, and so we must long with all our hearts for such an eternal life.

What human being is now able to live as he wishes when life itself is not in his power? Though he wants to live, he is compelled to die . . . Look at a man living as he wishes because he tortured and commanded himself not to wish what he cannot have and to wish only what he can. As Terence says, "Because you cannot do what you want, want what you can do." Is a person like this happy because he is patiently miserable? . . . Moreover, if the happy life is loved as much as it deserves to be . . . then he who loves it in this way cannot but wish it to be eternal. Therefore, life will be happy only when it is eternal (Augustine 1994, 108 [City of God XIV, 25]).

Yet, Hobbes does not call on human beings to devote themselves to the hope of attaining a perpetual security in an afterlife. Instead, he denies that hope and calls on human beings to devote themselves to a state that will provide them with as much security as is possible in this life. Hobbes urges humans to moderate their natural desire for a complete and perpetual security, to be sober in their pursuit of such security, and hence to be satisfied with the incomplete, temporary security that Leviathan, which is after all only a mortal god, can provide (see Leviathan xviii, 238-9). But, even by Hobbes's own account, are humans sufficiently reasonable to be content with a security that is mixed with and even based on the insecurity they seek to flee? Will not their natural desire for complete and perpetual security lead them to long and hope for such security and hence look beyond and even rebel against the secular Hobbesian state?

Perhaps it is because the positive desire for complete and perpetual security threatens to point beyond the state that Hobbes emphasizes more the negative counterpart to that desire: the fear of death. The positive desire for perfect security, like any other positive desire, tempts us to yearn for an unseen, mysterious, and actually nonexistent *Finis ultimus* and *Summum Bonum*, for a good the state cannot provide (*Leviathan*, xi, 160-1). The fear of death focuses our mind not only

²⁸ De Cive, Epistle Dedicatory, 91; Leviathan, xxx, 378. See also, for example, Leviathan, xix, 247−8; xx, 260−1; xxxi, 407−8; Review and Conclusion, 727−8 Consider as well Strauss 1952, 104−7.

²⁹ See Hobbes 1975, 20; *Lewathan*, xv, 215-6, xviii, 236-9; xx, 260-1; xxvi, 320; xivi, 699; *De Cive* 1.7.

on the Summum malum, "the greatest of all evils," "the terrible enemy of nature," but also on that evil from which the state can protect us (De Homine 11.6; De Corpore Politico I.1.6; see De Cive 1.7). The "Feare of Death," not the desire for self-preservation, is the primary passion that "encline[s] men to Peace," and therefore inclines men to establish and obey the state. Indeed, the "Obligation of Subjects to the Soveraign, is understood to last as long, and no longer, than the power lasteth, by which he is able to protect them," it would seem, from death (Leviathan, xxi, 272; see xxix, 375-6).

But does the fear of death simply incline human beings to obey and support the state? It cannot for the following reason: Even the mightiest of Leviathans cannot protect human beings from death. "The terrible enemy of nature" is an ineradicable, essential part of human nature. The very best the state can do is protect us from one form of death, violent death, in order to give us the security "of living out the time, which Nature ordinarily alloweth men to live" (Leviathan, xiv, 190; see xiii, 186; De Cive Preface, 103-4). Insofar as the fear of death leads us to become aware of our inevitable mortality, it would seem to lead to the conclusion that it is unreasonable that human beings should devote their life primarily to avoiding death. It cannot be reasonable to devote one's life to avoiding the unavoidable.31

The recognition that death is an unconquerable part of nature would seem to lead human beings to hope and strive to live on, in some fashion, after they die, perhaps through posthumous fame, or the salvation of the soul, or the contemplation of eternal truths (see De Homine 12.8, 14.12; De Cive 16.1; Leviathan, xi). It would seem to lead them to regard a life devoted to avoiding death, described by Hobbes as "a perpetuall and restlesse desire of Power after power, that ceaseth onely in Death," as a foolish attempt to escape the inescapable (Leviathan, xi, 161; see viii, 139; x, 150). It would seem to lead human beings to the conclusion that the Hobbesian state cannot reasonably command or deserve their complete obedience or loyalty since it cannot live up to its promise to provide them with a "perpetuall" security or protect them from death, but can at best protect them only from violent death (xix, 248; xx, 252; xxi, 272). Indeed, the recognition that death cannot be avoided would seem not only to call into question the reasonableness of the human being who aims above all to avoid death but also to vindicate, in some small measure, the reasonableness of the

³⁰ Leviathan, xiii, 188. See also xiv, 200; xx, 251-2; xxvii, 343 Compare Strauss 1952, 15-8. human being who hopes and strives to live on after death through, for example, a posthumous fame or an afterlife. For such human beings, however unreasonable the hopes they may entertain concerning death, at least face the fact that they must die.

Precisely because the full awareness of the inevitability of death would weaken human beings' attachment to the state and hence to peace itself Hobbes seeks to obscure or even to suppress this truth. For Hobbes, the primary end of philosophy itself is not knowledge or truth but "power," above all the power to avoid the "calamity of a warre with every other man, (which is the greatest evill that can happen in this life)."32 Hobbes rivets his readers' attention on "violent death," on that form of death from which the state can protect us and can inflict upon us, and thereby he seeks to inspire in us the fear of that form of death we can avoid if only we obey the state (Leviathan, xiii, 186). The fear of death as such points beyond the state to the awareness of our mortality and ultimately to heroic or religious hopes or philosophic resignation, but the fear of violent death points to our immediate need for protection and hence to the state.

At the same time, Hobbes blurs the distinction between violent death, which is avoidable, and death as such, which is not. For example, in De Corpore Politico (I.1.6), he calls death "the terrible enemy of nature," as though it were not essential to our nature. In De Cive (1.7), he states that "every man . . . shuns what is evil, but chiefly the chiefest of natural evils, which is death," as though it were possible ultimately to shun death. In chapter 13 of Leviathan he first identifies the "continuall feare, and danger of violent death" as the passion that all feel in the state of nature and then goes on to identify "Feare of Death" as the primary passion that inclines humans to peace (186, 188, emphasis added). Hobbes encourages us not to think about-or, as he might say, to dwell uselessly upon—the fact of our mortality; we must rather act as though death were avoidable and even believe, or at least feel, that death as such is avoidable (Leviathan, xxxi, 407-8). He encourages us to focus on avoiding death in the here and now rather than on seeking to live on after death. That focus will strengthen our attachment to the state and save us from the miseries of anarchy (xiv, 190; see also, e.g., xiii, 188).33

Paradoxically, insofar as reason entails foresight, insofar as it resembles "those prospective glasses, (namely Morall and Civill Science,)" that enable human beings "to see a farre off the miseries that hang over them," reason itself ultimately threatens to undermine the state and the peace (*Leviathan*, xviii, 239; see *De Homine* 12.1). Our rational capacity to "see farre off" must lead us to realize that ultimately we will die, that because the state cannot ultimately protect us from death, we must look beyond the state whose goal

³¹ But consider Hobbes's apparent defense of the reasonableness of devoting oneself to avoiding death in *De Corpore Politico* L1.6: "And forasmuch as necessity of nature maketh men to will and desire bonum sibs, that which is good for themselves, and to avoid [emphasis added] what is hurtful; but most of all, the terrible enemy of nature, death, from whom we expect both the less of all power, and also the greatest of bodily pains in the losing; it is not against reason, that a man doth all he can to preserve his own body and limbs both from death and pain." See Johnston 1986, 35–6; Kraynak 1990, 197–8. Kateb (1989, 373) goes so far as to claim that Hobbes "says next to nothing as to why staying alive is the highest good."

³² De Corpore 1.6-7; Leviathan, XXI, 376; xlvi, 682. See Leviathan, XV, 211 (cf. xiii, 183-4), 215-6; xviii, 232-3; XXVi, 322-3; XXXi, 407-8; De Crve, Preface, 102-4; 21-2; 3.19; 3.31; Johnston 1986, 56; Ryan 1996, 211

³³ For somewhat different accounts of this point, consider Kraynak 1990, 109-10; Manent 1977, 78-84

is peace at all costs. As Hobbes himself points out, the security and contentment of human beings under the Hobbesian state are based on a certain dimming of our foresight, a certain curbing of our reason, and hence on a certain unreason. "That man which looks too far before him, in the care of future time, hath his heart all the day long, gnawed on by feare of death, poverty, or calamity; and has no repose, nor pause of his anxiety, but in sleep" (Leviathan, xii, 169). But the man who does not look "too far before him," who does not think about the death that surely awaits him and hence about his mortal nature, is in a sense already asleep. By suggesting that human beings should suppress awareness of their mortality, Hobbes suggests that a certain sleep of reason, or science, or philosophy is necessary for human "repose."

To clarify this point, it is helpful to contrast Hobbes with Socrates, who also uses the image of wakefulness and sleep. Plato's Socrates suggests in the Apology (29a1-b7, 34b6-35a7) and the Phaedo (64a4-6) that the fundamental difference between philosophers and other humans is their posture toward death. Genuine philosophers practice nothing but dying and being dead; they do nothing but contemplate their mortal nature and urge others to do the same. Socrates compares them to wakeful and awakening gadflies. Nonphilosophers tend to be oblivious to their inevitable mortality. Socrates compares them to a sleeping horse who is angered when the gadfly awakens him. For Socrates, the wakeful, philosophic life is the best way of life for a human being, and the sleepy, unphilosophic, unexamined life is not worth living.34 Not only is it possible for human beings to face and be reconciled to the truth, even and especially the harsh truth about their mortality, but also such human beings lead the best and happiest life. Hobbes, however, suggests that the wakeful, rational awareness of the truth of our mortality leads to anxiety without "pause" and eventually to religious longings and hopes that will, in turn, unleash the miseries of war.35

Apparently because philosophy reminds us of the truth of our mortality and thereby weakens our attachment to the state, Hobbes generally attacks philosophy, attacks the "Leasure" that is "the mother of *Philosophy*," and praises the unphilosophic life, namely, the life of work. He argues that the question central to political philosophy, the question of what is the best regime, should not even be raised. "The present [regime] ought *alwaies* to be preferred, maintained, and accounted best; because it is against both the Law of Nature, and the Divine positive Law, to doe *anything* tending to the subversion thereof" (*Leviathan*, xlii, 577,

Plato 1977a, Phaedo 64a4-6; Apology of Socrates 30e1-31a7, 38a1-b1. See Plato 1976, Laws 808b3-c2, 1978, Republic 514a1-16e2.
 See Leviathan, xii, 168-9; xxxvi, 469; xxxviii, 478-9; De Cive 6.11, 12.5, 18.14; De Homine 13.6.

emphases added). He agues as well that "disobedience may lawfully be punished in them, that against the Laws teach even true Philosophy" (Leviathan, xlvi, 703, emphasis added). To be sure, in the Author's Epistle to the Reader of De Corpore (Hobbes 1889a),³⁷ Hobbes praises philosophy as a divine activity: "Imitate the creation: if you will be a philosopher in good earnest, let your reason move upon the deep of your own cogitations and experience: those things that lie in confusion must be set asunder, distinguished and every one stamped with its own name and set in order; that is to say, your method must resemble that of creation" [see also De Corpore 1.7].

Yet, even here, by comparing the philosopher to the creative, biblical God, Hobbes suggests that the proper goal of philosophy is not to understand and face the world as it truly is—by reflecting, for example, on the truth of our mortality—but, rather, to impose order on the chaotic natural world by acquiring and projecting power, much as Leviathan, "that Mortall God," imposes order on chaotic human nature (Leviathan, xvii, 227). Hobbes goes on to point out: "The end or scope of philosophy is that we may make use to our benefit of effects formerly seen . . . for the commodity of human life The end of knowledge is power" (De Corpore 1.6, emphasis in original). Similarly, it seems, partly because reason leads us to foresee our inevitable death and look beyond the state, Hobbes suggests that irrational creatures are more peaceful and contented than we are. In a certain sense, reason, and therefore our very humanity, is a curse, an enemy of our peace and contentment (Leviathan, xvii, 226; xii, 169; De Cive 5.5). The goal of the modern, Hobbesian state, the goal of peace and security, ultimately requires us to suppress not only our religious hopes and longings but also, to some extent, our very reason. The state that is based on our fear of violent death is based on a forgetting, not only of eternity but also of mortality.38

Hobbes's solution to the problem of anarchy requires a mean between unreason and reason, a mean, that is, between a defective and an excessive foresight. Humans who are at peace must look far enough into the future to see that, without the absolute power of the state, they will suffer the miseries of civil war, among "which is worst of all, continuall feare, and danger of violent death." But they must not look so far into their individual future that they recognize death is inevitable and the state cannot save them from it, lest such a recognition inspire religious passions and hopes that undermine the state. This mean will be achieved by giving humans who are at peace an education which reproduces for them the instructive experience of the state of war. Only if human beings feel truly insecure will they truly appreciate the security the state provides (see De Homine 11.14). And only if human beings fear violent death above all will they view the state as their supreme protector. Hobbes's fundamental hope is that

³⁶ Leviathan, xiv., 683 (emphasis in original); De Homme 11.11. For Hobbes's attacks on philosophy, consider Leviathan, iv, 105–6, v, 113–4; xv, 215–6; xxi, 267–8; xxvi, 322–3; xxix, 369–70; xxxi, 407–8; xivi, 683–703; De Cive, Epistle Dedicatory, 91–2; 1.2, 12.3–4; De Homme 14.4.

³⁷ Hereafter cited as *De Corpore*; all references at to the Molesworth edition (Hobbes 1889a).

Cf. Strauss 1953, 176; 1959, 55. Commuder as well Strauss 1952, 127 n. 2 and 107.

the direct or indirect experience of the state of war will produce in human beings a salutary fear of violent death that will enable them to escape from the state of war once and for all. But is that hope reasonable?

THUCYDIDES: THE HOPE FOR IMMORTALITY AND THE ENDURING THREAT OF ANARCHY

Thucydides offers an alternative understanding of the problem of anarchy. Although he was Hobbes's favorite classical author, unlike Hobbes, Thucydides believes the problem is essentially insoluble. "Many harsh things befell the cities on account of civil war, such as happen and will always happen so long as the nature of human beings is the same" (3.82.2, emphasis added). This emphatic statement is the clearest reference in the book to the opening claim that "as many as wish to consider clearly both the things that have happened and the things that once again will happen in the same or a similar way, in accordance with that which is human," will judge his work about the Peloponnesian War "useful" and "a possession for all time" (1.22.4). According to Bolotin (1987, 17), "the most obvious lesson of the work as a whole, for statesmen and others alike, is the sobering one that as long as our species remains, we must reckon on a human nature that will again and again, when given the chance, overpower the fragile restraints of law and justice."39 But why does Thucydides, when examining the apparently same phenomenon of civil war as Hobbes, not conclude with Hobbes that it is possible to escape from civil war once and for all by establishing a state aimed above all at peace? Why does he conclude that such an escape is

Like Hobbes, Thucydides emphasizes the ubiquity of the threat of death in his account of civil war: "Every form of death was present" (3.81.5). Yet, in striking contrast to Hobbes, he maintains that the continuous threat of violent death does not inspire in those who experience it a "continualle feare... of violent death" and a consequent rational and self-interested inclination toward peace (Leviathan, xiii, 186, 188). That threat inspires instead such warlike passions as violent and savage anger, fierce partisanship, and an overpowering desire to commit acts of vengeance, and these passions in turn deepen the disintegration of political society and increase the threat of violent death.

It is especially puzzling that the civil wars do not, according to Thucydides, provoke in those who live through them a self-interested desire for peace, since he suggests that the immediate cause of conflict was the self-interested desire for gain and honor, along with the effect of the war between Athens and Sparta on domestic politics within the Greek cities. After describing the horrors of the wars, Thucydides explains that "the cause of all these things was the desire to rule, on account of acquisitiveness and the love of honor, from which arose the zeal of those who have been plunged into the love of victory" (3.82.8; see 82.1–2). It

would appear, however, that no one's self-interest is ultimately served by the death, destruction, and collapse of society entailed by the civil war.

Even though self-interested passions are the immediate cause of the civil wars, and certain violent acts during them, such as the preemptive attacks by "those of meaner judgment" against their more resourceful opponents, may be motivated by the self-interested desire for security as well as by partisan passion (3.83.3; see 82.6-8), once those wars begin the violence seems to take on a life of its own and the "zeal" (3.82.8) which arises generally seems to overwhelm calculations of interest. Euben (1990, 187) claims that "men have gone mad" in the civil wars. Orwin (1988, 835, 845; see 1994, 181) characterizes the conflict as "a frenzied struggle to exceed one's rival at excess itself" and the men in it as "swept away by the torrent of violence...to prefer vengeance upon their fellow citizens to their own safety." I argue, however, that although Thucydides' account suggests that people became savagely moralistic and ultimately self-destructive, his account also indicates that their behavior was not simply "mad" or senselessly selfless; rather, it reflected their half-conscious, self-interested desire to overcome the threat of death that surrounded them precisely by demonstrating their noble superiority to self-interest.

Thucydides explains that the civil wars in Corcyrea and throughout Greece led to a moral revolution. The new moral code that emerged elevated "courage," or rather the "uncalculating" willingness to risk and even sacrifice one's life, over moderation and prudence (3.82.4) and elevated loyalty to one's party over loyalty to family, the laws of the city, the divine law, and concern for oneself (82.4-6). Far from inspiring a general fear of violent death, the civil war inspired an especially powerful admiration for the willingness to suffer death. Human beings came to celebrate the passionate man who dares to defy death and despises calculations of safety and self-interest as a truly courageous and "manly man" and scorned the prudent concern for life and limb as specious cowardice, "unmanly," and timorous (82.4-5). The ubiquity of the threat of death in the civil wars led humans to praise most emphatically the man who seems to be free from and superior to the desire for self-preservation and to condemn most sharply the one who seems to be enslaved to the fear of death.

But people did not simply praise human beings who were suicidal. Those who embodied the new moral virtues had to be willing not only to die but also to do so for the sake of a just cause, namely, their political party. They had to display greater devotion to the party than to their kin, the laws of their city, or their oaths to the gods, or even to themselves. They had to be willing to dare all and sacrifice all for the sake of the party (3.82.6). Thucydides emphasizes that these partisans did not genuinely care about justice or the common good (see esp. 81.4, 82.8, 84). Nevertheless, inasmuch as party is defined, ostensibly, by devotion to a specific kind of regime—be it oligarchic or democratic—and hence to a specific understanding of justice, these fanatically self-righteous partisans, who were willing

See also Connor 1984, 103-5; Euben 1990, 186; Orwin 1988, 833.

not only to kill but also to be killed for the sake of their party, evidently exhibited, in their own view of themselves, a noble and selfless devotion to justice (consider 3.70, 81.5, 82.4-8).

These two elements of the new morality—the willingness to sacrifice one's life and the zealous devotion to what one believes is "justice"—are brought together most vividly in what seems to be the overriding passion of the participants in the civil wars, namely, their passion for committing acts of vengeance. Again, Thucydides stresses how much injustice was committed as a result of this vengefulness (see esp. 3.84). Most of the cruelty and savagery of the civil wars seems to have been caused by the desire for revenge (see 3.81, 82.3, 7-8, 84; 4.47.3-48; Johnson 1993, 42). Nevertheless, it is important to keep in mind that the passion for vengeance is, from the viewpoint of the one who seeks vengeance, a passion for justice, since it necessarily entails seeking to punish what is thought to be a previous injustice. Earlier in the book, Sthenelaidas emphasizes that the Spartans should seek vengeance against Athens because of its unjust attacks on their allies (1.86). Similarly, Cleon calls for vengeance against the rebellious Mytilineans because of the tremendous injustice they committed against Athens (3.38.1, 39, 40.4-8).⁴⁰ Furthermore, Thucydides indicates that, during the civil war, the desire to commit acts of vengeance eclipsed human beings' attachment to physical well-being and even to life itself. "To avenge oneself against someone was valued more than never to have suffered [injustice] oneself" (3.82.7). He appears to suggest that human beings were willing and even eager to sacrifice their very life to avenge what they believed to be injustice and consequently that they exhibited, according to their own self-righteous selfunderstanding, a zealous, uncalculating, selfless, and hence noble devotion to what they believed to be justice.41

Yet, precisely by observing that "to avenge oneself against someone was valued more than never to have suffered [injustice] oneself," Thucydides indicates that people were not genuinely motivated by anger at injustice or a selfless concern for justice (3.82.7).⁴² He

*O See Connor (1984, 82-89, esp. 85) on "Cleon's justice of revenge"; Orwin 1994, 143-8, esp. 147.

indicates unmistakably that the desire for vengeance was not provoked by first suffering an injustice or, indeed, any harm. The desire to avenge oneself was not motivated by righteous indignation at all. Orwin (1994, 179) contends that during the civil wars, "in a strange but familiar paradox, men become heedless of their lives in their very rage at those who threaten these." But according to Thucydides, people did not truly rage at those upon whom they sought to inflict vengeance. They preferred to suffer injustice and then take revenge than not suffer injustice at all. They believed it was better to be wronged and to avenge that wrong than never to be wronged at all. They welcomed and presumably sought being victims of injustice as a pretext to inflict vengeance. Their apparently zealous. noble, and selfless desire to serve justice by punishing injustice was a pose or a facade that masked—not only from others but also from themselves—their true, self-interested motive. What was that motive? How was it served by affirming to others and themselves that they were so devoted to justice?

Thucydides suggests that the continuous threat of violent death, rather than sobering and frightening human beings, as Hobbes maintains, actually imbues them with a passionate desire to persuade others and themselves that they are superior to such self-interested concerns as the fear of death. War is a violent teacher not because it teaches a rational fear of violent death but rather because it teaches humans a violent, angry, seemingly selfless but actually self-interested passion for justice (3.82.2). But, especially in view of Hobbes's account, it is difficult to understand why the terrible experience of anarchy, in which "every form of death was present," does not lead human beings to cling to dear life at all costs and eventually establish a state aimed at securing peace at all costs.

Thucydides' account suggests that people did not cling to dear life during the conflict precisely because of the ubiquity of death. The terrible insecurity of civil war led them to sense in an especially powerful way their own human fragility and mortality. It made them realize that security for human beings is "hopeless," so they despaired of avoiding the unavoidable, at least by their own efforts alone (3.83.1-3). The war forced them to face "necessity," above all the necessity of death (3.82.2), but they did not calmly resign themselves to it. On the contrary, it aroused in them a violent anger, not toward any specific injustice, but with their overall predicament (3.82.2; see 3.84). Human beings responded to the "necessity" of death with anger, but their anger shows that they did not truly recognize or accept that necessity. Their awareness of the overpowering threat of death provoked in them an overpowering desire to overcome that threat. Their awareness of their mortality awakened or intensified in them a longing to escape the ills attendant upon their mortal nature and hence a longing for immortality. Moreover, their anger suggests that they actually cherished a hope of somehow overcoming their mortal nature. Anger at

not pay sufficient attention to the calculating, self-interested, and hopeful character of this passion for vengeance.

⁴¹ I cannot altogether agree with Connor's (1984, 99) description of the civil war as "moral anarchy" in which "the only principle is the calculation of self-interest" (see also Forde 1989, 142; Slomp 1990, 577-8). It seems to me that Connor underestimates the fanatical "zeal" with which the participants in the civil war engaged in what he calls "Revolutionary Newspeak" (101; cf Orwin 1988, 834-5), as well as the moralistic character of their partisan zeal and passion for vengeance noted by Thucydides (3.82); hence, Connor does not clarify sufficiently the nature of their self-interested hopes and longings. Also, although I agree with Cogan's (1981, 64, 149-55) emphasis on what he calls the "ideological" beliefs of the participants, it seems to me he underestimates the self-regarding hopes underlying their moralism.

⁴² Cogan (1981, 149-54), Connor (1984, 95-105, 244-5), Pouncey (1980, esp., 33, 35, 147), and Slomp (1990, 577-8) do not pay sufficient attention either to this remarkable and crucial statement or to the crucial importance of vengeance in the civil wars. Even the valuable discussions of this statement by Euben (1990, 187-8), Johnson (1993, 42-3), and Orwin (1988, 837, 845, 1994, 179, 183) do

an evil seems to presuppose the hope that the evil can be overcome.⁴³ Indeed, even though Thucydides states that people believed security was "hopeless," he immediately adds that people took precautions so as "not to suffer," which indicates they somehow hoped to secure themselves against suffering (3.83.1–3).

What was the focus of the hope to escape or overcome death? What is the relation between that hope and the desire to affirm to others and themselves that they are superior to such narrowly self-interested desires as self-preservation and are selflessly devoted to justice? According to Thucydides, these human beings in the midst of civil war instinctively pinned their hopes on the only beings who could possibly protect them from "every form of death," namely, the deathless or immortal gods, and they sought to strengthen those hopes by reassuring themselves that they are noble, just, and hence deserve divine favor.

Thucydides points most clearly to the hope for divine favor during the Corcyrean civil war when he observes: "To avenge oneself against someone was valued more than never to have suffered [injustice] oneself" (3.82.7). What is especially remarkable about this claim is that people preferred to suffer injustice and then take revenge rather than not suffer injustice at all. On the one hand, this preference is bewildering; those who suffer injustice are harmed, even mortally, whereas those who do not suffer are not.44 On the other hand, victims of injustice who then punish the wrong-doers may believe they have more reason to hope for divine assistance than those who do not suffer injustice. The experience of injustice can inspire the hope and even the confidence that the gods will assist you, since the gods are supposed to assist those who are wronged (see 5.104, 7.77.1-4 and Aristotle Rhetoric 2.5.20-22). Furthermore, by punishing a wrong-doer, you perform an apparently just deed, affirm your own justice, and presumably render yourself worthy of divine favor (see, e.g., 1.86.5, 1.118.3; cf. 7.18). By stating that people preferred to suffer injustice and then avenge it rather than not suffer injustice at all, Thucydides indicates that people were motivated not by a genuine anger at injustice but by an overriding desire to win divine assistance; they somehow expected to save themselves from death by clearly and emphatically suffering and punishing injustice.

Thucydides makes it clear that the participants in the civil war violated sacred oaths as well as divine law and so were not pious in any formal sense (3.82.6-8, 84). In their eagerness to display their courage, loyalty to party, and passion for vengeance, they clearly defied the letter of religion. Nevertheless, by indicating that

their moralism was not sincere—was not motivated by righteous indignation—but instead was histrionic and self-interested, Thucydides suggests they cherished in their heart an implicitly religious hope for immortality. Their instinctive reaction to the death surrounding them was to seek to affirm, through their courage, loyalty to party, and passion for vengeance, that they were morally superior to the self-interested concern for life and so were somehow deserving of a fate better than death. Their response to the seemingly invincible threat of mortality was to demonstrate to one and all their noble superiority to cowardice, weakness, and fear, and therewith their worthiness of immortal rewards. They acted on the implicit assumption that there are immortal beings who will somehow recognize their nobility and moral virtue and who will somehow enable them to overcome the threat of death. Although their moralism is not explicitly religious, it is guided by an implicitly religious hope for immortality.

The hope for immortality, as Thucydides presents it, does not simply consist of the hope for eternal happiness for the soul, as it does in the Christianity familiar to Hobbes. The hope for happiness in the hereafter was a part of Greek piety and is evident in Thucydides' work from the widespread concern for the recovery and proper burial of corpses.45 But the hope for immortality, as Thucydides portrays it, takes at least two other forms. One is the hope of living on through a city or nation that is protected by the gods forever, which, for example, the Melians express when they insist that the gods who have protected their city for seven hundred years will continue to do so indefinitely (5.112; see 2.36.1-2). The other is the hope of living on in the memory of others by winning the divine reward of eternal fame, which Pericles and the Athenians cherished (see 2.41-3, 64, 6.31-2). Thucydides suggests that at the core of the hope for immortality is the hope that one can somehow overcome one's mortal nature and live on after death by affirming one's justice, nobility, or piety and thereby winning the favor of the immortal gods. It is this hope which Hobbes believes can be checked by the fear of violent death, and it is this hope which Thucydides believes is stronger than and indeed inflamed by the threat of violent death.

The civil wars are, to Thucydides, a microcosm of the war between Sparta and Athens. The danger of violent death was broadly felt by all the participants over the course of the conflict. Throughout his account of the war, Thucydides suggests that the threat and awareness of death do not dampen but rather inflame the hope for immortality. In a general way, the hope to satisfy one's greatest desires cannot be mastered by the threat of even capital punishment. "Human beings are wont, when they desire a thing, to give in to unreflecting hope but to deny with imperious reasoning what they do not care for" (4.108.4). Similarly, Diodotus responds to

⁴³ See 3.45.4 Consider the efforts of Diodottus to calm the Athenians' anger at the Mytilineans who rebelled by arguing that they were compelled by their nature to do so (3.45; see 3.36, esp. 36.2; 3.39–40). Hobbes (De Homine 12.4) notes, "If, when one is pressed or assaulted by evil, a sudden hope is conceived that the evil may be overcome by opposition and resistance, the passion ariseth that is called anyor."

⁴⁴ As Johnson (1993, 42) puts it, "it is not in one's true interest to be reckless or to risk one's life to kill a rival." But Thucydides suggests one might think otherwise if one thinks it is just to do so and believes in gods who reward the just with immortality.

⁴⁵ Consider, for example, Homer 1957, Odyssey 4.561-69, 11.601-3, 6.172-4; Sophocles 1979, Antigone 74-7, 450-70, 897-902; Plato 1978, Republic 330d1-331b7; 1977a, Apology of Socrates 39e1-41c7; 1977b, Crito 54b2-c8; 1977c, Phaedo 63c4-7, 72d6-e2; Thucydides 1963, 4.42-4, 97-9, 7.75.1-4; Xenophon 1968, Hellenica 1.6.24-7.35; and Fustel de Coulanges 1900, 11-2.

Cleon's argument that the threat of violent death will stifle humans' destabilizing hopes with the thesis that "hope and passionate desire are upon everyone, desire leading, hope following, desire contriving the plan, hope supposing the bounty of fortune, both together do the most harm, and being invisible, they are stronger than the terrible things seen" (3.45.5–6).

The Athenian ambassadors at Melos shed light on that thesis by suggesting that humans' "invisible" hope may be so powerful because humans cherish the hope that the invisible gods will satisfy their greatest longings: "Do not resemble the many, who, when they may still be saved by human means, once they are pressed and the manifest grounds for hope abandon them, betake themselves to the invisible ones, to divination and oracles and such things which, together with hopes, cause ruin" (5.103). The Athenians lament that, when pressed by the threat of destruction, as the Melians are, humans are especially inclined to hope that the gods will help them overcome that threat. And the Melians do end up affirming that the gods, who have protected Melos for seven hundred years, will continue to do so (5.112).

Thucydides suggests more specifically, through the case of Pericles and the Athenians, that the awareness of death inflames the longing and hope to live on after death through the attainment of immortal glory. In his last speech, which immediately follows the deadly plague at Athens, Pericles declares that "all things by nature...decline" (2.64.3). With these philosophic words—and also with his general silence about the gods (see 2.13.5)—he seems to call on the Athenians to eschew whatever hopes for immortality they may cherish and to reconcile themselves to their mortal nature. Yet, in the same breath and indeed throughout his speeches, Pericles argues, as do the Athenian envoys at Sparta and Melos, that the Athenians are so noble, so generous and superior to calculations of profit or safety, that their city deserves an "eternal" fame (2.64.3–6; see 2.36, 39, 40.1, 4–5, 41–3; 1.76.3–77, 5.91, 105.3, 107, 111.1-2). Pericles seems to respond to the philosophic thought that all things by nature perish by affirming his hope that nature can somehow be overcome and the Athenians can somehow win the immortal glory they deserve.

This hope seems to be, albeit implicitly and half-consciously, a pious hope. By claiming that the Athenians deserve the reward of eternal glory, Pericles expresses the belief or hope that they will get what they deserve and hence that the world is such that human beings get what they deserve. He must therefore believe or hope that there are gods who ensure rewards in accordance with desert. Furthermore, by claiming that the Athenians deserve in particular the reward of "eternal" glory but at the same time claiming that all things "by nature" decline, Pericles must believe—albeit implicitly and semiconsciously—that there are supernatural and eternal beings who have it in their power to confer eternal rewards on mere mortals. 46 His

awareness of human mortality does not lead him to cling to dear life at all costs or to reconcile himself to his mortal nature; rather he embraces the implicitly pious hope for immortal glory. Indeed, so little does Pericles accept human mortality that, with one exception, he doggedly avoids referring directly to death or the dead in his Funeral Oration (2.43.6; see Orwin 1994, 19; Strauss 1964, 229).

The Athenians as a whole seem to respond to the death and destruction the war inflicts on them by looking more and more explicitly to the gods to bestow on them the reward of immortal glory. Throughout the war they betray an increasingly intense desire to purify the sacred island of Delos in order, it seems, to win the favor of the gods (see 1.8.1, 2.54, 3.104, 5.1, and esp. 5.32.1-2). Furthermore, the emergence of the superlatively pious Nicias as a respected and trusted leader in Athens evidently reflects a growing desire by Athenians as a whole to gain divine favor for their city (see 4.42-4, 7.50, 77, 86). Finally, on the eve of their most glorious military expedition to conquer Sicily, the Athenians zealously persecute those accused of religious crimes and attempt to purify their city of all impiety presumably to enlist the gods' help (6.27-32, 47-53, 60-1). Indeed, it is only after the Athenians initiate their campaign to purge their city of impiety that Thucydides says they had the "greatest hope" for success in Sicily (6.31.6; see 6.32.1-2). Their experience of war seems to intensify their longing for immortality and their hope that, by demonstrating their zealous piety, they can win the divine reward of immortal glory.

Thucydides highlights most clearly the relation among the threat of violent death, moral self-assertion. and religious hope through the example of the Melians. In their effort to subject Melos to imperial rule, the Athenian ambassadors attempt to achieve a peaceful surrender by repeated and brutal threats of complete destruction at the hands of the overwhelmingly superior Athenian forces (see 5.87, 89, 91-2, 101, 103). This strategy backfires. Rather than instill in the Melians a rational fear of violent death, the threats inspire in them what they insist is a reasonable hope that the gods will lead them to victory. "For we are pious men standing against men who are unjust" (5.104; see 1.86.5). In the face of violent death the Melians dig in their heels and affirm their own justice, by word and by deed, in the hope that the gods will save their city from destruction, as they have done for seven hundred years (5.112).47 The participants in the civil wars, moved by a similar hope, respond to the threat of violent death by affirming their own nobility and justice by word and by deed (see 5.85–7, 91–3, 101–4, 112, and 3.81.5–82).

According to both Thucydides and Hobbes, humans respond to the continuous threat of violent death by seeking to escape it. But Hobbes contends they will seek to escape through purely human means, by establishing and supporting a state aimed at peace at all

⁴⁶ Consider Strauss (1964, 229): "There is something reminding of religion in Athenian imperialism."

⁴⁷ Consider especially Bolotin 1987, 19, 28; see also Connor 1984, 149–50; Orwin 1994, 110. For accounts that argue for—and overstate—the reasonableness of the Athenians' speeches but not their deeds at Melos, consider Cogan 1981, 89–90; Palmer 1992, 73.

costs, whereas Thucydides contends they will seek to escape by somehow overcoming their mortal condition, by living on after their death—either through their city, or through their glory, or in an afterlife—and by winning the gods' favor through the vehement affirmation of their own nobility, or piety, or justice. Both Hobbes and Thucydides identify the longing for immortality as the deepest human desire. They both recognize that it cannot be satisfied by the state or by any merely human institution (if it can be satisfied at all), that it points beyond the realm of politics, and that it therefore threatens to undermine political life. Hobbes maintains this desire can be tamed either by the sobering experience of civil war or by an education that reproduces that experience. The primary effect of that experience is to convince human beings that violent death is the summum malum, the avoidance of violent death is the greatest good we can reasonably hope to attain, and the state which protects us from such death deserves our absolute obedience and support. Because Hobbes believes the fear of violent death can control the longing for immortality, he believes the state can provide mortal human beings with an "immortal peace." Thucydides, in contrast, suggests not only that the longing for immortality is more powerful than the fear of violent death but also that the continuous threat of violent death, by deepening our sense of our mortality, intensifies our longing and therewith our hope for immortality.48

CONCLUSION

At the dawn of the century there seems to be increasing dissatisfaction with the modern secular state, and it is tempting to conclude that Hobbes's hope that the Leviathan might constitute the definitive solution to the problem of anarchy is less reasonable than Thucydides' belief that this problem can never be solved. This conclusion might seem especially dispiriting because Thucydides seems to hold a harsher and bleaker view of human nature than does Hobbes (see esp. Pouncey 1980, 42-4, 156-7). Whereas Hobbes believes the

experience of civil war inclines humans toward peace, Thucydides believes it renders most of them violent and savage. Hobbes offers the hope that anarchy can be removed once and for all and an immortal peace attained; Thucydides claims that anarchy is ineradicable because it is rooted in unconquerable human nature. Hobbes views the fear of violent death as, actually or potentially, the most powerful human passion and as a check on the destabilizing longing for immortality; Thucydides considers that longing invincible, and hence considers anarchy an enduring feature of political life.

It must be stressed, however, that Thucydides believes the problem of anarchy can be effectively addressed to a considerable extent. He would not advocate a universal version of the thesis so sharply attacked by Russell Hardin (1995, 177-8) that the civil wars in the former Yugoslavia are "merely predictable history replaying itself through the horrid psychology of singularly wretched people" rather than "failures of political structures."49 In the first place, inasmuch as the longing for immortality is the most powerful human passion, Thucydides suggests that the most stable regimes are those, such as Sparta, which cultivate a political or civil religion.⁵⁰ As Orwin points out (1994, 183), "the passage on stasis reveals . . . better than any other in the work, how deeply mindful is Thucydides of the benefits of Spartan sobriety. He notes that stasis convulsed, 'so to speak,' all of Hellas; in fact it engulfed Athens but not Sparta" (cf. 1.18.1 with 2.65.11-2, 6.53-61, 8.47-98). Thucydides might argue that regimes which appeal to pious hopes through a unifying civil religion rather than appeal primarily to the individual desire for security will be less vulnerable to the threat of anarchy. A modern version of such an argument is found in Abraham Lincoln's (1989a, 28-36, esp. 30-3) "Address to the Young Men's Lyceum of Springfield, Illinois" on "The Perpetuation of Our Political Institutions"; he argued (in 1838) for establishment of a "political religion" in order to address the growing threat of anarchy in the United States.51

But even such regimes as Sparta do not constitute a definitive or "everlasting" solution to the problem of anarchy, of the kind for which Hobbes hopes (*Leviathan*, xxx, 378), since the pious hopes and longings to which such regimes appeal necessarily point beyond the regime and therefore always threaten to undermine it. More generally, Thucydides suggests that extraordi-

⁴⁸ One might object that, m the case of the plague, the threat of death leads human beings, according to Thucydides, to immerse themselves in mmediate, bodily pleasures and abandon moral and pious concerns (see esp. 2.52.3-53; consider Orwin 1988, 842-3). Yet, Thucydides refers to those "who made pretentions to virtue" during the plague and suggests that their visits to the dying, which only served to spread the plague and hence caused "the most destruction," were motivated by a desire to display their own virtue, perhaps to the gods as well as to other humans (2.51.4-5). Furthermore, he stresses that the Athenians believed the plague had been foretold by an oracle and had been sent by the gods to punish Athens in particular, which quite clearly implies that the plague may have made them, in some ways, more rather than less pious (2.54; see 53.4; consider Palmer 1992, 31). Finally, the plague inspired a desire for vengeance As Orwin (1988, 843) notes, the Athenians "go so far as to avenge themselves on Perikles for the plague by fining and temporarily deposing him"; more important, they respond vengefully to the Mytilinean revolt, which occurs during the plague (2.59, 65.1-3, 3.3.1, 13.3, 16.1, 36-40, 87; 104). The account by Thucydides of the plague's effects on the Athenians lends support to the thesis that, in his view, the threat of death intensifies the pious longing for mmortality, as well as the desire to win drvine favor through the vehement affirmation of one's own justice.

⁴⁹ Consider as a whole Hardin 1995, 142-82 and 215-31. See also Hassner 1995, 7-20, 335-54.

⁵⁰ For a fuller account, see Ahrensdorf 1997, 251–61. See also Euben 1990, 190; Strauss 1964, 146–7. For the Spartans' piety, see 1.86.5, 103.1–2, 118.3, 126–7, 134, 2.74–2–3, 5.16–7, 7.18. But for Thucydides' criticusms of Sparta, see 2.2, 71–4, 3.52–68, 4.80, 8.40.2, and Connor 1984, 126–40. Johnson (1993, 28–32, 137, 212–3) does not give sufficient weight to these criticisms.

⁵¹ For Lincoln's later attempts to appeal to plous longings, consider especially his "Second Inaugural Address" (1989b, 686–7). A letter of Lincoln's (1989b, 522–7, esp. 522–3) from 1863 (on conflict in Missouri) presents what Finley (1963, 182–3) rightly calls a "remarkable parallel" to Thucydides' account of the civil war in Corcyra. On Lincoln and Thucydides, consider Finley 1963, 144–5; Orwin 1994, 26–7

nary statesmen, such as Pericles and Hermocrates, can effectively promote unity within their political communities (see 2.34-46, 2.65, 4.58-65, 6.62-3; Johnson 1993, 200). Indeed, Thucydides seeks to provide a political and moral education for his readers—a positive and negative education in wise and humane statesmanship—as is revealed, for example, by his explicit judgments of such leaders as Archidamus, Themistocles, Pericles, Cleon, Brasidas, Peisistratus, Hermocrates, Nicias, and Antiphon.⁵² But, as the fate and legacy of these leaders demonstrate, Thucydides does not believe even the greatest statesmen can achieve anything like the "immortal peace" of the Hobbesian project.53 Thucydides, like Hobbes, encourages his readers to act against inhumanity and folly in the political world as exemplified by Diodotus' efforts to save the Mytilineans from being butchered by the Athenians and to save his fellow Athenians from becoming butchers. Unlike Hobbes, Thucydides encourages his readers to do so with a somber, Diodotean appreciation of the ultimately ineradicable character of inhumanity and folly.54

In one sense Thucydides evidently holds a loftier and brighter view of the human condition than does Hobbes. Hobbes maintains that human contentment requires us to suppress our awareness of the truth that we are mortal, lest we be "gnawed on by feare of death" (Leviathan, xii, 169), whereas Thucydides suggests, most clearly through his own example, that the awareness of mortality and other harsh truths about our nature can constitute the core of a noble and happy life. As his book shows, Thucydides contemplated the horrible things to which our mortal nature exposes us, relentlessly but humanely, with true compassion for his fellow mortals. His account of the plague reveals that Thucydides faced death squarely, without flinching but also without bitterness.55 He composed a book that he judged worthy of immortality—a possession for all time—without any hope that it would survive for all time. Thucydides foresaw with perfect clarity but also with perfect equanimity the future destruction of Athens and Sparta, of his entire world, of all that he cared about, apart from the truth.⁵⁶ Through his own example, he unobtrusively but unmistakably bears witness to a possibility, and therewith a hope, to which Hobbes never clearly refers or for which he never even allows:

52 Sec 1.89, 1.138.3, 1.139.4, 2.65, 3.36.6, 4.81, 6.54.5-7, 6.72, 7.86,

the possibility of combining a full, unshrinking awareness of the truth of our mortality with a genuine serenity of spirit and hence the possibility of becoming genuinely reconciled to our mortal, human nature.57

⁵⁷ I disagree with Pouncey's (1980, 150; see xiii, 42-4) conclusion that Thucydides' "intelligence and his strong concentration on the events as they happen have shown him that neither Pericles' ideals for Athens nor Niclas' personal virtue were equal to the demands made by the war itself. The real coldness of Thucydides' unfinished work is that it falls alent without telling us whether he finally found anything to take their place."

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⁵³ Compare, for example, 2.34-46 with 47-54 and 65.

⁵⁴ See 3.36, 41-9. See especially 3.45 and compare with, for example, 4.108.4 as well as 3.82-4, esp. 3.82.2. Consider as well the character and career of Demosthenes within the book

⁵⁵ See esp. 247-54, above all 48.3, 51.6. See as well 3.70-84 (cf. 1.22.4 with 3.82.2); 1.23.1-4 See Aubrey's (1975, 156) reference to Hobbes's "extraordinary Timorousness." But consider as well Warrender's (1983, 8) remarks: "Hobbes always represented himself as a timid man. His verse autobiography relates how his mother gave birth to twins, himself and Fear. In modern times this estimate has been taken too much at its face value. What impressed Hobbes's contemporaries was his courage." Consider also Gert 1972, 30, Trainor 1985, 362; Tuck 1993, 302, 326, 329-30, 335-44; 1996a, xliii-xlv, hi-liii.

⁵⁶ Sec 1.10.1-3 (cf. 1.8.1), see also 1.1-2. For Thucydides' devotion to the truth, see 1.20.3, 22.4.

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Equilibria in Campaign Spending Games: Theory and Data

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e present a formal game-theoretic model to explain the simultaneity problem that makes it difficult to obtain unbiased estimates of the effects of both incumbent and challenger spending in U.S. House elections. The model predicts a particular form of correlation between the expected closeness of the race and the level of spending by both candidates, which implies that the simultaneity problem should not be present in close races and should be progressively more severe in the range of safe races that are empirically observed. This is confirmed by comparing simple OLS regression of races that are expected to be close with races that are not, using House incumbent races spanning two decades.

his article presents new estimates of the effects on the vote of spending by incumbents and challengers in U.S. House elections. As first pointed out by Jacobson (1978), estimation by ordinary least squares regression produces strong coefficients for challenger spending but virtually no effect for incumbent spending. Few would argue, however, that the latter does not matter. The obvious reason for the near-zero coefficients for incumbent spending using OLS is a simultaneity bias, because incumbents spend more when they are in electoral trouble. Somewhat less obvious, and less commonly acknowledged, is that the opposite bias can be present for challengers, who spend more when their prospects are good. In short, the OLS estimates are biased because one is not controlling for candidate expectations of the vote, and these expectations drive spending decisions. We address this problem directly by formulating and solving a game-theoretic model of campaign spending and identifying precisely the finer structure of these simultaneity problems. This characterization of the equilibrium of the spending game has direct implications for obtaining unbiased estimates of incumbent and challenger spending effects, without resorting to multiequation systems.

Several statistical solutions have been proposed to estimate the effects of incumbent spending by overcoming the simultaneity bias. The most common solution is two-stage least squares, whereby instrumental variables are used as proxies for observed incumbent spending. Green and Krasno (1988) used lagged incumbent spending as an instrument, whereas Gerber (1998) used such instruments as challenger wealth and state population in an analysis of Senate elections. Both found significant effects for incumbent spending. In an alternative approach, Erikson and Palfrey (1993, 1998) achieved statistical identification by means of restrictions on the variance-covariance matrix. By assuming that the covariances between each spending

variable and the vote were caused by only vote-onspending and spending-on-vote effects, they also found significant effects for incumbent spending, somewhat larger in magnitude than those of Green and Krasno (1988) but still significantly smaller than challenger spending effects. In a third approach, Abramowitz (1991) used OLS but attempted to neutralize the simultaneity bias by using Congressional Quarterly forecasts of election outcomes as a control for expectations. Even with this control, negligible coefficients for incumbent spending were found, and Abramowitz concluded that incumbent spending has little effect on the vote. In sum, due to seemingly unavoidable methodological difficulties, a consensus has not yet emerged regarding both the relative and absolute magnitudes of incumbent and challenger spending effects on congressional election outcomes.

Our new approach to estimating spending effects uses insights from game theory. We apply OLS to a subset of congressional districts for which a gametheoretic model predicts that the simultaneity bias should be minimal or nonexistent. In these districts, new sources of challenger vote support do not necessarily drive up challenger and incumbent spending. Which districts are they? They are districts in which, before taking spending into account, the vote is expected to be close or even slightly in favor of the challenger. When a close race is expected, both spending effects can be reliably estimated by simple OLS.

We proceed as follows. First, we present the theoretical argument for a near-zero simultaneity bias in districts in which the vote is expected to be close. Second, we translate this theoretical argument into a set of statistical hypotheses and derive closed-form expressions for the asymptotic bias of OLS estimates of spending effects. Third, we provide evidence in support of the predicted curvilinear effects of the expected vote on spending. When the expected vote approximates a 50-50 split, the slopes for spending on (expected) vote are near zero, so that the simultaneity bias should be near zero. Fourth, we present the key empirical result. When the vote is expected to be close (and, thus, the simultaneity bias is minimal), the OLS estimates of spending effects are of roughly equal magnitude for incumbents and challengers. We conclude with a discussion of our findings.

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THEORY: THE SPENDING GAME

Our empirical results can be understood in the context of an extended version of the simple spending game framework of Erikson and Palfrey (1993). The model is meant to capture the strategic aspect of spending by two competing candidates, I (the incumbent) and C (the challenger).1 The spending decisions of the two candidates are intertwined because these decisions jointly affect the probability the incumbent will win. Thus, the amount one candidate chooses to spend will depend both on the amount the other candidate is spending and on the expected closeness of the race. This is the spending game.² An equilibrium of the game consists of a spending decision for each candidate with the property that neither candidate can do better by spending more or less than this amount, given the opponent's spending decision and the current electoral conditions. What we show below is the intuitive idea that the equilibrium spending levels of both candidates will increase monotonically with the expected closeness of the race.

The importance of our theoretical analysis is as follows. The formal model implies, among other things, that candidates will indeed maximize their spending when the expected vote is in the neighborhood of 50-50. As a result, for "toss-up" races, the vote-onspending effects will be approximately zero. For competitive races in the range of a close expected vote but in which incumbents are slightly favored, the vote-onspending slopes should be negative but small in magnitude. For races expected to be lopsided in favor of the incumbent, the slopes will be negative and large in magnitude, which can create possibly severe simultaneity problems. Because most races fall in the latter category, regressions that pool all races are fundamentally flawed, even if instrumental variables are used, since that method does not adjust for the fact that the simultaneity problem varies with expected closeness in a predictable and nonlinear way.

The formal structure of the game has the following basic features. Each candidate cares about the probability of winning more than 50% of the vote in the election. The probability I wins is determined by district characteristics, short-term forces, candidate characteristics, campaign spending, and chance. For the moment, treat the first three categories of variables as exogenously fixed. We then can summarize the effects of campaign spending and chance by a simple twice continuously differentiable function, P, which denotes the incumbent's probability of winning as a function of spending by each of the candidates. The probability the incumbent wins also depends on what we call the (prespending) anticipated margin of victory of the incumbent, denoted m. All else equal, the higher is m, the higher is P. The anticipated victory margin incorporates factors that the candidates both treat as exogenous, including national short-term forces and

such district characteristics as partisanship, as well as other factors, such as candidates' health, a recent scandal, and so forth. Each candidate can raise and spend money in the campaign, and the outcome of the election is a function of how much each spends, the prespending anticipated margin of victory of the incumbent, and some random noise.

Raising campaign resources is a costly activity. Promises must be made (Baron 1989a, 1989b), issue positions compromised, fundraisers attended, and so forth. This is formally represented by two twice continuously differentiable, increasing, convex fundraising cost functions, one for each candidate, denoted $K_I(I)$ and $K_C(C)$. The final piece of the equation is the value of winning. We normalize this value at 1 for both the incumbent and the challenger.³ Hence, the payoff functions to the two candidates are given by:

$$U_I(I, C; m) = P(I, C, m) - K_I(I);$$

 $U_C(I, C; m) = 1 - P(I, C, m) - K_C(C).$

We proceed by studying the properties of the Nash equilibria of this game. A Nash equilibrium is defined as a pair of spending levels for each candidate, I^* and C^* , such that each maximizes her payoff (probability of winning minus fundraising costs), given the spending level of the opponent. In particular, we wish to establish existence of a Nash equilibrium of the spending game and then prove that equilibrium spending levels will be higher in closer races.

To guarantee existence of a Nash equilibrium that is fully characterized by the first-order conditions, we assume that each payoff function is strictly concave in the candidate's own spending level. This assumption implies properties of both P and K. Roughly speaking, it says that in addition to K being convex, P cannot be too deterministic. The concavity assumption is:

$$\frac{\partial^2 P}{\partial I^2}(I, C, m) - K_I''(I) < 0;$$

$$-\frac{\partial^2 P}{\partial C^2}(I, C, m) - K_C''(C) < 0.$$

To rule out uninteresting boundary solutions, we also assume $K'_I(0) = K'_C(0) = 0$. Thus, the optimal level of spending for the incumbent, I^* , given some level of spending by C, is always unique, is always strictly positive, and is characterized by the solution to:

$$\frac{\partial P}{\partial I}(I^{\bullet}, C, m) - K'_{I}(I^{\bullet}) = 0.$$

Similarly, for the challenger, we have:

¹ The theoretical analysis also can be applied to open seat races

² Related theoretical work on tournaments as incentive devices and on the effect of commitment on behavior in contests can be found in Drut 1987, Nalebuff and Stiglitz 1983, Rosen 1986.

³ This normalization is without loss of generality, since we allow the incumbent and challenger to have different cost functions. It is also possible to generalize the cost functions. For example, the campaign fundraising cost function could be allowed to depend on m in an additively separable way.

⁴ If the game were not concave, we would have to resort to mixed equilibria.

$$-\frac{\partial P}{\partial C}(I, C^*, m) - K'_{C}(C^*) = 0.$$

Because the game is concave and reaction functions are strictly positive, there exists an equilibrium with strictly positive levels of spending by both candidates. In equilibrium, these conditions are simultaneously satisfied for both, so equilibrium pairs (I^*, C^*) are characterized by the following equations:⁵

$$\frac{\partial P}{\partial I}(I^*, C^*, m) - K_I'(I^*) = 0; \qquad (1)$$

$$-\frac{\partial P}{\partial C}(I^*, C^*, m) - K'_C(C^*) = 0.$$
 (2)

For the empirical section, we are interested in identifying how I^* and C^* change with m and how they both covary with P. We next identify intuitive assumptions on the cross-partial derivatives of P, which imply that I^* and C^* are decreasing in m in elections that favor the incumbent, increasing in m for elections that favor the challenger, and are independent of m for toss-ups. This also implies that spending covaries with P in a similar direction. Moreover, we provide a particular specification of $P(\cdot)$ that satisfies these assumptions and is consistent with the standard linear regression models used in empirical studies.

Since the first-order conditions must hold for every value of m, we can take the total derivative of the left-hand side of equations 1 and 2 with respect to m; set it equal to zero; and use this to identify conditions $dI^*/dm < 0$. As we pointed out earlier, and show formally in Appendix A, this will imply that OLS estimates of the I^* coefficient are biased downward in elections the incumbent is expected to win. This procedure also allows us to obtain sufficient conditions for $dC^*/dm < 0$, which are slightly different owing to the fact that challengers who expect incumbents to spend less will respond by spending more. Taking these total derivatives produces the following two equations:

$$\frac{dI^*}{dm} = \frac{\frac{\partial^2 P}{\partial I \partial C} \frac{dC^*}{dm} + \frac{\partial^2 P}{\partial I \partial m}}{K_I'' - \frac{\partial^2 P}{\partial I^2}};$$

$$\frac{dC^*}{dm} = -\frac{\frac{\partial^2 P}{\partial I \partial C} \frac{dI^*}{dm} + \frac{\partial^2 P}{\partial C \partial m}}{K_C'' + \frac{\partial^2 P}{\partial C^2}}.$$

These are linear in the two unknowns, dC^*/dm and dI^*/dm , and can be solved easily. The solutions are:

$$\frac{dI^*}{dm} = \frac{\frac{\partial^2 P}{\partial I \partial m} \left[K_C'' + \frac{\partial^2 P}{\partial C^2} \right] - \frac{\partial^2 P}{\partial C \partial m} \frac{\partial^2 P}{\partial I \partial C}}{A};$$

$$\frac{dC^*}{dm} = \frac{-\frac{\partial^2 P}{\partial C \partial m} \left[K_I'' - \frac{\partial^2 P}{\partial I^2} \right] - \frac{\partial^2 P}{\partial I \partial m} \frac{\partial^2 P}{\partial I \partial C}}{A};$$

where

$$A = \left[K_C'' + \frac{\partial^2 P}{\partial C^2} \right] \left[K_I'' - \frac{\partial^2 P}{\partial I^2} \right] + \left[\frac{\partial^2 P}{\partial I \partial C} \right]^2 > 0$$

by the second-order conditions. Therefore, $dI^*/dm < 0$ if and only if

$$\frac{\partial^2 P}{\partial I \partial m} \left[K_C^* + \frac{\partial^2 P}{\partial C^2} \right] - \frac{\partial^2 P}{\partial C \partial m} \frac{\partial^2 P}{\partial I \partial C} < 0, \quad (3)$$

and $dC^*/dm < 0$ if and only if

$$-\frac{\partial^2 P}{\partial C \partial m} \left[K_I'' - \frac{\partial^2 P}{\partial I^2} \right] - \frac{\partial^2 P}{\partial I \partial m} \frac{\partial^2 P}{\partial I \partial C} < 0. \tag{4}$$

We assume that inequalities 3 and 4 hold. We next show that these conditions for $dI^*/dm < 0$ and $dC^*/dm < 0$ are satisfied in a wide class of models of P that are commonly used in empirical work.

First, let the index of incumbent vote margin, m, be simply the baseline (prespending) expected incumbent share of the vote minus .5, normalized at candidate spending levels of I = C = 0. Let the incumbent's actual margin of victory, M (which can be negative), equal m, plus a function (not necessarily linear) of incumbent spending, $g_I(I)$, plus some function of challenger spending, $g_C(C)$, plus a random term, ε , with cumulative distribution function $F(\varepsilon)$. Assume $g_I'(I) > 0$, $g_C'(C) < 0$; F is twice differentiable; and its density function, f = F', is symmetric and single peaked around 0.7 This implies $f'(\varepsilon) > 0$ for $\varepsilon < 0$, and $f'(\varepsilon) < 0$ for $\varepsilon > 0$. Thus, we can write

$$M = m + g_{r}(I) + g_{c}(C) + \varepsilon$$
.

The incumbent wins if and only if M > 0 or if and only if $\varepsilon > -m - g_I(I) - g_C(C)$, so

$$P(I, C, m) = 1 - F[-m - g_I(I) - g_C(C)]$$

= $F[m + g_I(I) + g_C(C)].$ (5)

It is easy to verify that inequalities 3 and 4 for $dI^*/dm < 0$ and for $dC^*/dm < 0$ are satisfied when f' < 0 for many P functions. As an illustration, suppose that F is normally distributed with variance σ^2 and that g_I and g_C are linear.⁸ Thus, equation 5 reduces to:

$$P(I, C, m) = \Phi \left[\frac{m + \beta_{VI}I + \beta_{VC}C}{\sigma} \right],$$

⁵ There may be multiple components of the equilibrium correspondence, but the comparative static properties we derive below are satisfied locally with respect to each equilibrium.

⁶ Moreover, equilibrium is unique for this specification, which guarantees unambiguous comparative statics.

⁷ The symmetry assumption can be dropped without loss of generality.

This corresponds to most empirical works on the subject, which employ regression models with linear spending effects.

where Φ is the unit normal cumulative distribution function. Substitution into equilibrium conditions 1 and 2 yields the following two equations as the first-order conditions:

$$\frac{\beta_{II}}{\sigma\sqrt{2\pi}}\,\varphi(\Pi_m^*)=K_I'(I^*);\tag{6}$$

$$\frac{-\beta_{VC}}{\sigma\sqrt{2\pi}}\,\varphi(\Pi_m^*) = K_C'(C^*);\tag{7}$$

where $\varphi(\Pi_m^*)$ is the unit normal density function evaluated at the expected win margin, adjusted for equilibrium spending effects and incumbent vulnerability, $\Pi_m^* = (m + \beta_{VL}I^* + \beta_{VC}C^*)/\sigma$. The expression Π_m^* is the "postspending" expected vote margin for the incumbent, in contrast to m, which is the prespending expected vote margin for the incumbent. Thus, it is important to keep in mind that Π_m^* is endogenous, whereas m is exogenous.

It follows immediately from equations 6 and 7 that elections expected to be close (measured by Π_m^*) will involve higher spending levels by both candidates. This is true because φ is maximized at $\Pi_m^* = 0$, and fundraising costs increase with spending.

We also wish to show that spending is higher in closer elections, measured by m. Because m indexes incumbent vote margin rather than expected closeness, 10 the comparative statics with respect to m will necessarily depend on whether we are at an equilibrium with $\Pi_m^* > 0$ or $\Pi_m^* < 0$. Suppose that $\Pi_m^* > 0$. Because spending effects enter linearly and the normal density function, φ , is monotonically decreasing in the absolute value of its argument, inequalities 3 and 4 derived above are both satisfied. Formally:

$$\begin{split} &\frac{\partial^2 P}{\partial I \partial m} = \frac{\beta_{\mathit{II}}}{\sigma^2 \sqrt{2\pi}} \, \phi'(\Pi_{\mathit{m}}^*) < 0; \\ &\frac{\partial^2 P}{\partial C^2} = \frac{\beta_{\mathit{IC}}^2}{\sigma^2 \sqrt{2\pi}} \, \phi'(\Pi_{\mathit{m}}^*) < 0; \\ &\frac{\partial^2 P}{\partial C \partial m} = \frac{\beta_{\mathit{IC}}}{\sigma^2 \sqrt{2\pi}} \, \phi'(\Pi_{\mathit{m}}^*) > 0; \\ &\frac{\partial^2 P}{\partial I \partial C} = \frac{\beta_{\mathit{II}} \beta_{\mathit{IC}}}{\sigma^2 \sqrt{2\pi}} \, \phi'(\Pi_{\mathit{m}}^*) > 0; \text{ and} \\ &\frac{\partial^2 P}{\partial I^2} = \frac{\beta_{\mathit{II}}^2}{\sigma^2 \sqrt{2\pi}} \, \phi'(\Pi_{\mathit{m}}^*) < 0. \end{split}$$

Therefore,

$$\frac{\partial^{2}P}{\partial I\partial m}K_{C}^{"}<0;$$

$$-\frac{\partial^{2}P}{\partial C\partial m}K_{I}^{"}<0;$$

and

$$\begin{split} \frac{\partial^2 P}{\partial C \partial m} \, \frac{\partial^2 P}{\partial I^2} - \frac{\partial^2 P}{\partial I \partial m} \, \frac{\partial^2 P}{\partial I \partial C} &= \frac{\beta_{\mathcal{VC}} \beta_{\mathcal{VI}}^2}{\sigma^4 \sqrt{2\pi}} \, \phi'(\Pi_{\textit{m}}^{*}) \\ &- \frac{\beta_{\mathcal{VI}} (\beta_{\mathcal{VI}} \beta_{\mathcal{C}})}{\sigma^4 \sqrt{2\pi}} \, \phi'(\Pi_{\textit{m}}^{*}) = 0; \\ \frac{\partial^2 P}{\partial I \partial m} \, \frac{\partial^2 P}{\partial C^2} - \frac{\partial^2 P}{\partial C \partial m} \, \frac{\partial^2 P}{\partial I \partial C} &= \frac{\beta_{\mathcal{VI}} \beta_{\mathcal{VC}}^2}{\sigma^4 \sqrt{2\pi}} \, \phi'(\Pi_{\textit{m}}^{*}) \\ &- \frac{\beta_{\mathcal{VC}} (\beta_{\mathcal{VI}} \beta_{\mathcal{C}})}{\sigma^4 \sqrt{2\pi}} \, \phi'(\Pi_{\textit{m}}^{*}) = 0, \end{split}$$

which, from inequalities 3 and 4, implies that both parties spend more in elections that are expected to be close. That is, $dI^*/dm < 0$ and $dC^*/dm < 0$. Also notice that this specification of P has the nice property that if spending effects and marginal fundraising costs are symmetric for incumbents and challengers, then Π_m^* is exactly equal to m. Even if these effects are not exactly symmetric for the two candidates, continuity implies that Π_m^* will closely track m, so the prespending index of incumbent strength, m, is also an equilibrium postspending index of incumbent expected vote margin. In the closest possible race, in which $\Pi_m^* = 0$, φ attains its maximum, so equilibrium spending is maximized for both candidates. For incumbents less vulnerable than this (nearly all cases in practice), equilibrium spending by each candidate is decreasing in the (normalized) equilibrium expected incumbent share, Π_m^* .

Before proceeding, it is useful to point out that signs on the three cross-partial derivatives— $\partial^2 P/\partial C\partial m > 0$, $\partial^2 P/\partial I\partial m < 0$, and $\partial^2 P/\partial I\partial C > 0$ —have a natural interpretation. Consider the first two of these. Because the density function is single peaked at 0, the effect of spending on the probability of winning is higher the closer the election is expected to be. That is, it is highest at $\Pi_m^* = 0$. Then, for values of m such that the equilibrium probability the incumbent wins is exactly 5, the sign of both cross-partials is 0, since $\varphi'(0) = 0$. (In this case, equilibrium spending levels are locally constant in m.) For values of m higher than this, the equilibrium probability the incumbent wins is greater than 5; therefore, as m increases, all else held constant, the election will be less close, so the marginal effect of spending (proportional to $\varphi(\Pi_m^*)$) is lower, which is precisely what is meant by $\partial^2 P/\partial C \partial m > 0$ and $\partial^2 P/\partial I \partial m < 0$. The third cross-partial derivative has a similar interpretation. When $\Pi_m^* > 0$, greater spending by the challenger leads to a closer election, which makes the marginal effect of incumbent spending higher, hence $\partial^2 P/\partial I \partial C > 0.11$

A mirror image of this effect arises if $\Pi_m^* < 0$. For this rare category of races, in which incumbents are

⁹ Keep in mind that the equilibrium spending effects themselves are a function of m.

¹⁰ That is, expected closeness decreases in the absolute value of m.

 $^{^{11}}$ For values of m such that the equilibrium probability the incumbent wins is less than .5, the signs of these cross-partials are all reversed. In that case, data would exhibit the opposite feature, in which equilibrium incumbent and challenger spending would be positively correlated with the vote.

more likely to lose than to win, equilibrium spending by each candidate actually increases in the normalized equilibrium expected incumbent margin of victory, Π_m^* . Finally, if cost and spending effects are roughly the same for the incumbent and the challenger, then $\Pi_m^* \sim 0$ if and only if $m \sim 0$.

To consider specific candidate cost functions, suppose K is quadratic, so $K_I(I) = (1/2\alpha_I)I^2$ and $K_C(C) = (1/2\alpha_C)C^2$, where α_I and α_C can be interpreted as fundraising efficiency parameters for the incumbent and challenger, respectively. Substituting this into the equilibrium equations above yields

$$\frac{\alpha_I \beta_{VI}}{\sigma \sqrt{2\pi}} \varphi(\Pi_m^*) = I^*; \tag{8}$$

$$\frac{-\alpha_C \beta_{VC}}{\sigma \sqrt{2\pi}} \varphi(\Pi_m^*) = C^*. \tag{9}$$

This implies that equilibrium candidate spending should be proportional to the normal density of the expected incumbent margin of victory. It is also interesting to note another property of the equilibrium. Our assumptions guarantee that C^* and I^* are both strictly positive, so we can divide equation 8 by equation 9 to obtain

$$\frac{I^*(m)}{C^*(m)} = -\frac{\alpha_I \beta_{VI}}{\alpha_C \beta_{VC}},$$

which illustrates explicitly why there is a positive correlation between I^* and C^* in the normal-quadratic model that is so widely used for estimation.

The key is that, for districts in the range of a 50-50 expected vote, the vote-on-spending slopes are near zero; cases with mildly negative slopes balance a similar number of mildly positive slopes. Therefore, by limiting our sample to races expected to be close, there should be little concern about a simultaneity bias to distort the estimation of spending effects in the OLS estimation of the vote equation.

METHODOLOGY: SIMULTANEITY BIAS AND VARYING PARAMETERS

The following three-equation system is used to explain the methodological implications of our theoretical model. The first equation has incumbent share of the two-party vote as a dependent variable, and the other two have challenger and incumbent spending, respectively, as the dependent variable. This system was studied extensively in Erikson and Palfrey (1998), and the reader is referred there for more details.

$$V = \beta_{VC}C + \beta_{VI}I + \gamma Z + \varepsilon_{V}; \qquad (10)$$

$$C = \beta_{CV} V_C + \varepsilon_C = \beta_{CV} V + w_C; \qquad (11)$$

$$I = \beta_{IV} V_I + \varepsilon_I = \beta_{IV} V + w_I; \qquad (12)$$

where

V = incumbent percentage of the (two-party)vote;

C = the log of challenger spending (in 1978 dollars) plus 5,000;

I =the log of incumbent spending (in 1978 dollars) plus 5,000;

Z = a set of instruments to predict the incumbent vote without directly affecting spending;

 V_C , V_I the challenger and incumbent expectations of

the incumbent vote margin; $V_C = V + u_C$ and $V_I = V + u_I$ (candidates observe an imperfect signal of V); and

 $w_C = \beta_{CV} u_C + \varepsilon_C$ and $w_I = \beta_{IV} u_I + \varepsilon_I$.

We assume w_C and w_I are uncorrelated with ε_V , but we allow that w_C and w_I may be correlated with each other.

Two interesting methodological problems occur in the estimation of this three-equation system. The first and most commonly noted is simultaneity. In a nutshell, both candidates spend more when races are close. The second involves "varying parameters." That is, the coefficients on the independent variables in the two spending equations (11 and 12) vary across the sample in a systematic way. Specifically, our theoretical model establishes that the equilibrium values of β_{CV} and β_{IV} covary with expected vote.

We next derive the asymptotic bias of the coefficients in the vote equation and prove that this bias vanishes in close races. Before presenting the formal argument, we explain the intuition behind this. The results from the theory section established that both β_{CV} and β_{IV} are approximately zero when V is in the neighborhood of 50%. This is very important, because it means that incumbent and challenger spending on "close" races in this range can be treated simply as exogenous, and unbiased estimation of the vote equation can be done by OLS, which we prove formally below. Intuitively, it means that the parameters of equations 11 and 12 vanish. In contrast, for the large majority of districts used in past cross-sectional studies, V is significantly greater than 50, and β_{CV} and β_{IV} are negative in this region, with their magnitude varying with V in a nonlinear way. This implies a negative bias in the OLS estimates of β_{CV} and β_{IV} , so OLS will inflate challenger spending effects and deflate incumbent spending effects.

To derive formally the asymptotic bias of the spending-on-vote coefficients, we fix Z at some level, so that β_{IV} and β_{CV} can be treated as approximately constant. To simplify the exposition, suppose that, conditional on Z, I and C are uncorrelated. 12 Standard analysis then yields (see Appendix A for derivation):

$$\text{plim } \beta_{VI}^{OLS} = \beta_{VI} + \frac{\beta_{IV}\sigma_{ev}^2}{(1 - \beta_{IV}\beta_{VI} - \beta_{CV}\beta_{VC})\sigma_I^2} \neq \beta_{VI},$$
(13)

unless $\beta_{IV} = 0$, and

$$\text{plim } \beta_{VC}^{OLS} = \beta_{VC} + \frac{\beta_{CV}\sigma_{ev}^2}{(1 - \beta_{IV}\beta_{VI} - \beta_{CV}\beta_{VC})\sigma_C^2} \neq \beta_{VC},$$
(14)

¹² The general case is treated in Appendix A.

unless $\beta_{CV} = 0$. The second terms in equations 13 and 14 represent the asymptotic bias of the OLS estimate of the spending-on-vote coefficients, and σ^2 denotes variance.

It follows immediately that if $\beta_{IV} = \beta_{CV} = 0$, then the asymptotic bias disappears for both challenger and incumbent spending effects, and OLS estimates of β_{VI} and β_{VC} are consistent. This is indeed our main theoretical point, since the spending game model implies that $\beta_{IV} = \beta_{CV} = 0$ when the expected vote is 50-50.13 Therefore, β_{VI} and β_{VC} can be estimated without simultaneity bias by using close races. Furthermore, as we show in Appendix A, this is true even if I and C are highly correlated.

Even when races are not expected to be close, it is not difficult to sign the bias terms. It is easy to show that $(1 - \beta_{IV}\beta_{VI} - \beta_{CV}\beta_{VC}) > 0$, so the sign of the two bias terms in equations 13 and 14 are given by the signs of β_{IV} and β_{CV} , respectively. If the incumbent is expected to have a safe margin of victory, then the vote effects on spending (β_{IV} and β_{CV}) are negative (safer incumbents induce less spending), so the estimates of both spending effects on the vote are biased in the negative direction. Hence, the bias deflates the positive effect of incumbent spending on the incumbent vote and inflates the negative effect of challenger spending on the incumbent vote.¹⁴

Interestingly, the two biases are not exactly symmetrical, even if the vote-on-spending effects are equal for incumbents and challengers. The reason is that, empirically, challenger spending has more variance than incumbent spending $(\sigma_I^2 < \sigma_C^2)$, so the negative simultaneity bias deflates the incumbent spending effect to a greater extent than it inflates the challenger spending effect. One final point remains. The parameters β_{IV} and β_{CV} are only approximately constant because, for instance, β_{IV} will locally vary as V responds as a function of both w_V and ε_I , even with Z held fixed. But if the variability in β_{IV} and β_{CV} is symmetrical in the neighborhood of 50% (so that the effect of 50 + ϵ percent of the vote equals the effect of $50 - \epsilon$ percent), then nothing of consequence changes. Should this symmetry not hold, the point at which the asymptotic bias is exactly equal to 0 could be slightly greater than or slightly less than 50-50.

THE DATA

The units of our analysis are 1,792 contested House races, 1974–80 and 1984–90, involving a veteran (i.e., nonfreshman) incumbent who won a contested race from the same district in the previous election. ¹⁵ We generated a reduced-form equation whereby we predict the expected prespending incumbent vote (Z)

from a collection of exogenous variables: lagged incumbent vote; district presidential vote for the incumbent's party (1976 in the 1970s, 1988 in the 1980s); and effects for year, incumbent party, a dummy for southern states, and the relevant interaction terms. ¹⁶ Because this reduced-form equation is lengthy and tangential to our central point, the details of its construction are presented in Appendix B. It accounts for 58% of the variance in the incumbent vote. We exploit this equation to obtain an expected incumbent vote, which is not identical to the expected vote that candidates themselves observe, but we consider it a good measure of the candidates' expected vote that is uncontaminated by spending effects.

This measure of the expected vote (Z) serves as an instrumental variable for a two-stage least-squares analysis of the effects of the (expected) vote on the two spending variables. If the vote-on-spending effects were linear, then we would simply regress spending on the predicted vote. Here, our prediction is decidedly nonlinear, following our theoretical argument. Accordingly, our task is to estimate the nonlinear slope contingent on the value of the expected vote.

The Effect of the Vote on Spending

Do we obtain our prediction of a curvilinear effect of the vote on spending, that is, zero when the vote is 50-50? Figure 1 is a scatter plot of the logarithm of real challenger spending in all veteran-contested House elections between 1972 and 1990 against incumbent share of the vote.

The fit with the simple normal-quadratic model proposed above is surprisingly good. To illustrate, we have superimposed a fitted nonlinear regression line, ¹⁷ using locally weighted least squares, or LOWESS (Cleveland 1979). This has a shape that resembles the bell curve signature of a normal distribution, with a maximum near to 50% of the incumbent vote, as predicted. A similar graph in Figure 2 for incumbent spending as a function of vote shows a similar fit.

As clear as the patterns of figures 1 and 2 appear to be, technically they are not quite right. Just as we should not estimate the effects of spending on the vote without considering the simultaneity bias from the influence of anticipated vote on spending, we should not depict the (anticipated) vote-on-spending effect without considering the reverse effects of spending on the vote. Accordingly, we turn to the two-stage least-squares approach to estimate the nonlinear slopes of spending predicted from the proxy for the vote, that is, our prediction of the vote in the reduced-form equation (Z). We simply show the nonlinear lopes of spending contingent on the expected vote rather than the actual vote. This task approximates the presentation of figures 1 and 2; by using the instrumental

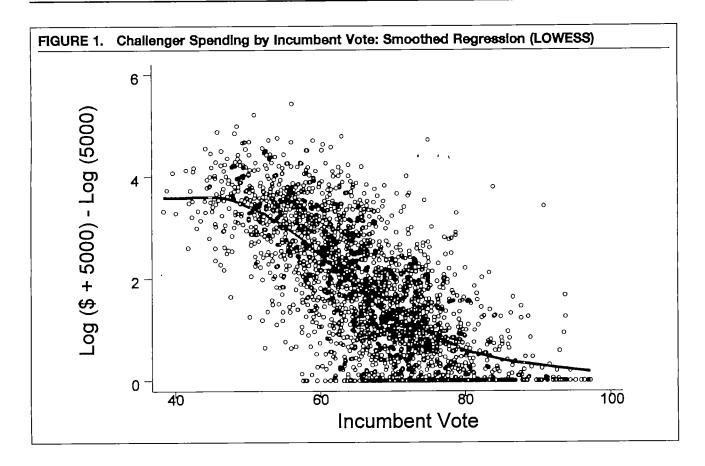
¹³ By continuity, the asymptotic blas is approximately 0 for close races in the neighborhood of 50-50.

¹⁴ Note that if the expected incumbent vote were less than 50%, then the direction of the bias would reverse, and spending effects would be inflated.

¹⁵ The 1982 races were excluded because we use lagged incumbent vote to construct Z, and the 1980 incumbent vote does not match the 1982 incumbent vote due to redistricting.

¹⁶ Year, incumbent party, and region (South/non-South) were combined in all possible interactions, in effect generating separate intercepts for $8 \times 2 \times 2 = 32$ groups, such as "nonsouthern 1974 Republicans."

¹⁷ The fit is done using a bandwidth of .20.



variable of the expected vote, we avoid any distortion due to causal feedback.

Figure 3 displays the estimated vote-on-spending effects, using Z as our proxy for the actual vote. The slopes shown are LOWESS estimates—nonlinear curves built from locally linear approximations over different bands of the expected vote. Figure 3 replicates the patterns shown in figures 1 and 2. We observe the nonlinearity in the slopes, corresponding to the kind of nonlinearity predicted by the theoretical model presented previously. For lopsided races in the 55–75% range, the spending-on-vote slopes are steeply negative. As the expected incumbent vote approaches 50%, the spending-on-vote slopes flatten considerably. We also see evidence of the predicted sign reversal when the incumbent's expected vote drops below 50%, a fate that befell 17 incumbents.

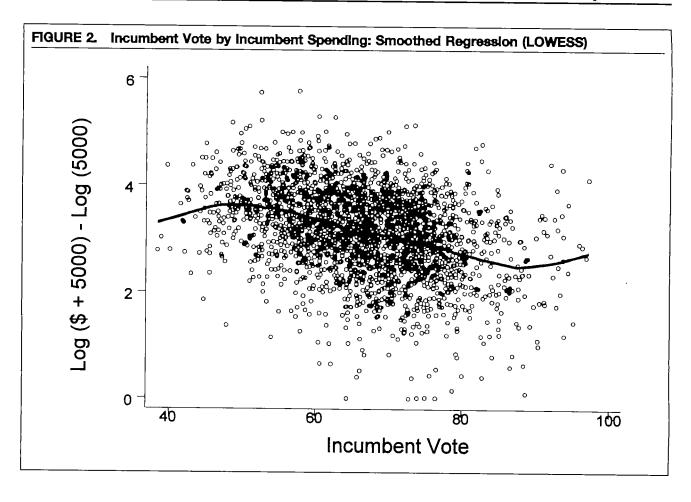
By overlaying the spending functions for incumbents and challengers in the same graph, Figure 3 highlights asymmetries between their spending equations. The slope is steeper for challengers than for incumbents. The largest spending gaps are in safe districts, which is consistent with the theoretical argument that in safer seats there is more incentive for contributors to invest in incumbents rather than challengers (Baron 1989a, 1989b; Snyder 1990). Note that in very competitive districts the gap between incumbent and challenger spending virtually disappears. Incumbents evidently are usually better able to raise money than are challengers but not due only to their incumbency. Rather, an incumbent's advantage over a challenger in raising

cash appears to increase with his likelihood of winning. To the extent that the expectation of victory induces lower costs of fundraising, the effect of spending on the vote becomes all the more crucial. A spending advantage induced by an anticipated victory further increases the advantage of likely winners if indeed candidate spending appreciably affects the vote.

For the purpose of estimating spending effects, it is less important to account for the exact spending functions than to note that the functions shown in figures 1 and 2 are maximized when the vote is 50–50. We now see that our game-theoretic model of optimal spending decisions by candidates is not merely an exercise in formal theory, it matches up with the data surprisingly well. Thus, it provides a rigorous and empirically sound theoretical foundation for what common sense has long suggested should be a basic law of campaign spending: All else equal, both candidates will spend more when the election is expected to be close.

These results clearly indicate the nature of the measurement problem in estimating the effects of challenger and incumbent spending on the vote. Since the (expected) incumbent vote in contested House elections is almost always in the range of 55–75%, simultaneity should bias OLS estimates of incumbent spending effects downward and estimates of challenger spending effects upward (more negative for the incumbent vote). Any simple one-equation model will

¹⁸ Approximately 80% of the cases (1,491 of 1,792) have an expected vote in the 55-75% range.



generally overestimate the negative effect of challenger spending and underestimate the positive effect of incumbent spending on the incumbent share of the vote.

We are now in a position to correct this situation. By isolating districts where the vote is expected to be close, we select districts where the additional shocks to the expected vote should have little effect on spending. For these districts, incumbent spending and challenger spending can be considered exogenous, which we can estimate via OLS.

The Effects of Spending on the Vote

Table 1 presents OLS equations that predict the incumbent vote from the two logged spending variables and the expected vote, with cases grouped according to expected vote. In column 1 is the equation for the 40 very close races with an expected incumbent vote below 52%. For these contests, the means for both the actual and predicted incumbent vote are virtually 50%. In theory, if the effects of incumbent and challenger spending are identical, we should find virtually identical coefficients for the two spending variables, except for their sign. This is what column 1 shows. 19 More-

over, even with only 40 cases, both spending coefficients are statistically significant.²⁰

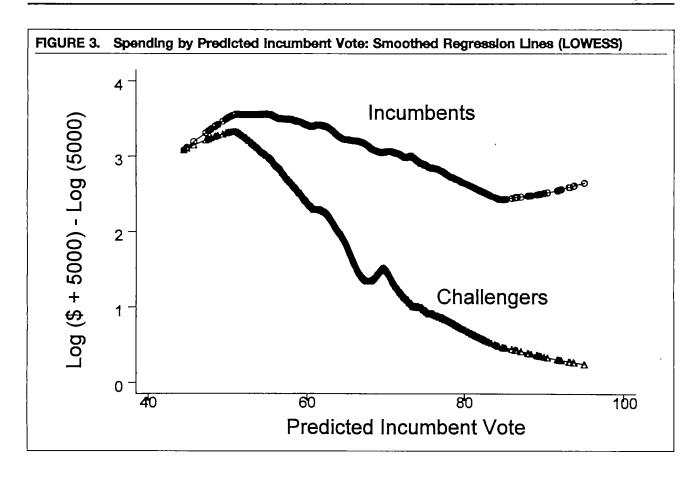
Column 2 expands the analysis to the 77 cases in which the expected incumbent vote was 52–55%. As illustrated in Figure 3, this is the beginning of the range of the expected vote at which the slopes for its effect on spending are negative. Here, the statistical bias should create only a mild diminution of the incumbent spending coefficient. Column 2 shows another statistically significant coefficient for incumbent spending, but less than that for challenger spending, as predicted.²¹

When the incumbent is expected to win by about 55% or more, the vote-on-spending slopes are steeply negative, so the bias is quite severe. As column 3 of Table 1 reveals, for districts in which the incumbent is expected to receive 55–58% of the vote, the OLS estimate of incumbent spending is negligible and decidedly nonsignificant. This pattern is repeated in further categories of three-point increments of the expected incumbent vote. Overall, when that vote exceeds 58%, the simultaneity problem is so severe that

¹⁹ Using conventional significance levels, one cannot reject the null hypothesis that these coefficients are the same.

²⁰ These results hold when candidate quality is controlled by means of a dummy variable, challenger prior office, added to the right-hand side; they also hold (with an increased standard error, as expected), when Z is replaced by its many component variables. See Appendix B for details.

²¹ The difference is significant at the p = .11 level.



the coefficient on incumbent spending actually reverses sign. The estimates are presented in column 4. The pooled results for all cases are given in column 5, which produces the well-known implausible result that challenger spending matters, but incumbent spending does not.

Our OLS estimates convincingly demonstrate that incumbent spending matters in close races. It may be tempting also to accept the OLS estimates for less competitive races, which would suggest that, unlike vulnerable incumbents, safe incumbents cannot spend effectively. It would be a mistake, however, to treat the coefficients in Table 1, columns 4 and 5, as anything short of seriously biased, given the formal results discussed earlier. Of course, there is no plausible reason why marginal incumbents (often marginal because of their own ineptitude) would spend effectively, whereas safe incumbents (coded safe in part due to their track record) would spend ineffectively.

As we move from competitive to safe seats, the OLS coefficients for incumbent spending decline so far as to be virtually zero. Meanwhile, the OLS coefficients for challenger spending show no corresponding increase. If simultaneity bias for noncompetitive races accounts for the deflated estimates for incumbent spending, why do we not see an inflation in coefficients for challenger spending? There are at least three contributing factors that can be identified.

First, for challengers and perhaps for incumbents as well, it is plausible that spending effects (votes gained

per logged dollar) decline when the seat is perceived as safe. When the outcome appears foregone, voters may pay less attention, and candidates may spend a smaller portion of their budget on actual vote-winning activities. Furthermore, the generally inexperienced challengers for safe seats may spend inefficiently. All this suggests offsetting processes that affect OLS estimates of challenger spending effects as one moves from competitive to safe seats. The bias exaggerates challenger spending effects that are getting weaker and deflates incumbent spending effects that may also be getting weaker.

A second reason OLS estimates of challenger spending effects appear insensitive to simultaneity bias may be that there actually is less such bias for challengers. Being less informed than incumbents, inexperienced challengers may estimate the expected vote with greater noise. In particular, they may be less able to gauge the unmeasured causes of the vote, such as their own vote appeal, the contribution of the previous challenger to the incumbent's previous margin, and any shift in the incumbent's attractiveness to voters.²²

A third reason, already noted, is the fundamental asymmetry of the simultaneity bias for challengers and incumbents: The vote-on-spending effects and spending-on-vote effects have opposite signs for incumbents but the same sign for challengers. For favored incum-

²² Some empirical support for this argument is reported in Erikson and Palfrey 1998.

			Spending by Expo expected incumbent		
	<52%	52–55%	55–58%	>58%	All Cases
Constant	50.55 (2.23)	16.72 (0.48)	34.21 (1.10)	54.10 (17.52)	53.92 (19.56)
Log (I)	4.04 (2.21)	3.06 (3.64)	0.8 6 (1.59)	-0.10 (-0.56)	0.07 (0.38)
Log (C)	-4.11 (-3.49)	-4.37 (-7.80)	-2.94 (-6.47)	−3.41 (−26.34)	-3.36 (-27.83)
Expected vote	0.00 (0.00)	0.95 (0.62)	0.86 (0.59)	0.73 (31.46)	0.70 (36.07)
Adj. R ²	.193	.457	.291	.675	.722
SEE	5.23	4.55	4.99	5.26	5.24
N	40	77	119	1,556	1,792

bents, the cycle is negative: New spending increases the incumbent's vote, which dampens the value of spending further. For underdog challengers, the cycle is positive: New spending increases their vote, which amplifies the value of spending further. One statistical consequence is that (everything else being equal), in equilibrium, challenger spending will have greater variance than incumbent spending, a pattern clearly observed in the data. As can be inferred from equations 13 and 14, higher spending variance attenuates the bias in estimated spending effects. See Appendix A for further details.

Putting the three parts of the explanation together, as one moves from competitive to safe districts, the OLS estimate of incumbent spending collapses due to the increased bias plus a possible decline in spending effectiveness when the seat is safe. Meanwhile, the OLS estimate of the challenger spending effect stays roughly constant, as the lesser bias may decline only modesty and is offset by the decline in spending effectiveness.²³

The CQ Expected Vote

One alternative way of measuring perceived electoral closeness deserves our attention. We supplement our measurement of electoral expectations by incorporating Congressional Quarterly's measure of the expected vote. During October of every campaign over the two decades of this analysis, CQ rated each of the 435 House races on a scale from "safe Democratic" to "safe Republican."²⁴

We fold this scale based on incumbent party affiliation to form a scale of expected vulnerability. Three categories emerge (the numbers in parentheses are usable cases): close, coded by CQ as "favoring the challenger" or "too close to call" (N=83); leaning, coded by CQ as "leaning" or "likely" for the incumbent (N=340); and safe, coded by CQ as "safe" for the incumbent (N=1,369).

A district's rating on this scale reflects not only the exogenous variables that make up our measure of the expected vote but also the more intangible sources of the current vote (challenger strength, and so on) that candidates may observe during the campaign but are not part of our equation. To the extent that CQ raters observe the same intangible sources of the vote that candidates observe when they make their spending decisions, the simultaneity problem becomes minimized.

This argument is not new (Abramowitz 1991). Following logic similar to that in the previous paragraph, Abramowitz decided to include the CQ expected vote category in OLS equations predicting the incumbent vote. Even with CQ ratings on the right-hand side of the equation, the coefficient for incumbent spending was insignificant. Abramowitz concluded that the estimates of small incumbent spending effects were not an artifact of simultaneity bias, as commonly supposed.

One limitation of CQ ratings as a control for intangible sources of the vote is that they chop the cases coarsely into only three usable categories, and 78% fall in the safe category. Obviously, different degrees of "safeness" are not distinguished. In contrast, the CQ ratings single out only a select few races as "close." Among the 83 cases so defined, there may be little variation in degree of vulnerability as observed by CQ or the candidates. In any case, CQ-coded close races are very close on average, so the simultaneity problem

²³ As side evidence for declining spending effectiveness in general as seat safety increases, consider the OLS regression of the incumbent vote on the difference between I and C for different levels of Z. Since the IC difference expands as a function of the anticipated vote margin, the positive bias in this estimate should increase with Z, the instrument. Instead, the coefficient for the IC difference declines with Z.

²⁴ The *CQ* predictions are found in *Congressional Quarterly*'s biennial election supplements. Each election year, *Congressional Quarterly* publishes a supplement of more than 100 pages, handicapping the congressional elections. The supplement is almost always published

in issue 41 (the 41st week), and (for the years of our analysis) can always be found for the CQ issue for the date ranging from October 9 to October 15.

TABLE 2. OLS Regression of incumbent Vote on Spending by CQ Categories of Competitiveness

		CQ	Forecast		
	Close	Leaning	Sefe	All Cases	
Constant	41.02 (3.59)	51.98 (7.74)	48.92 (16.26)	50.98 (18.54)	
Log (/)	2.66 (2.33)	2.24 (4.56)	−0.27 (−1.48)	0.09 (0.55)	
Log (C)	-2.31 (-1.96)	-3.84 (-8.88)	-2.64 (-18.51)	-2.80 (-20.33)	
Expected vote	0.07 (0.70)	0.38 (7.49)	0.73 (35.06)	0.66 (34.67)	
Close (CQ)				-6.01 (-9.12)	
Leaning to incumbent (CQ)				-2.27 (-5.00)	
Adj. R²	.048	.296	.643	.735	
SEE	4.80	5.20	4.92	5.12	
N	83	340	1,369	1,792	
Note: T-statistics	Note: T-statistics are given in parentheses.				

is minimal. The mean incumbent vote in the CQ-defined close races is 49.5%.

In the analysis that follows, we estimate the vote equation separately for the three categories (close, leaning, and safe districts), using as right-hand-side variables the log of spending for incumbents, the log of spending for challengers, and our measure of the expected vote. We also show the results when we pool the cases, with CQ groupings on the right-hand side as dummy variables. Table 2 displays the results. (See Appendix B for the auxiliary analysis, which gives the results with challenger prior office added to the righthand side and with Z replaced by its many component parts.) With the control for CQ ratings, the spending coefficients are slightly lower than in Table 1, because the ratings are created just a few weeks before the election and inevitably absorb some of the effects of early spending during the campaign.

The fourth column of Table 2 shows the results with all cases pooled, comparable to column 5 of Table 1 but with the addition of two CQ variables. Here, CQ categorization helps predict the vote independent of our expected-vote measure; just as Abramowitz observed, we find a nonsignificant coefficient for incumbent spending. Simply including these variables in the regression does not redress the theoretical problem that equilibrium spending varies systematically with expected closeness. Hence, we consider the first three columns, which show the regressions separately by degree of CQ "safeness."

In column 1 are the OLS results for races CQ defined as close. For this group, our expected vote measure is of little importance for predicting the

outcome. Crucially, incumbent spending is statistically significant and, for the first time among our sets of equations, larger in absolute magnitude than the coefficient for challenger spending.²⁵

This result contrasts with that given in column 3, at the other end of the safety spectrum. For the vast bulk of contests deemed safe for the incumbent, the coefficient for incumbent spending is nonsignificant, as if incumbent spending did not matter. In these races the simultaneity distortion is severe.

In races that lean to the incumbent, column 2, the coefficient for incumbent spending is of lesser magnitude than that for challenger spending.²⁶ Yet, the incumbent spending coefficient is statistically significant, even with the evident simultaneity bias.

Let us focus on the crucial set of races deemed by informed observers at the time as too close to call. These are the pivotal districts that were seen to be most in play during the actual election campaign. The incumbent spending coefficient for this set of cases is, if anything, slightly stronger than that for challengers because of a drastic reduction if not elimination of the simultaneity bias. In races that CQ rates too close to call, the simultaneity bias is minimal in the first place.

Note that the coefficients for both incumbent and challenger spending in CQ's close races are lower than in races predicted to be close by the Z index. This is expected, because the CQ rating incorporates the effects of early spending. In effect, Table 2 shows the influence of unanticipated late spending by both candidates, as measured by the proxies of incumbent and challenger spending levels over the duration of the campaign. Put another way, the coefficients for spending were muted due to CQ's anticipation of their effects. Therefore, if anything, Table 2 underestimates the magnitudes of spending effects.

CONCLUSION

This article has examined a simple theoretical model of the spending game in an election between competing candidates for public office. That model strongly suggests a nonlinear relationship between expected incumbent share of the vote and spending by both candidates. It also illuminates the nature of the simultaneity bias introduced when OLS regressions use cross-sectional data pooled across both safe and competitive districts. The key insight is that, in equilibrium, total spending is continuously increasing in the expected closeness of the race. Because total spending reaches a maximum when expected incumbent share of the vote is 50%, the slope of spending with respect to incumbent vulnerability in this range is necessarily zero, so a sample

²⁵ The difference in magnitude is not statistically significant, however. Using conventional significance levels, one cannot reject the null hypothesis that incumbent and challenger spending effects are equal.

²⁶ The null hypothesis of equal spending effects for incumbents and challengers is rejected at the .01 significance level.

that includes only close races (expected incumbent vote share in the neighborhood of 50%) will be immune from the kind of simultaneity bias that plagues OLS regression on the full sample. This facilitates a clean estimate of incumbent and challenger spending effects.

We tested the model with veteran incumbent U.S. House races from 1974 to 1990. For close races, not only does incumbent spending pass the threshold of statistical analysis but also our estimate of the size of this effect is statistically indistinguishable from the effect of challenger spending. Thus, in close races, a given amount of spending wins about as large a share of the vote for an incumbent as for a challenger. We also show clearly how the simultaneity problem grows progressively more severe as the closeness of races diminishes. Two different measures of expected closeness are employed, and both yield identical conclusions. The first combines prespending long-term partisan strength in the district, short-term national forces, and lagged incumbent vote. The second uses the preelection CQ categorization of close, incumbentleaning, and safe races.

Both the theory and empirical findings complement the results reported in Erikson and Palfrey (1998), which presents a three-equation model of spending and makes two qualitative findings: (1) Incumbent and challenger spending is an increasing function of race closeness, and (2) the estimated effects of incumbent spending decline with seniority. Point 1 is a central prediction of our theoretical model, and point 2 is consistent with the fact that junior incumbents are typically more vulnerable than senior incumbents. In our vulnerable incumbent subsample (expected vote <52), nearly half (48%) were either sophomores or juniors, compared to only slightly more than one-third (36%) in the remaining races. As for the magnitude of spending effects, the estimates for veteran races in Erikson and Palfrey (1998) (and elsewhere in the literature) are smaller in magnitude than those reported here for close races. There are two possible reasons. First, because of pooling over all races, close and safe, the estimates elsewhere could be biased, even with carefully designed methods to correct for simultaneity. We showed here, both theoretically and empirically, that the degree of bias will vary with incumbent vulnerability in predictable ways, and this is not accounted for in those methods. Second, it may be that spending effects are indeed high in a tight race, due to its saliency to the voters and extensive media coverage. This interesting possibility would require a new approach to sort out differences in spending effects as a function of incumbent vulnerability.

To summarize, the results presented here have potentially important consequences for understanding the connection between money and the incumbency advantage. Incumbents outspend challengers and, according to our analysis, achieve roughly equal effectiveness per dollar in close races, where it matters most. In popular discussions, fundraising capacity is seen as a

major source of the incumbency advantage. Political scientists have hesitated to endorse this view, in part because it is difficult to estimate spending effects. Our findings have implications too complex to explore here, but they should finally put to rest any lingering doubt about the significance (and similarity) of incumbent and challenger spending effects in close races.

APPENDIX A

Consider our three-equation system with Z fixed at some level:

$$V = \beta_{\nu\nu}I + \beta_{\nu\nu}C + \varepsilon_{\nu}; \tag{15}$$

$$I = \beta_{IV}V + w_{I}; \text{ and}$$
 (16)

$$C = \beta_{CV}V + w_C. \tag{17}$$

Since β_{IV} and β_{CV} themselves vary as a function of expected vote, we fix Z (the instrument for expected vote) in the analysis below, so β_{IV} and β_{CV} can be treated as constant.²⁷ Our interest is in the bias in the OLS estimates of β_{VI} and β_{VC} , the spending effects on vote. The analysis below derives the asymptotic bias of the OLS estimate of β_{VI} . Calculation of the asymptotic bias of β_{VC} is done in a similar way.

To start, by successive algebraic substitution, we solve the simultaneous equations for I, V, and C and obtain the following exact expression for the OLS estimate of β_{VI} . The lowercase symbols indicate sample values of V, C, and I; observations in the sample are indexed by j; and b_{IC} is the sample bivariate regression coefficient of I on C.

$$\hat{\beta}_{VI}^{OLS} = \frac{\sum_{j} (i_{j} - b_{KC}c_{j})(v - b_{VC}c_{j})}{\sum_{j} (i_{j} - b_{KC}c_{j})^{2}}$$
(18)

$$OLS = \beta_{VI} + \frac{\sum_{j} (i_{j} - b_{IC}c_{j})\epsilon_{V_{j}}}{\sum_{i} (i_{i} - b_{IC}c_{i})^{2}}.$$
 (19)

Applying the rules of probability limit gives

plim
$$\beta_{VI}^{OLS} = \beta_{VI} + \frac{cov(I - \gamma C, \varepsilon_{V})}{var(I - \gamma C)}$$
, (20)

where γ is the expected value of b_{IC} . Some more algebra yields the following expressions:

$$cov(I, \varepsilon_{\nu}) = \frac{\beta_{I\nu}\sigma_{\nu\nu}^{2}}{1 - \beta_{I\nu}\beta_{\nu I} - \beta_{C\nu}\beta_{\nu C}},$$

$$cov(C, \varepsilon_{\nu}) = \frac{\beta_{C\nu}\sigma_{\nu\nu}^{2}}{1 - \beta_{I\nu}\beta_{\nu I} - \beta_{C\nu}\beta_{\nu C}}, \text{ and}$$

$$var(I - \gamma C) = \sigma_{I}^{2} - \gamma^{2}\sigma_{C}^{2}$$

which gives

plim
$$\beta_{\nu I}^{OLS} = \beta_{\nu I} + \frac{(\beta_{I\nu} - \gamma \beta_{C\nu})\sigma_{\mathbf{p}\nu}^2}{(1 - \beta_{I\nu}\beta_{\nu I} - \beta_{C\nu}\beta_{\nu C})(\sigma_I^2 - \gamma^2\sigma_C^2)}$$

= $\beta_{\nu I} + \Delta\Phi$, (21)

where

²⁷ Recall that β_{IV} and β_{CV} vary as a function of V, so, for any fixed Z, they are only approximately constant.

$$\Delta = \frac{\sigma_{\text{ev}}^2}{(1-\beta_{\text{IV}}\beta_{\text{VI}}-\beta_{\text{CV}}\beta_{\text{VC}})(\sigma_{\text{I}}^2-\gamma^2\sigma_{\text{C}}^2)}\text{,}$$

and

$$\Phi = \beta_{IV} - \gamma \beta_{CV}.$$

So the asymptotic bias of the OLS estimate of the incumbent spending effect on vote is simply $\Delta\Phi$. Because Δ is always positive, the sign of the bias is determined by the sign of Φ , which is governed by the signs of β_{IV} , β_{CV} , and γ .

It follows immediately that if $\beta_{IV} = \beta_{CV} = 0$, then the asymptotic bias disappears. This is indeed our main theoretical point, since our model implies that $\beta_{IV} = \beta_{CV} = 0$ when the expected vote is 50-50.25 Therefore, spending effects β_{VI} and β_{VC} can be estimated consistently by restricting attention to close races. This key result also would hold in more general models if one were to introduce additional parameters to index the degree of the simultaneity problem, as in Erikson and Palfrey (1998).

We next turn to signing the bias for the remainder of races in our sample, those in which the incumbent is expected to win with a safe margin. For these races, our model shows that β_{IV} and β_{CV} are both negative. Therefore, unless γ is very large, the sign of β_{IV} will determine the sign of the asymptotic bias. If $\gamma=0$ (as discussed in the text), then the sign of the bias is determined by the sign of β_{IV} , which is negative for safe races, by the game-theoretic model. The bias could not necessarily be signed were γ very large (and positive), but empirically it turns out that γ is small, which implies the intuitive result that OLS causes a downward bias in the estimated incumbent spending effect.

By similar argument, we can obtain the following expression for the bias in challenger spending effects:

$$\mathrm{plim}~\beta_{\nu c}^{\mathit{OLS}} = \beta_{\nu c} + \frac{(\beta_{\mathit{CV}} - \delta \beta_{\mathit{IV}}) \sigma_{\mathit{sv}}^2}{(1 - \beta_{\mathit{IV}} \beta_{\mathit{II}} - \beta_{\mathit{CV}} \beta_{\mathit{VC}}) (\sigma_{\mathit{C}}^2 - \delta^2 \sigma_{\mathit{I}}^2)},$$

where δ is the expected value of b_{CI} , and the asymptotic bias is determined by the sign of $(\beta_{CV} - \delta \beta_{IV})$. As with the incumbent, if $\beta_{IV} = \beta_{CV} = 0$, then OLS estimates are consistent. Otherwise, the bias is negative (which overestimates the effect of challenger spending, since $\beta_{VC} < 0$) unless δ is very large and positive, in which case the negative bias becomes negligible or possibly reverses sign. Empirically, it turns out that δ is much larger than γ , which helps explain why the data for safe races indicate a greater problem of simultaneity bias in β_{VI}^{OLS} compared to β_{VC}^{OLS} . By definition, $\delta = (\sigma_C^2/\sigma_I^2)\gamma$, and empirically $\sigma_C^2 > \sigma_I^2$ (even with Z held constant, as we have been assuming throughout). The latter fact has implications for the numerator and denominators of both bias terms. This is evident from inspection of equations 13 and 14.

The difference in magnitude between σ_C^2 and σ_I^2 is partly driven by fine details of the simultaneous system. From equations 15–17 we can decompose σ_C^2 and σ_I^2 as follows:

$$\sigma_I^2 = \beta_{IV}^2 \sigma_V^2 + \sigma_{NC}^2 + 2\beta_{IV} \sigma_{V,NF},$$

$$\sigma_C^2 = \beta_{CV}^2 \sigma_V^2 + \sigma_{NI}^2 + 2\beta_{CV} \sigma_{V,NC}$$

Due to the opposite signs of the spending-on-vote effects, the terms σ_{V,μ_I} and σ_{V,μ_C} have opposite signs, with $\sigma_{V,\mu_I} > 0$ and $\sigma_{V,\mu_C} < 0$. As a result, $2\beta_{IV}\sigma_{V,\mu_I} < 0$ and $2\beta_{CV}\sigma_{V,\mu_C} > 0$, thus expanding σ_I^2 while contracting σ_C^2 , even if everything else is symmetric between incumbents and challengers.

APPENDIX B

Our presentation of results in the text is drawn from regressions in which the control variables are summarized in terms of the predicted vote, or Z in the terminology of our statistical discussion. The measure Z is derived from a reduced-form regression that predicts the vote from its exogenous sources—the lagged incumbent vote, the district presidential vote, and dummy variables representing year, incumbent party, region (South/non-South), and their interactions. The regression equation that produces Z is shown in Table B-1.

In the text, the key regressions (tables 1 and 2) predict the vote, when it is expected to be close, from the two spending variables plus Z. Other specifications are possible. For the record, tables B-2 and B-3 present estimates from our key equations using regressions in which there is no control and in which Z is replaced by its 34 component parts. To provide complete information, and comparability with past findings, tables B-2 and B-3 also show the insignificant contribution of a dummy variable, whether the challenger held prior elective office. This dummy typically produces significant effects in regressions that use the full cross-section of races (Jacobson 1978).

The contribution (or lack of it) of the prior office dummy variable is noteworthy. Although it shows the expected negative effect on incumbent vote when all cases are combined, its effect is statistically insignificant when the regressions are conducted separately on each safeness category. Furthermore, the inclusion or exclusion of this dummy has no effect on our estimated coefficients on incumbent and challenger spending.

In theory, the separate components should predict the vote no better than their summary measure, Z. This is confirmed by the deterioration of the explained variance when the components are substituted for Z. Also as expected, we observe that the coefficients for incumbent and challenger spending are unaffected by the replacement of Z by its components. But also as expected, because substituting the components for Z uses additional statistical degrees of freedom, this substitution expands the standard errors of the coefficients. (Tables B-2 and B-3 present standard errors rather than t-values in order to highlight that the declines in significance are due to inflated standard errors rather than deflated regression coefficients.)

For the key regressions, the spending coefficients are insensitive to the control for Z, and they even show about the same values when there are no control variables whatsoever. In fact, except for the set of mcumbent-leaning districts, Z and its components make little contribution to the equations at all. This is as one would expect. When Z is held constant at values below 52% or between 52% and 55%, its range is sufficiently restricted for the presence of controls to be of little practical consequence. In effect, Z is already controlled. Similarly, the control matters little for districts that CQ rates as too close to call. The reason here is that CQ takes into account variables omitted by Z. Once a race is rated a toss-up, it matters not whether the verdict results from

²⁸ By continuity, the asymptotic bias is approximately 0 for close races in the neighborhood of 50-50.

²⁹ For the rare races in which the challenger is expected to win handily, the bias will have the perverse opposite sign, and incumbent spending effects would be overestimated.

TABLE B-1. Coefficients from Regression Equation Generating Z, the Predicted Vote incumbent Vote_{t=1} + 0.63 (0.02) × Incumbent Vote_{t=1} + 0.22 (0.02) × Presidential Vote, where α_{yrp} is the following matrix of year-region-party dummles

Year	Nonsouthern	Southern	Nonsouthern	Southern
	Republicans	Republicans	Democrats	Democrats
1974	2.25	1.49	17.64	18.72
	(1.94)	(2.40)	(1.91)	(2.90)
1976	13.97	11.85	10.98	8.80
	(1.93)	(2.33)	(1.92)	(1.92)
1978	14.69	14.89	10.69	12.29
	(1.95)	(2.59)	(1.99)	(2.24)
1980	14.49	12.11	9.63	7.03
	(1.95)	(3.19)	(1.89)	(2.31)
1984	15.68	14.81	10.05	6.12
	(1.95)	(2.72)	(1.9 6)	(2.31)
1986	11.46	10.23	16.91	21.21
	(1.91)	(2.53)	(1.89)	(2.28)
1988	12.84	12.40	13.22	14.16
	(1.91)	(2.35)	(1.90)	(2.31)
1990	6.11	5.85	10.29	14.05
	(1.93)	(2.20)	(1.89)	(2.24)

Note: Standard errors are given in perentheses: N = 1,792. Coefficients for year-party-region combinations represent the constant or intercept perticular to the specific set of cases.

TABLE B-2. OLS Regression of incumbent Vote on Spending by Expected incumbent Vote, Controlling for Z and its Components

	-		Z < 52		52 < Z < 55			
	No Control	Control Z	Control Z, Prior Office	Control Z Components, Prior Office	No Control	Control Z	Control Z, Prior Office	Control Z Components, Prior Office
Constant	50.55 (15.94)	50.55 (22.71)	52.35 (23.09)		16.80 (35.10)	51.26 (37.54)	18.12 (35.42)	
Log (/)	4.04 (1.65)	4.04 (1.85)	4.08 (1.84)	5.60 (3.26)	3.07 (0.85)	3.06 (0.84)	3.04 (0.84)	2.61 (1.41)
Log (C)	-4.11 (1.16)	-4.11 (1.18)	-3.95 (1.21)	-5.17 (1.61)	-4.47 (0.5 6)	-4.37 (0.56)	-4.32 (0.57)	-4.65 (0.70)
Exp. vote (Z)		-0.00 (0.48)	-0.07 (0.50)			0.95 (0.62)	0.93 (0.62)	
IV_{t-1}				0.02 (0.51)				0.41 (0.48)
PV				-0.32 (0.28)				0.09 (0.19)
C's prior office			-1.10 (1.77)	1.63 (2.16)			-0.51 (1.10)	-0.53 (1.21)
Year-party-region dummles				not significant				not significant
Adj. R²	.22	.19	.18	.05	.45	.46	.45	.45
SEE	5.16	5.23	5.27	5.70	4.60	4.55	4.58	4.59
N	40	40	40	40	77	77	77	77

Note: IV_{t-1} = lagged incumbent vote. PV = incumbent party presidential vote (1976 or 1988). Year-party-region dummies represent combinations of the year, incumbent party, and South/non-South. Constants are not reported when year-party-region dummies are used, since the choice of base category is arbitrary. Standard errors are given in parentheses.

TABLE B-3. OLS Regression of Incumbent Vote on Spending by CQ Categories of Competitiveness, Controlling for Z and its Components

		CQ I	Rated Close			CQ Rated L	eaning incu	mbent
	No Control	Control Z	Control Z, Prior Office	Control Z Components, Prior Office	No Control	Control Z	Control Z, Prior Office	Control Z Components, Prior Office
Constant	44.89 (9.99)	41.05 (11.43)	41.50 (11.60)		78.37 (6.17)	51.99 (6.71)	52.16 (6.72)	_
Log (/)	2.8 4 (1.11)	2.67 (1.14)	2.61 (1.16)	2.33 (1.46)	2.16 (0.53)	2.23 (0.49)	2.24 (0.49)	1.31 (0.66)
Log (C)	-2.47 (1.16)	-2.31 (1.18)	-2.30 (1.19)	-1.45 (1.41)	-4.07 (0.46)	-3.84 (0.43)	-3.84 (0.43)	-3.83 (0.46)
Exp. vote (Z)		0.07 (0.10)	0.07 (0.10)	_		0.38 (0.05)	0.37 (0.05)	
IV_{t-1}				-0.08 (0.09)				0.13 (0.05)
PV				0.06 (0.10)				0.01 (0.05)
C's prior office			-0.34 (1.09)	0.26 (1.28)			-0.47 (0.58)	-0.58 (0.60)
Year-party-region dummies				not significant				significant at .01 level
Adj. R²	0.05	0.05	0.04	0.16	0.18	0.30	0.30	0.33
SEE	4.78	4.80	4.83	4.50	5.60	5.20	5.20	5.08
N	83	83	83	83	340	340	340	340

Note: IV_{t-1} = lagged incumbent vote, PV = incumbent party presidential vote (1976 or 1988). Year-party-region dummies represent combinations of the year, incumbent party, and South/non-South. Constants are not reported when year-party-region dummies are used, since the choice of base category is arbitrary. Standard errors are given in parentheses.

measurable variables composing Z or intangible sources of candidate strength and weakness known to CQ and other observers but not measured as part of Z.

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The Meaning and Measure of Policy Metaphors

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The apparent ability of the American public to form coherent assessments of policy options—while being largely ignorant of political institutions, actors, and ideology—remains a persistent puzzle for political science. We develop a theory of political decision making that helps resolve this puzzle. We postulate that both the public and political elites comprehend complex policies in part through "reasoning by policy metaphor," which involves comparisons between proposed alternative policies and more readily understood social institutions. Using data from 169 intensive interviews, we test claims about metaphorical reasoning for a particularly complex policy domain: health care reform. We demonstrate that our hypothesized policy metaphors are coherent to both elites and the general public, including the least sophisticated members of the public. We further show that elites and the public share a common understanding of the relevant policy metaphors, that metaphorical reasoning differs from other forms of analogic reasoning, and that metaphorical cognition is distinct from ideological orientation.

he apparent gap between political elites and the general public in their understanding of public issues remains troubling for political science and for modern democracy. Public ignorance and apathy challenge advocates of a more participatory politics, who have responded either by trying to diagnose the problem more carefully (Lasch 1995; Sandel 1996) or by formulating strategies to encourage more effective and comprehensive public involvement in political debates (Barber 1984; Etzioni 1993; Fishkin 1995).

The challenges to contemporary political science are equally profound. These are often portrayed as a paradox between the aspirations of democratic theory and the reality of public opinion research, the latter reflected in attitudes that are transitory and inconsistent (Converse 1964; Zaller 1992). In our assessment, however, a more fundamental paradox appears within research on public opinion. We have, on the one hand, compelling evidence that the public is largely unaware of political debates, institutions, or lexicons (Delli Carpini and Keeter 1996). These studies suggest that the average American knows "virtually nothing about the public issues that occupy officials from Washington to city hall" (Ferejohn 1990, 3). On the other hand, there is equally compelling evidence that the American public understands social policy at some level, which is reflected in detailed studies of the ways in which Americans describe and discuss public policy (Gamson 1992; Graber 1984); the public may even influence that policy, which is reflected in shifts in aggregate public opinion that often precede related shifts in elite decision making (Page and Shapiro 1992).

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Political scientists have offered a variety of explanations for these seemingly inconsistent findings (Neuman 1986; Sniderman, Brody, and Tetlock 1991; Zaller 1992), but none is entirely consistent with the evidence, and all rely on efforts to define how the formation of public opinion is constrained relative to elite judgments. A more promising approach involves identifying cognitive processes for assessing social policy that are shared by political elites and the general public. If such processes can be identified, they can help explain common patterns of reasoning by policymakers and the public and may reveal ways to promote richer discourse about the causes of and solutions to societal problems. Any proposed cognitive process must be able to explain how the average citizen can assess policy matters without recourse to much knowledge of the political context or to the sort of ideological frameworks that help shape elite reasoning. It also must describe a process of reasoning which is feasible for a populace that devotes little time or attention to social problems. Furthermore, the same framing must prove useful for policy elites, despite their greater knowledge of political matters and their greater reliance on ideology.

We believe that a promising candidate exists, which we will refer to as "reasoning by policy metaphor." This political calculus bases decisions on comparisons between complex policy issues and more readily understood social institutions. We hypothesize that every society has a set of commonly understood ways of arranging social institutions and judging the effectiveness of their performance. Citizens in each country whether members of the general public or political elite—share a broad understanding of the nature of these "templates" for collective activity. Each of these arrangements constitutes a sort of archetype, an ideal against which people compare the consequences of actual policies or project the expected outcomes of proposed policy reforms. It is this process of comparison that makes the reasoning metaphorical.

Our claim that elites and the public think about policy in similar ways seemingly conflicts with the conventional wisdom of decades of political science research, which consistently portrays elites as basing

their decisions on political ideology and characterizes the American public as ideologically naive (Converse 1964; Jennings 1992). We do not disagree with this portrayal of political ideology. We argue that an additional set of conceptual frames complement the role of ideology for elite decision making and largely replace ideological reasoning in the public's assessment of policy alternatives. Our goal is to explore metaphorical reasoning and demonstrate how it differs from more familiar constructs of reasoning by analogy and political ideology.

REASONING BY POLICY METAPHOR

Political scientists have long recognized the use of metaphors in political discourse (Edelman 1964; Landau 1961; Zashin and Chapman 1974), elite policy debates (Rein and Schön 1994; Shimko 1994), and media presentations about social problems (Lakoff 1996; Stone 1988). But researchers have been much less successful at establishing whether the use of metaphors reflects fundamental cognitive processes as opposed to simply figures of speech. Indeed, one generally sympathetic observer recently concluded that he had "yet to see a means of determining whether metaphors are playing a cognitive role" (Shimko 1994, 663). Identifying and measuring this cognitive role is a primary focus of this article.

The Distinctive Features of Metaphorical Reasoning

Metaphorical reasoning shares important similarities with reasoning by analogy, which has been widely studied in both foreign (Jervis 1976; Khong 1992; Vertzberger 1990) and domestic (Houghton 1998; Neustadt and May 1986) policy. Both metaphors and analogies translate situations that are complicated or novel into others that are more familiar or better understood. Such comparisons reduce cognitive burden and reveal potential alternatives for addressing problems that might otherwise bewilder decision makers (Holyoak and Thagard 1995).

But past studies suggest that metaphorical and analogic reasoning differ in at least five ways. First, analogies are drawn within domains, metaphors across domains (Kittay 1987; Shimko 1994). Reasoning by analogy, policymakers compare contemporary conflicts to wars fought in the past (Khong 1992), current urban crises to similar episodes in earlier years or other jurisdictions (Houghton 1998), or ongoing crises of presidential authority to earlier episodes in which impeachment was pending or threatened. Reasoning by metaphor, in contrast, policymakers describe health issues in terms of war (Sontag 1989) or ecology (Annas 1995), interpret international conflicts from epidemiology perspectives (Glad and Taber 1990), and liken urban crises to weeding a garden (Rein and Schön 1994). Metaphorical reasoning thus involves what we term "concrete embodiment." The implications of comparing across domains—comparing a political campaign to a horse race, for example—depend in part on the decision makers' actual experiences with horse races. Those living in eras or jurisdictions in which races are fixed will see very different connotations than those who experience races as fair and carefully regulated. The meaning of the metaphor depends on this translation to concrete experience.

Second, analogies can, at least in principle, be exact. One situation may be so much like another that it follows that successful policies under the first circumstances are very likely to prosper under the second. Indeed, the prescriptive power of analogies largely depends on the closeness of this "matching" (Holyoak and Thagard 1995). In contrast, metaphorical comparisons are always partial and are so recognized by decision makers. Those who liken surrogate motherhood to "baby selling" argue for prohibition on the same grounds that most countries outlaw slavery. But proponents make a different and equally metaphorical comparison, arguing that the surrogates' contracts for use of their bodies are similar to other labor contracts quite common in contemporary society. Each side recognizes that the comparisons illustrate parts-although potentially vital parts-of the meaning of surrogacy arrangements (Stone 1988).

Third, analogic reasoning among political elites has been shown to embody several distinct cognitive functions: description, prediction, and prescription (Khong 1992). Metaphorical reasoning also can have both descriptive and prescriptive implications. Indeed, metaphors can generate a "normative leap... from data to recommendations, from fact to values, from 'is' to 'ought'" that can powerfully shape the preferences of decision makers (Rein and Schön 1994, 94). Yet, metaphors generally are less useful than analogies as sources of prediction. Because each metaphor is a partial comparison, there is no reason to presume that the particular aspects highlighted by a metaphor are causally related to future outcomes (Shimko 1994).

Fourth, metaphorical reasoning is more likely to embody an affective component than are analogical comparisons, which are typically portrayed as "cold cognition" (Khong 1992, 225). It has long been recognized that much of the power of metaphors involves the emotional responses that they can induce in a variety of political or social contexts (Edelman 1971; Zashin and Chapman 1974). Moreover, the partiality of metaphors invites an affective response (Crocker 1977). Emotional responses provide a stimulus to focus the attention of decision makers on particular aspects of complex social issues. In this way, policy metaphors introduce an affective dimension comparable to the "likability" heuristic that has been demonstrated to influence stands on political issues (Sniderman, Brody, and Tetlock 1991). In a similar manner, the affective dimension of policy metaphors helps less sophisticated decision makers select among policy alternatives, based on their emotional response to the institutional templates.

Finally, analogies and metaphors are likely to be the heuristic of choice under different circumstances. Analogies are most frequently invoked for understanding events that are novel or take place with limited frequency (Vertzberger 1990). "Although it is easy to compare one discrete situation with another, it is difficult to analogize through time in a continuous situation" (Houghton 1998, 283). Metaphors, in contrast, appear equally useful under both novel circumstances (Sontag 1989; Stone 1988) and when addressing long-standing social problems (Rein and Schön 1994; Szasz 1974).

A Model of Reasoning by Policy Metaphors

We believe that all cultures are constructed around a core of social institutions, familiar to all citizens. By "institutions," we refer to complex combinations of norms, practices, and organizational arrangements that shape various collective activities. Our understanding of these institutions is shaped by a combination of culturally transmitted "stories" (Nimmo and Combs 1980) and personal experience with the most common embodiments of each institution (Gamson 1992). For example, the meaning of "family" is a product of one's own family history (Lakoff 1996) as well as the images of family constructed by the media and other sources of cultural authority (Coontz 1992). Because these institutions are familiar to all members of the culture, they are cognitively accessible to both the public and elite decision makers.

We argue that these institutions become lenses or templates for interpreting emerging social problems and evaluating proposed policy solutions. Psycholinguists often talk about the target of metaphors, the more familiar object to which a less familiar or ambiguous phenomenon is compared. The institutional templates described above are the targets when people reason by policy metaphor. Citizens understand problems and policies by comparing their implied consequences to the societal arrangements and functional norms that are implied by each of these institutional templates. This process of interpretation has been recognized for individual institutions by a number of analysts; see, for example, Lakoff (1996) on the family, Rodwin (1995) on fiduciary obligations, Fowler (1991) on community, and Stone (1988) on societal rights. These comparisons are metaphorical precisely because the social institutions—which embody norms, expectations, and routinized practices—exist at a different level of abstraction and social structure than do the social problems in question, which reflect behaviors and their consequences for those involved.

This hypothesized reasoning process can perhaps best be explained by illustration. It is often claimed that a particular good or service (e.g., a peaceful death, housing, a decent retirement income) ought to be considered a societal right (Glendon 1991; Scheingold 1974). Clearly, there are no explicit rights to these goods embodied in any constitution or legal code in the United States. But in thinking about how we might allocate certain goods differently if they were rights, we come to think about the moral terms in which we should judge existing allocations of goods and services, to consider whether existing arrangements are problematic precisely because they do not treat health care

or housing in the same manner as claims to a lawyer for criminal trials or access to public schools (Dougherty 1989).¹ These comparisons are metaphorical in that they are understood to be partial—a particular social issue is recognized as having some attributes of rights, but not all (e.g., being written into the Constitution). Their incompleteness allows for these goods to be seen in the light of other metaphors as well as rights.

From these postulated patterns of cognition we derive our first testable hypothesis.

HYPOTHESIS 1: Both public and elites will make use of multiple policy metaphors for constructing interpretations of policy issues. They will share a common understanding of the broad meaning of those metaphors in specific policy contexts.

We also hypothesize that metaphorical reasoning occurs in two distinct stages, the first descriptive, the second prescriptive. To avoid the vagueness of past treatments of cognitive processes in political science, such as "schemas," and to differentiate clearly metaphorical reasoning from older notions of "attitudes" (Kuklinski, Luskin, and Bolland 1991), it is essential that these two stages are in fact differentiable—that a person can understand the relevance of a metaphor in a policy domain without necessarily agreeing that it represents a desirable direction for policy reform. The first assessment represents the "coherence" of the metaphor in a particular policy domain. The second judgment we will refer to as the "support" for the metaphor in that domain. This suggests our second testable hypothesis.

HYPOTHESIS 2: The coherence of a metaphor in a particular policy domain in general will be unrelated to support for that metaphor as a prescription for future policy.

We believe that the coherence of metaphorical reasoning can be assessed by identifying the cognitive processes hypothesized to be a part of metaphorical comparisons and then testing for their congruence in actual decision making. Our analysis above suggests that metaphorical reasoning may invoke norms of equity, emotional responses, and concrete translations of the institutional template into organizational arrangements that have been directly experienced by the decision maker. We believe that another crucial aspect of the description of a social issue involves causal attributions. Evidence suggests that such attributions are an important influence on the decisions of both the general public (Iyengar 1991; Skitka and Tetlock 1993) and policy elites (Sabatier and Hunter 1989). Iyengar (1991, 8) draws the distinction between attributions of responsibility for causing the problem (causal respon-

¹ More precisely, courts and policymakers have identified some natural ("negative") rights in health care, that is, the right not to have certain health-related practices restricted by others. Abortion is one example. Unlike legal representation, however, there is no established human ("positive") right to health care, in the sense that government is required to ensure the availability of medical services, other than for prisoners and other wards of the state.

sibility) and attributions of responsibility for its remedy (treatment responsibility).

How might policy metaphors assist decision makers in assigning responsibility? They may do so in several ways. First, some (although not all) institutional templates incorporate within them strong allocations of treatment responsibility. For example, when a particular good and service is described as a societal right, it is commonly perceived that the federal government bears responsibility for remedying situations in which individuals cannot gain access to those goods (Glendon 1991). Other templates highlight causal responsibility. When a particular good is described as a professional service, there is a strong presumption that the good will be allocated based on scientific definitions of need (Kultgen 1988). Failures within this context are failures of knowledge, and their remedy is linked to selfcorrective efforts from within the profession itself (Krause 1996).

We thus hypothesize that metaphorical reasoning involves five distinct cognitive processes. The coherence of a metaphor will be measured in terms of the congruence among these processes.

HYPOTHESIS 3: Metaphorical reasoning will be evident in respondents' assessments of (a) causal responsibility, (b) treatment responsibility, (c) norms of fairness, (d) comfort with particular institutions, and (e) concrete comparisons to relevant institutional templates.

The second stage of metaphorical reasoning involves prescription. Each metaphor invokes its own "normative leap," as decision makers respond to the emotional and ethical bases embedded in the target of the metaphor, the institutional template. As in the descriptive phase, we postulate that there will be more than one guiding metaphor for most decision makers, which allows for diverse although broadly compatible directions for policy reform.

Hypothesis 4: In any given policy domain, individuals may support more than one metaphor as defining an appropriate path for future policy.

The invocation of multiple policy metaphors suggests another important stage in the reasoning process. For any given policy domain, particular metaphors will be judged as relevant in part because of their intrinsic qualities (the coherence of the metaphor, as defined above), in part related to whether they are seen as compatible or conflicting with other relevant institutions. "Compatibility" refers to the "fit" among metaphors in the various cognitive dimensions associated with metaphorical reasoning. For example, two metaphors may appear incompatible because each invokes different standards of fairness. The norms of desert associated with markets are often seen as incompatible with the more need-oriented practices associated with community (Schlesinger 1997) or family (Hochschild 1981). For the same reasons that members of a culture share understanding of particular institutional templates, they also should share an assessment of compatibility among metaphors. This generates a "logic" of metaphorical reasoning, complete with transitive properties and other operating principles.

HYPOTHESIS 5: There are broadly shared understandings among both the general public and elites about which policy metaphors fit with one another.

Decision makers may, of course, have access to heuristics other than policy metaphors. In particular, ideology may guide elite policy choices. Elite decisions are certainly more ideologically consistent than are those of the public (Converse 1964; Jennings 1992). But even for those with strong ideological guideposts, metaphors can provide useful insights that go beyond the allocations of responsibility between public and private sectors or between individual and collective action that are the mainstay of conventional American ideologies (Lakoff 1996; Tetlock and Mitchell 1993). We will test explicitly for differences between reasoning associated with policy metaphors and reasoning based on ideology to determine whether the two represent distinctive cognitive processes.

METHODS

Our general claim is that policy metaphors can assist reasoning about any complex social problem. To provide an initial test for our hypotheses, however, it is useful to focus on a particular policy domain. We begin by selecting a policy area and identifying the array of institutional templates potentially relevant for that domain. It is necessary to develop methods for assessing the *coherence* and *compatibility* of metaphorical thinking. We describe these methods and their application to a sample of public and elite respondents.

An Application to Health Policy

Health policy was selected as the domain for this study because at the time the data were collected (primarily summer 1995) health care reform was an issue of considerable salience. Polls indicated that much of the public followed these debates and formulated opinions about appropriate reforms, although people were often confused by the complex details of reform proposals (Blendon, Brodie, and Benson 1995; West, Heith, and Goodwin 1996).

The salience, complexity, and multivalent nature of the health care reform debate make it an attractive test for policy metaphors. If our model is correct, then policy metaphors have the potential to clarify what people saw as the failings of the existing system, to reduce the complexity of the debate, and to make sense of the ways in which competing proposals might fit together. To operationalize these tests, we need to establish which institutional templates may have been invoked.

Templates Relevant to Health Policy. We conducted an extensive review of health policy proposals over the past two centuries of American politics. From this review emerged five conceptions of the health care

system, each of which defines a distinctive set of core institutions for allocating medical care.²

The earliest metaphorical referents, dating back to the colonial period, treat health care as a "community obligation" (Schlesinger 1997). An outgrowth of the British poor laws, these early legislative initiatives made all infirm and indigent residents who lacked family caregivers the responsibility of the town or village, with the standards of appropriate care being locally determined (Grob 1994). Although the scope of community responsibility and authority varied considerably over the subsequent two centuries, notions of community responsibility motivate contemporary federal programs that support community health and mental health centers (Sardell 1988) and remain a powerful rhetorical frame for assessing the performance of the health care system (Light 1997). Notions of community emphasize the heterogeneity of health needs in varied subpopulations and the desirability of decentralized allocation of health care resources.

During the latter part of the nineteenth century, health care grew increasingly market oriented as different schools of medicine competed for legitimacy (Rothstein 1979), hospitals competed to attract patients (Rosenberg 1987), and individual physicians engaged in aggressive practices to market their services (Rosen 1983). In keeping with the consensus favoring laissez-faire economic policies at that time, government policymakers assumed a basically passive role. We refer to this conception as the "marketable commodity" metaphor of health care, which reemerged to guide policymaking in the early 1980s (Seay and Vladeck 1988). It emphasizes the benefits of individual choice and resource allocations based on individual preferences, with deservingness based on productive contributions to the economy.

As was true for a variety of industries at the turn of the century, the consequences of unconstrained market forces alarmed policymakers concerned about medical care. In keeping with the general Progressive orientation toward scientific reform, state policies encouraged a consolidation of professional control along with more stringent standards for professional training (Rothman 1980). The ethos of mainstream American medicine shifted to that of medical care as a "professional service," an image that subsequently dominated the thinking of doctors and policymakers for the first half of the twentieth century (Starr 1982). Medical professionalism places primacy on scientific definitions of need and the importance of professional discretion in allocating services.

It was not until almost mid-century that the notion of health care as a basic "societal right" was first clearly enunciated in American politics by Franklin Roosevelt. This metaphor implicitly redefined the norms for the performance of the health care system toward a greater concern with equal access and a greater share of responsibility for the federal government. Concepts of a right to health care quickly grew in importance, which paralleled a growing social concern for other human and civil rights (Schlesinger and Lee 1993). Although this perspective was most clearly stated in policy debates during the 1960s and 1970s, public opinion polls suggest that the idea of a right to health care continued to garner strong support through the mid-1980s (Gabel, Cohen, and Fink 1989). Notions of health care rights embody standards of minimum adequate packages of services and emphasize norms of equal access.

The fifth metaphor to appear in elite commentary on health policy is found in debates only in the last decade. Employers are viewed as intermediaries who assure the availability of health care, much in the way that geographic communities played this role in earlier periods of history. Although employer involvement in providing health insurance emerged largely as the unintended byproduct of federal policies in the period between World War II and the Korean War (Silow-Carroll et al. 1995), it is consistent with growing corporate involvement in a wide range of "social benefits" (Stevens 1988). Beginning with congressional deliberations over health care reform in the late 1980s, employer involvement was at least partially translated to employer responsibility, as political leaders chastised companies that failed to provide health insurance for their workers (Field and Shapiro 1993). As with the community metaphor, this fifth perspective emphasizes the desirability of decentralized allocation of resources, although with a stronger emphasis on equity defined in terms of deservingness.

Institutional Templates and Metaphorical Reasoning. Do invocations of these five institutions really involve metaphors? At first glance, the claims seem more literal than metaphorical. Would not the enactment of national health insurance create an actual "right" to health care? When individuals choose among health plans, are they not literally treating medicine as a commodity? It is our contention that the apparent literalness of these comparisons masks their true metaphorical nature. The comparisons are metaphorical in three distinct senses. First, the appeal of the five institutional templates is not in terms of performance but of the ideals they embody. For example, the Federal Trade Commission became an advocate for markets in medical care based on textbook notions of consumer empowerment, not on any actual evidence that market choices lead to better treatment (Starr 1982). Second, the translation of proposed services to institutional templates will always be incomplete. Health care can never be fully a "right" because the social determinants that shape the efficacy of medical treatment are too complex for any conceivable scope of government responsibility. Health care can never be completely a marketable commodity, because its "caring" aspects can be no more translated into monetary terms than can any other loving relationship. Third, the partiality of the "fit" with each of these institutional templates—whether cast in terms of rights (Dougherty 1989), markets (Annas 1994), or community (Light

² A more detailed review of the emergence and evolution of these five metaphors is presented in Schlesinger, Lau, and Thomas n.d.

1997)—is explicitly understood even by the most fervent advocates. The translations are not literal and are not meant to be. They thus allow complex combinations of templates as a guide to health care reform.

Methods for Assessing Metaphorical Reasoning

To test our hypotheses about policy metaphors, we need to assess how actual elite and public decision makers view health care reform. It is necessary to translate these broad institutional templates into a set of more specific descriptions. For each template, we constructed five statements that embody an attribution of causal responsibility, an allocation of treatment responsibility, an evocation of particular norms of equity, a description of an affective response ("comfort" with particular institutional arrangements), and a concrete comparison to another familiar policy domain which served as an exemplar for that template. See Appendix A for a complete listing of these statements.

Measuring Coherence. Although we believe that metaphorical cognition is distinct from other forms of attitude formation, respondents will still hold attitudes (i.e., agree or disagree) with particular aspects of each metaphor. One measure of metaphorical coherence could be "attitudinal coherence," the extent to which respondents who agree with any one statement relevant to a metaphor also tend to agree with the other four statements. As Kuklinski, Luskin, and Bollard (1991) point out, however, if we are to assure that policy metaphors are not simply another form of "old wine in new bottles," we need a way to measure cognitive coherence that is distinct from these more conventional assessments of attitudes. The method used here we refer to as "grouping" coherence. This reflects the extent to which the statements associated with each template seem to the respondent to "fit together" into a coherent picture of the health care system, apart from whether those statements are favored as a direction for reform. To assess metaphorical coherence, participants were presented—in randomized order-with all 25 statements about the five institutional templates,³ each on a separate card. Their initial task was to read each card and indicate the extent to which they agreed or disagreed with the statement, taken in isolation. At this point participants could ask the researcher coordinating the interview to clarify the meaning of any statement.

Participants were then instructed to group the cards into a set of piles, each of which represented for them a distinctive approach to structuring the health care system. They were explicitly told that this

grouping should *not* be based on the extent to which they agreed with particular statements, but on the ways in which the statements seemed to fit with one another. Participants were allowed to establish any number of groupings that they considered sensible; on average, 5.4 distinct groups were constructed. Respondents were given as much time as they desired to create groups that they considered satisfactory.

Sorting exercises have been used in various contexts to measure complex social and political attitudes (Brewer and Lui 1996; Conover and Feldman 1984). But these past studies were empirically rather than theoretically focused; groupings that emerged from the sorting process were assigned labels and interpreted after the fact. Our strategy is deductive rather than inductive. We postulate that statements will be grouped in a particular manner and test for the emergence of these groupings.

Attitudinal coherence can be assessed through familiar methods of confirmatory factor analysis (CFA), by treating agreement with the five statements associated with each template as distinct manifest indicators of support for that metaphor. Assessing grouping coherence requires somewhat less conventional methods. Hubert and Levin (1976) developed a statistical test (the "Quadratic Assignment Paradigm"-see also Hubert and Schultz 1976) for determining the probability that a particular sorting configuration will occur, compared to the probability that the configuration would have emerged from a random process of sorting. Their test statistic (Λ if there is only one theoretically defined grouping, Δ if the theoretical matrix is partitioned into $g \ge 2$ distinct subgroups) is based on the association between two matrices; the first is a theoretical matrix of 0s and 1s indicating what items should be grouped together, and the second is an observed matrix of the proportion of time each pair of items is placed in the same group. Asymptotic properties have been developed that allow for the calculation of a z score to determine the probability that particular observed configurations would have occurred by chance.

The Hubert-Levin measure, like CFA, provides an aggregate statistic for how well the observed data fit the hypothesized model for a group of respondents. For several of our hypothesis tests, however, it is more useful to have measures of attitudinal and cognitive coherence for individual respondents. For these purposes, we used several additional measures. Attitudinal Coherence is measured straightforwardly at the individual level by the variance in agreement ratings for the five statements associated with each metaphor. Lower variance means more attitudinal coherence. To assess Grouping Coherence at the individual level, however, it was necessary to develop a new measure, which we will label here *omega* (Ω) . For each policy metaphor, omega is calculated as the proportion of correctly grouped statements (i.e., the five that belong with a given metaphor) minus the proportion of statements that do not "belong" to that metaphor but were

³ Also presented were a dozen other "cover" statements about the health care system that were not linked to any of the five metaphors. These statements were included to increase the complexity of the sorting task and create a more realistic representation of the sort of claims made during the health care reform debate. The cover statements are not included in the subsequent measures of cognitive coherence, since we had no a priori expectations about how they would group or not group with one another.

included in that grouping. See Appendix B for more details.

Assessing Support and Compatibility. After grouping the separate statements in whatever manner they deemed sensible, respondents were shown summaries of the hypothesized metaphors that included all five hypothesized statements. They were asked to indicate the extent to which they agreed or disagreed that a particular metaphor represented a "good direction" for the future of the American health care system using a conventional five-point scale. We will refer to these as measures of overall support for (the application of) the metaphor. Respondents were then asked to consider the extent to which pairs of metaphors seemed compatible with one another, with responses on a fivepoints scale ranging from "completely compatible" to "completely incompatible." All ten possible pairings of the five hypothesized policy metaphors were considered. "Compatibility" was left undefined and thus could have been interpreted in different ways by respondents.

Subjects

To collect data on metaphorical reasoning, we identified two groups of respondents. The first was a convenience sample of the general public involving 119 participants. Although it was not possible to draw a random sample for this part of the study, recruitment strategies were adopted to ensure adequate variation in age, socioeconomic status, and political ideology. To reduce the idiosyncratic influences of residence in a particular community, participants were drawn from five sites in four states—Connecticut, New York, New Jersey, and Pennsylvania. General public subjects were 55% female and 17% nonwhite. The average age was 45, and two-thirds had a college education.

To assess the sophistication of thinking about health care issues, participants were asked how extensively they had followed the debate over health care reform and how informed they believed themselves to be on these issues. Using these two questions, we categorized participants according to their knowledge of health policy. Those who did not feel at least somewhat informed and also had not followed the reform debate (23.5%) were labeled "low sophistication"; those who considered themselves very informed or who reported following the debate very closely were labeled "highly sophisticated" (24.6% of the sample). The remaining 52% were considered moderately informed.⁵

The elite sample was composed of individuals sophisticated in health policy. Congressional staff and officials in the executive branch who participated in the formulation of the Health Security Act were chosen. Representatives of interest groups were identified from a published compendium of people considered influential in the policymaking process (Faulkner and Gray 1995). The elite sampling frame was divided into provider groups, patient groups, industry, and government; a random sample of 80 potential respondents was drawn. Fifty elite respondents were successfully interviewed, a response rate of 62.5%.

RESULTS

The Coherence of Policy Metaphors Applied to Health Care

The primary goal of our analyses is to illustrate the cognitive role that metaphorical reasoning plays in helping people make sense of complex policy issues. To do that we will demonstrate both what metaphorical reasoning is and what it is not. To begin with the affirmative, our primary evidence for the existence of metaphorical reasoning comes from the grouping activity. But will either elites and/or the general public group statements in ways compatible with the five institutional templates?

The relevant data are presented in Table 1. Because there is no intuitive way to gauge the magnitude of Hubert and Levin's Λ and Δ , the table also presents the asymptotic z score associated with these statistics. The larger the z score, the less is the likelihood that the observed grouping occurred by chance, and the more consistent the observed data are with our hypothesized groupings. We present data separately for policy elite and the total general public samples, then for the general public sample broken down by level of sophistication.

Considering all metaphors taken together (the top row of Table 1), the grouping scores (Δ) provide strong evidence of the salience of metaphorical thinking for both public and policy elites, including the least sophisticated members of the public. Although there is variation in the levels of cognitive coherence across these different subgroups (e.g., elite groupings are more coherent than those of the general public), the level of metaphorical coherence appears as high among the moderately informed public as among the most sophisticated members of the public. These data provide strong support for hypothesis 1.

If we look separately at the coherence of each of the five metaphors, we continue to see commonalities across the subgroups, although some differences in metaphorical reasoning emerge as well. The marketable commodity notion is consistently the most coherent metaphor applied to contemporary health policy; employer responsibility is the least coherent. The societal rights metaphor displays the sharpest contrast across groups; it is most strongly coherent for the elite respondents, but much less so for the public, particularly the least sophisticated.

We can look at the coherence of policy metaphors in a second way. Hypothesis 3 states that metaphorical reasoning will be evident in the five cognitive processes identified above (assessment of causal and of treatment

 $^{^4}$ In a similar manner, one can construct a grouping score Ω for each separate cognitive process—such as causal responsibility, ethical standards, concrete comparisons—as the proportion of statements for each process correctly matched with their appropriate metaphors, minus the proportion of mismatched statements. We use such a measure to help test hypothesis 3.

⁵ The results reported below change little if we divide the general public by education rather than by our measure of sophistication.

TABLE 1. Cognitive (Grouping) Coherence of Policy Metaphors, by Subgroup of Respondents

				abgroup of 1100	<u> </u>
	Pollcy	General	Public	, by Level of Sophi	stication
	Elites (N = 50)	Public (N = 119)	High (N = 29)	Medlum $(N = 61)$	Low (N = 28)
Across all 25 Items and all five metaphors	$\Delta = 24.52$ $z = 12.13***$	$\Delta = 20.53$ $z = 10.47^{***}$	$\Delta = 20.85$ $z = 9.32^{***}$	$\Delta = 21.18$ $z = 10.29***$	$\Delta = 18.25$ $z = 8.36***$
Societal Right metaphor	$ \Lambda = 6.12 z = 7.21*** $	$ \Lambda = 3.59 z = 3.07** $	$ \Lambda = 3.81 z = 3.08** $	$ \Lambda = 3.72 z = 3.11** $	$ \Lambda = 3.07 z = 1.99 $
Community Obligation metaphor	$ \Lambda = 4.16 $ $ z = 3.77^{***} $	$ \Lambda = 3.74 z = 3.43*** $	$ \Lambda = 3.81 z = 3.08** $	$ \Lambda = 3.62 z = 2.89** $	$ \Lambda = 3.54 z = 3.15** $
Employer Responsibility metaphor	$ \Lambda = 3.24 z = 2.15* $	$ \Lambda = 3.43 z = 2.66* $	$ \Lambda = 3.56 z = 2.50^* $	$ \Lambda = 3.38 z = 2.36* $	$ \Lambda = 3.21 z = 2.35^* $
Marketable Commodity metaphor	$ \Lambda = 6.12 $ $ z = 7.21^{***} $	$ \Lambda = 5.70 z = 8.32*** $	$ \Lambda = 5.41 z = 6.64*** $	$ \Lambda = 6.00 z = 8.24*** $	$ \Lambda = 5.39 $ $ z = 7.76*** $
Professional Service metaphor	$\Lambda = 4.88$ $z = 5.03^{***}$	$\Lambda = 4.08$ $z = 4.28***$	$ \Lambda = 4.26 z = 4.07*** $	$\Lambda = 4.47$ $z = 4.80^{***}$	$ \Lambda = 3.04 z = 1.91 $

Note: The measure of sophistication was messing for one general public respondent, who is excluded from the subgroup breakdown. A and Δ (the latter is appropriate when Items are divided into subgroups) can be thought of as unnormalized correlation coefficients indicating the degree of similarity between an observed co-occurrence matrix of item groupings, and a structure matrix of theoretically determined item groupings. Significance levels noted in the table assume a normal distribution. The small N sampling distributions of Λ or Δ are unknown, however, and the noted significance levels may be overly optimistic, particularly for the subgroup breakdown of the general public. If we take a very conservative approach and use Tchebychell's inequality, only z accores greater than 4.5 are significant at the p < 0.5 level. The true sliphs level will be less than or equal to that resulting from Tchebychell's procedure p < 0.01, p < 0.01, p < 0.01, p < 0.01, p < 0.01.

responsibility, norms of fairness, comfort with particular institutions, and concrete comparisons), and the analysis just presented is based on respondents' grouping of items representing these different processes. But are each of these individual processes equally strongly linked to the overall metaphor? We constructed separate measures of grouping coherence for causal responsibility, treatment responsibility, ethical norms, emotional response, and concrete comparisons, in each case averaged across the five policy metaphors. This measure equals 0 if the statements are grouped randomly or combined into a single aggregated grouping; it equals 1 if the statements are grouped in a way completely consistent with the hypothesized metaphors. Here we are not testing whether the statements about each process are grouped together across metaphors; rather, we test the extent to which statements about each cognitive process (e.g., causal responsibility), considered separately, are correctly placed with the statements about other processes related to that metaphor (see Table A-1 for statements organized by metaphor and cognitive process).

The results are presented in Table 2. They suggest that all five of the hypothesized processes have some salience for metaphorical reasoning about policy, although there are some clear differences across levels of cognitive sophistication. Among the least sophisticated of the general public, all five processes reflect only modest levels of consistency across the metaphors (that is, being appropriately grouped with their corresponding metaphor), with little differentiation among the five. In contrast, for the remainder of the public respondents, three cognitive processes stand out as significantly more salient: assignment of treatment responsibility, invoking an affective response (noted as personal familiarity/comfort), and establishing a concrete comparison. There were no distinct differences in reasoning between moderately and highly sophisticated members of the public. Among the elite respondents, all five processes were noticeably more likely to be

TABLE 2	Identification	of Cognitive	Process	Statements	with	Appropriate I	Metaphor

			Public, I	by Level of Soph	Istication
	Policy Elites (N = 50)	General Public $(N = 119)$	High (N = 29)	Medlum (N = 61)	Low (N = 28)
Diagnosis of Problem			<u> </u>	<u> </u>	
(Causal Responsibility)	0.23	0.11	0.09	0.14	0.12
Treatment Responsibility	0.42	0.27	0.28	0.31	0.23
Norms of Equity	0.28	0.16	0.16	0.18	0.14
Personal Familiarity/Comfort	0.39	0.22	0.24	0.25	0.17
Concrete Comparison	0.40	0.24	0.25	0.27	0.17

Note Table entries are the mean grouping coherence score Ω , based on the proportion of each of the five types of statements that were correctly grouped with their corresponding metaphors, less the proportion of statements grouped with them that did not belong to the metaphor. As are the same as reported in Table 1. For more discussion of the construction of this measure, see Appendix B.

		ollcy Metaphors	Public,	by Level of Sophis	stication
	Pollcy ⊟ttes (N = 50)	General Public $(N = 119)$	Hlgh (N = 29)	Medlum $(N = 61)$	Low (N = 28)
Societal Right	0.85	0.45	0.33	0.50	0.41
Community Obligation	0.37	0.66	0.68	0.67	0.62
Employer Responsibility	0.24	0.41	0.36	0.35	0.45
Marketable Commodity	0.78	0.55	0.66	0.56	0.56
Professional Service	0.45	0.58	0.58	0.61	0.45

assigned to the correct metaphor than among even the most sophisticated of the general public. But the three processes that were most consistent for the public—treatment responsibility, affective response, and concrete comparisons—were also significantly more consistent for policy elites, with an average Ω of 0.40, compared to 0.25 for the other two cognitive processes. So there is qualified support for hypothesis 3, with metaphorical comparison primarily based on three cognitive processes.

What Metaphorical Reasoning is Not

The data presented so far support the hypothesized cognitive (or grouping) coherence of policy metaphors. But is this simply a relabeling of other more familiar categories of attitudes, belief systems, or ideology? Given the variety of such principles described in the literature (e.g., Kinder and Sears 1985), answering this challenge is potentially an overwhelming task. We limit our efforts to two likely alternatives. We first contrast grouping coherence with attitudinal coherence. Second, we contrast the groupings associated with policy metaphors to those one would expect to find if respondents were grouping statements on the basis of liberalism or conservatism. This latter distinction is particularly important for elite respondents and more sophisticated members of the public, since past research suggests that they are more likely to rely on ideological reasoning.

Attitudinal Coherence. We first conducted a confirmatory factor analysis on the agreement items from the entire set of respondents in order to assess the extent of attitudinal coherence. Statistically, the hypothesized model fit the data reasonably well (a less than 2-to-1 ratio of chi-square to degrees of freedom), although the various fit indices were lower than desirable. Unfortunately, the sample sizes are too small to conduct comparable CFAs for subgroups of respondents. We can obtain roughly analogous assessments, however, by estimating the alpha coefficients for each of the five metaphors separately. These were calculated for both elite and public samples as well as for subgroups of the public, again distinguishing among public

respondents on the basis of sophistication. These results, shown in Table 3, suggest that two metaphors—societal right and marketable commodity—are most coherent for elites, while only community obligation stands out as noticeably more coherent than the others for the general public.

The alpha coefficients highlight differences between attitudinal and grouping coherence. Grouping coherence (Table 1) was generally higher for elites than for the public, but attitudinal coherence is higher among the public for the community obligation, employer responsibility, and professional service metaphors. In other words, elite respondents are more likely to understand metaphors, but the public is more likely to agree or disagree consistently with different aspects of each metaphor. And the measures of attitudinal coherence are less likely to be related to the sophistication of the general public than the measures of cognitive coherence.

These aggregated results illustrate differences in cognitive and attitudinal coherence. We can more thoroughly assess the relationship between these two forms of coherence by using our individualized measures of cognitive coherence Ω (based on the grouping scores) and attitudinal coherence (the variance in agreement with the five statements associated with each metaphor). To what extent are the two forms of coherence correlated at the individual level? Since high values of Ω represent more grouping coherence, but high variance scores represent lower attitudinal coherence, if the two are related we should expect negative associations. The simple Pearson correlation between our measures of cognitive and attitudinal coherence are shown in the first column of Table 4. All the correlations are small for both elite and general public respondents, and none is significantly distinguishable from zero. Attitudinal coherence and grouping coherence are quite distinct.

Ideology. To what extent is grouping coherence determined by ideology? We have no reason to believe that liberals or conservatives would be more likely to think in terms of metaphors in general, but several of the individual policy metaphors do sound as if they could be ideologically loaded (societal rights potentially appealing to liberals, marketable commodity to conservatives). We want to make sure that their coherence is

These data are available from the APSR web site and from the authors upon request.

-0.11 (116)

TABLE 4. Distinguishing Metaphorical Coherence from Other Attitude Measures Correlations with Alternative Measures Cognitive or Grouping Coherence,* Attitudinal Ideological Ideological Support for the By Metaphor Coherence^b Position^o Strength^a Metaphor* Elite Respondents Societal Right -0.05(50)-0.15(48)0.15 (48) 0.14(49)Community Obligation 0.05 (50) -0.15(48)0.25(48)-0.08(49)**Employer Responsibility** -0.03(50)0.23(48)0.22(48)-0.38*(49)Marketable Commodity -0.10(50)0.09 (48) 0.29*(48)0.16 (49) Professional Service 0.15 (50) -0.11(48)-0.12(48)0.07 (49) General Public Respondents Societal Right -0.03(116)-0.20*(107)-0.03(107)0.14 (116) Community Obligation 0.01 (115) -0.10(107)0.08 (107) -0.09(116)Employer Responsibility 0.04(116)-0.01(107)-0.01(107)-0.11 (116) Marketable Commodity -0.04(116)0.02 (107) 0.05 (107) -0.14(116)Professional Service -0.08(116)

Note. Table entries are Pearson product-moment correlations, with Ne in parentheses, *p < 05

-0.07 (107)

not due entirely to the groupings of conventional ideologues.

As a first cut, Table 4 shows the zero-order correlations between our individual measures of grouping coherence of each metaphor and a simple seven-point measure of ideological self-identification in the second column, and the extremity of ideological identification (the same seven-point scale, "folded" at its midpoint) in the third. Most of these correlations are indistinguishable from zero, and the only pattern in the data is a tendency for strength of ideological identification to be related to grouping coherence among elite respondents. These correlations are modest at best, and only one—the coherence of the marketable commodity metaphor—is statistically significant. At first glance, then, ideology seems to have little to do with the cognitive coherence of policy metaphors.

To compare metaphorical and ideological bases for grouping, we must identify the statements used in the grouping exercise that are ideologically loaded. We did this by regressing support for each statement on selfreported ideological orientation to identify statements distinctly favored by particular ideological groups (e.g., self-reported liberals, moderates, and conservatives). We then constructed the Hubert-Levin grouping score, in this case using ideology as the map for assessing the respondents' sorting.7 Ideology proved a poor substitute for policy metaphors, at least in terms of thinking about health policy concerns. Among elites, Hubert and Levin's $\Delta = 24.52$ (z = 12.13) for metaphorical grouping, whereas $\Delta = 5.76$ (z = 2.67) for ideological grouping. Among the general public, $\Delta = 20.53$ (z = 10.47) for metaphorical grouping, while $\Delta = 5.34$ (z =1.58) for ideological grouping. This pattern also held for even the most sophisticated subset of public respondents. Thus, the data are much more consistent with metaphorical than ideological reasoning.

-0.12(107)

Support for Metaphors and the Relationship between Support and Coherence

The distinction between attitudinal and grouping coherence presented above offers some indirect evidence relevant to hypothesis 2. If our model of metaphorical reasoning is accurate, then one would expect individuals to be able to reach judgments about whether they support the application of a metaphor to a particular policy domain independent of the extent to which they understand that metaphor as a coherent whole. We can test this relationship more directly using our individuallevel measure Ω of grouping coherence and testing for the correlation between grouping coherence and support measures for the five metaphors.

These correlations are shown in the last column of Table 4. If metaphorical coherence is a function of support for the application of the metaphor, then all these correlations should be positive. They are not; in fact, across the elite and general public samples, six of the ten correlations (and the only statistically significant one) are negative. This provides strong evidence that support for and understanding of policy meta-

^{*}Measured by Ω, the extent to which statements associated with each metaphor were correctly grouped with one another and not with other etatements. See Appendix B for details.

Measured by the variance of respondents' agreement with the five statements making up each metaphor. "We used the standard NES item." We hear a lot of talk these days about liberals and conservatives. Here is a 7-point scale on which the political views that

people might hold are arranged from extremely liberal to extremely conservative. Measured as the absolute value of the deviation from the mid-point of the ideological scale.

[&]quot;Measured by agreement that the metaphor serves as "a good guide to health care reform

⁷ We created three dummy variables for these regressions, for self-identified liberals, moderates, and conservatives (nonideological respondents were the excluded group). A statement was considered ideologically loaded if it was significantly related to one and only one of the three dummy variables. Twelve statements (four for liberals, three for moderates, and five for conservatives) met this criterion. There were, therefore, four expected groups: liberal statements, moderate statements, conservative statements, and those with no apparent ideological loading.

TABLE 5. Overall Leve			Public,	by Level of Sophistication		
	Policy Elites (N = 50)	General Public $(N = 119)$	Hlgh (N = 29)	Medlum (N = 61)	Low (N = 28)	
Societal Right	73.4%	63.3%	55.6%	61.7%	74.1%	
Community Obligation	53.1%	41.9%	40.7%	38.3%	51.9%	
Employer Responsibility	46.9%	55.5%	57.8%	56.7%	55.6%	
Marketable Commodity	42.8%	53.9%	51.8%	55.0%	55.6%	
Professional Service	22.4%	47.9%	51.8%	45.0%	55.6%	

phors represent two distinct cognitive processes, as suggested by hypothesis 2.

reported in the table reflect "strongly agree" and "somewhat agree."

Hypothesis 4 predicts that individuals will recognize desirable features of more than one metaphor for each policy domain. This can be tested with our overall measure of metaphor support. Table 5 shows the percentage of respondents supporting each metaphor (where support is defined as the two most positive responses on the five-point scale). Within each column of Table 5 (i.e., each subgroup) these percentages would sum to no more than 100% if respondents supported only a single metaphor. Clearly, this is not the case. For the entire sample, 78% of respondents agreed with more than one metaphor as a guide to health care reform. Half supported three or more of the metaphors. There were modest differences in this regard between elite and public respondents. Elites were slightly more likely (84%, versus 75% of the general public) to support multiple metaphors, but they were also less likely to favor a large number of metaphors. Thirty-one percent of the public supported four or more of the metaphors; this was true of only 12% of the elite respondents.

Compatibility among Policy Metaphors Applied to Health Care

Hypothesis 5 suggests that there is a broadly shared understanding among the general public and elites about which policy metaphors fit together. Our measures of compatibility among metaphors provide a final test of the comparability of metaphorical reasoning between elites and the public; they also offer the opportunity to assess whether there is a distinctive "logic" to reasoning in metaphorical terms. In both cases, we rely on the scores that respondents assigned for the fit between different pairs of metaphors (5 = completely compatible, 1 = completely incompatible).

The average compatibility scores are reported in Table 6 for both samples, with the public again disaggregated by level of sophistication. Both the public and elites clearly see some metaphors as reasonably compatible (most notably societal rights and community obligation), others as directly conflicting with one another (both societal rights and community obligation metaphors conflicting with marketable commodities). Com-

paring the elite assessments of fit to those of the public as a whole, we find remarkable congruity; the overall correlation between the two sets of ratings is r=.91. Indeed, a multivariate test considering all ten compatibility ratings simultaneously reveals no significant difference between public and elite (F < 1). The only apparent deviation involves the perceived fit of the professional and market metaphors, which are considered more compatible by the public than by elites. These ratings provide strong evidence, albeit indirect, that the metaphors have common meaning to the two groups, as suggested by hypothesis 5.8

When we disaggregate public responses by level of sophistication, we continue to see considerable congruence,9 but also gain some insights into the differences in perceptions of metaphors for the least sophisticated. The overall correlations in compatibility scores between the elite and the public ranged from r = .91 for the most sophisticated public participants to r = .75 for the least. The primary difference lies in the extent to which professional services are seen as compatible with more privatized institutional arrangements. The less sophisticated the respondent, the more compatible professional services are perceived to be with both markets and employer-mediated institutions. We suspect that this pattern emerges because the most sophisticated respondents are more aware of the collective functions performed by most professions, including the generation of knowledge, the development and implementation of standards of training, and so on. Less sophisticated respondents may be more likely to personalize the meaning of professional service into the image of the specific doctor's office or medical group with which they have regular contact.

We suggested earlier that metaphorical reasoning ought to display certain logical properties, such as transitivity. One can test for this by examining the correlations of compatibility ratings for pairwise combinations of metaphors. If metaphorical reasoning is transitive, then ft templates A and B are seen as compatible, and templates B and C are judged to be (m)compatible, then templates A and C also ought to be seen as (in)compatible. This was true for more than 85% of the pairwise comparisons reported by our respondents, which suggests that transitivity holds among metaphors.

^{*} Again, a multivariate test revealed no significant difference in compatibility rating, across the three public groups, F < 1.

TABLE 6. Perceived Compatibility of Metaphors

			Public, by	Level of Sop	histication
Palrwise comparison between:	Policy Elite (N = 50)	General Public (N = 119)	High (N = 29)	Medlum (N = 61)	Low (N = 28)
Socletal Right-Marketable Commodity	2.13ª	2.14	2.15ª	2.10⁴	2.37
Community Obligation-Marketable Commodity	2.64 ^b	2.52 ^b	2.26	2.50 ^b	2.85 ^{e,b}
Employer Responsibility-Professional Service	3.04 ^{b,o}	3.27 ^{o,d}	3.00 ^b	3.27°	3.37 ^{a,b}
Community Obligation-Professional Service	3.47° ^{,d}	3.28 ^{o,d}	3.00 ^b	3.47°	3.15 ^{a,b,o}
Community Obligation-Employer Responsibility	3.28 ^{0,d}	3.28 ^{o,d}	3.26 ^{b,o}	3.30°	3.26ª,b,o
Societal Right-Professional Service	3.23 ^{b,o,d}	3.34 ^{o,e,f}	3.48 ^{b,o}	3.23°	3.37 ^{b,o}
Societal Right-Employer Responsibility	3.53 ^{o,d}	3.38 ^{o,f}	3.44 ^{b,o}	3.28°	3.44ª,b,o
Employer Responsibility-Marketable Commodity	3.57 ^{o,d}	3.68 ^{d,●,g}	3.48 ^{b,o}	3.78°,d	3.70 ^{b,o}
Professional Service-Marketable Commodity	3.13 ^{b,o,d}	3.76 ^{d,f,g}	3.41 ^{b.o}	3.81 ^{c,d}	4.00°
Societal Right-Community Obligation	3.79 ^d	3.97	4.07°	4.07°,d	3.63ª.b.o

Notes: Compatibility reported on a five-point scale ranging from "completely compatible" (5) to "completely incompatible" (1) Multivariate tests comparing all ten compatibility ratings of elites to those of the general public suggest that any differences between the elite and public are probably due to chance. Likewise, there are no reliable differences between the compatibility rating of the three subgroups of the general public. Hence, there are no significant differences within rows across columns of Table 6. Within columns, however, there are some significant differences. Means sharing supersoripts do not differ from each other at the .06 level by the Newman-Kaule post hoc test. For example, in the first column of the table (i.e., the elite sample), the first mean 2.13 is significantly less than every other mean in that column since no other mean in that column shares an "a" supersoript. The next two means in that column (2.64, 3.04) do not differ from each other, as indicated by the sharing of a "b" superscript. The second mean (2.64) does differ significantly from the fourth (3.47) anoe they share no common superscripts. But the third mean (3.04) does not differ significantly from the fourth, as indicated by the sharing of a "o" superscript.

DISCUSSION

In this article we develop a model of reasoning by policy metaphor and apply it to the arena of health care reform. The model identifies several distinct stages in metaphorical reasoning—understanding the metaphor, deciding whether it is appropriate as a guide to policy, and determining whether it is compatible with other favored institutional templates. We hypothesized that five cognitive processes underlay metaphorical reasoning-identifying causal responsibility, assigning treatment responsibility, applying norms of fairness, evoking affective responses, and establishing concrete comparisons-although our results suggest that policy metaphors are dominated by the second, fourth, and fifth of these processes. The data provide evidence that (1) policy metaphors are coherent to both policy elites and members of the general public; (2) understanding particular metaphors appears to be distinct from favoring that metaphor as a guide for policymaking; and (3) there are shared patterns in metaphorical thinking between elites and the general public, as reflected in the relative coherence of different metaphors, the cognitive processes that are most salient for metaphorical reasoning, and the extent to which pairs of metaphors are seen as compatible with one another. Both understanding of individual metaphors and assessments of compatibility among metaphors are largely distinct from a respondent's support for that metaphor in a particular policy domain. This study also offers strong evidence that policy metaphors are distinct from political ideology.

It is, of course, important to interpret these findings in light of some of the methodological limitations of this study. First, we focused on a single policy domain. The

extent to which the particular metaphors identified here or the general model of metaphorical thinking can be applied to other policy areas is not clear. ¹⁰ In particular, the salience of health care reform in 1993–95 may have made less sophisticated public respondents more aware of prevailing templates and thus more capable of metaphorical reasoning. Yet, we have no reason to expect that health care is so unique that health policy issues would be thought about in ways entirely different from other forms of domestic policy.

Second, we cannot determine through the techniques used here whether metaphorical reasoning is used spontaneously by decision makers or was in part stimulated by our experimental methods. For several reasons, we do not believe that the findings can be dismissed as an artifact of our testing procedure. First, there is considerable evidence in the literature of the spontaneous use of metaphorical reasoning about policy (Annas 1995; Lakoff 1996; Rein and Schön 1994; Shimko 1994). Although largely drawn from episodes involving policy elites, these situations often involve elites trying to communicate issues to the public by using metaphors to make issues more comprehensible. Second, we do not think that our primary methods artificially induced patterns of metaphor coherence.

¹⁰ Lau and Schlesinger (1997) explore the relevance of the five metaphors identified here to four additional policy domains: long-term care, the treatment of substance abuse, programs for the homeless, and public education. The metaphors are shown to predict policy positions (above and beyond the traditional measures of political ideology, party identification, political values, and so on) toward medical care as well as government actions in these four other policy areas.

Although we did frame metaphors in terms of the particular statements, the grouping exercise from which we constructed our measures of cognitive coherence represents a relatively unobtrusive method of assessing thinking. Indeed, we believe that the process was somewhat biased against grouping by metaphor. There would likely have been a natural tendency for respondents to group statements by the extent to which they agreed with them (despite instructions to the contrary) or their commonality in terms of the cognitive processes (e.g., assigning treatment responsibility) that they invoked. Finally, one would not expect whatever mild cues were created by our methods to have any substantial influence on the responses from policy elites, whose experience gives them well-defined ideas about health policy. Yet, this was the group for which the cognitive coherence of policy metaphors was stron-

The use of policy metaphors in political judgments offers the first explanation of the paradox of public opinion we posed at the outset. Members of the public can reach reasoned (and reasonable) assessments of policy even if largely ignorant of the details of policy or the workings of American politics. They can do so because policy metaphors involve comparisons to familiar social institutions and are thus largely independent of the political context. But the evidence from the policy elites in the study suggests that even for those who have a clear understanding of the political factors affecting health policy, the metaphors identified here are meaningful constructs.

Metaphorical reasoning calls for an approach to assessing public opinion somewhat different from those conventionally employed. Measures of political knowledge become less relevant; exposure to and assessment of various social institutions more relevant. Support for policies is less likely to be shaped by conventional understandings of ideology, self-interest, or sociotropic considerations and more likely to be influenced by the policy metaphors that are evoked by particular social issues or policy proposals. Needless to say, this requires a rather different set of questions on public opinion surveys.

If our findings can be generalized, they may shed some light on the political dynamics of responses to complex social problems. Coalition formation may require appealing to supporters of several different metaphors. Our findings suggest that not all conceivable combinations of metaphors will make sense. Some are seen as fundamentally incompatible with others. Policies that try to bridge these gaps will thus appear incoherent to the public, however much sense they make from a more technical or political perspective. We predict that the resulting loss of legitimacy would make such policies hard to enact, and we note in passing that President Clinton's failed health care reforms tried to wed two metaphors that our respondents perceived to be the most incompatible—societal rights and marketable commodity.

Metaphors also address the problems of communication between policy elites and the general public. Efforts to improve democratic participation have been hampered by their exclusive focus on the rules or protocols for deliberation. They offer no help with challenges of effectively discussing complex societal problems. To invoke a linguistic metaphor of our own, past approaches emphasize a grammar of deliberation but fail to provide any vocabulary. We believe that policy metaphors represent one potentially useful vocabulary. By melding this approach with protocols for effective deliberation, we believe that the language of policy debate can be substantially enhanced.

In describing the heuristic properties of policy metaphors, we have emphasized positive attributes of metaphorical reasoning, but psychologists have shown that virtually all decision-making heuristics can bias or otherwise distort decision making (Kahneman, Slovic, and Tversky 1982; Nisbett and Ross 1980). As discussed above, these sorts of biases have been extensively documented for the use of analogies in various policymaking contexts (e.g., "top-down" processing, "availability" biases; see Khong 1992 or Neustadt and May 1986). Metaphorical reasoning appears less vulnerable to these particular biases, but it may have other liabilities. Because metaphors must be grounded in concrete experience to be effectively interpreted, they may be viewed in quite varied ways by different decision makers. Americans' understanding of "community," for example, varies considerably, both geographically and over time (Fowler 1991). Consequently, its application to American health policy tends to be deeply contested (Light 1997; Schlesinger 1997).

Despite the limitations inherent in metaphorical reasoning, more explicit reliance on metaphorical alignment may have the potential to improve democratic deliberation. Because this has not yet been demonstrated empirically, our prescriptions must be viewed as speculative. Clearly, the next step would be to establish the relevance of policy metaphors to a broader set of social policy concerns. There also is considerable potential in experiments that would attempt to establish a common language for policy deliberation that is based on a shared understanding of policy metaphors. If the findings presented here are generalizable, such a dialogue ought to prove accessible to both elites and the general public. This certainly would represent an improvement over the state of much contemporary political debate on complex policy matters.

APPENDIX A: DESCRIPTIVE STATEMENTS CAPTURING METAPHORICAL REASONING

The 25 metaphorically oriented statements provided to respondents for grouping are listed below. One statement represents each of the five hypothesized cognitive processes for each of the five hypothesized policy metaphors. Each statement was presented to subjects on a separate note card.

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Statement Applied to a			Cognitive Process		
Particular Policy Metaphor	Diagnosts of Problem (Causal Responsibility)	Treatment Responsibility	Personal Familiarty (Comfort)	Norms of Equity	Concrete Compartson
Societal Right	A big problem with the health care system is that your health care depends on what your health plan allows, what you earn, and where you live.	You need national solutions to address skyrocketing health care costs and the millions of people without any health insurance.	You can trust federal programs to be fair and responsive to public needs.	The most important aspect of a just health care system is that everyone be treated equally.	The health care system would work better if it was run more like the Social Security system.
Community Obligation	The biggest problem with the health care system today is that hospitals and physicians are no longer committed to meeting the needs of the cities and towns where they provide health care.	We should seek solutions to health care problems from local government or community-based groups because each community has different health needs.	People who live in the same area know each other's health problems and share beliefs about how to solve them.	A fair health care system is one in which people go to the same hospitals and doctors as do other people whom they know and with whom they share common values.	The health care system would work better if it were run locally, like elementary and secondary education.
Employer Responsibility	The most important falling of the health care system is that you can't count on getting health benefits when you get a job or keeping decent benefits from one year to the next.	The best way to find a good solution to the challenges of health care reform is to let workers and managers bargain with one another to decide on health benefits.	People who work together share a lot of the same needs and concerns; they would be comfortable sharing the same health insurance, doctors, and hospitals.	People who are most productive should make the most income and be offered the best fringe benefits.	The health care system would work much better if retired workers could get health care benefits from their former employer, just like they get other retirement benefits.
Marketable Commodity	Our health care system is struggling because people don't think about purchasing health care wisely.	In the end, each person ought to make the choices that determine how much health care he or she will pay for and receive.	Most people are comfortable choosing a good health care provider and can tell when they're getting a good value for their health dollar.	The health care system would be most fair if it allowed individuals to decide about the health insurance and health care that best matched what they want to pay for.	The health care system would be more effective if people looked for a doctor in the same way they look for a good auto mechanic.
Professional Service	A major problem with American medicine is that doctors no longer place the patient's well-being above concerns about health care costs.	The best solutions to our country's health care crists will be found by physicians and others who provide health services.	Most people trust their physician with their health care problems and would be comfortable having physicians guide policymakers in reforming the health care system.	People should get health care based on needs, as determined by medical experts, regardless of costs.	The health care system would work better if doctors had full control over the system in the same way that judges run courtrooms.

APPENDIX B: AN INDIVIDUAL-DIFFERENCE MEASURE OF GROUPING COHERENCE

The overall coherence score Ω is given by $\Sigma_{\iota}\Sigma_{\iota}[h_{\iota\iota}]$ $(N_i-1)-m_{ij}/(N_i-N_i)/J$, where for every statement j in metaphor I the h ("hits") refers to the number of other statements from the metaphor that were correctly grouped with the statement in question; the m ("misses") represents the number of other statements that did not belong in the metaphor that were grouped with the statement in question; N_i , is the number of items hypothesized to belong in group j; J is the number of distinct groups in the entire set of statements; and N_i represents the total number of statements in the grouping exercise. Translated into English, the measure represents for any individual subject the average proportion of items (across all items theoretically in the group) that were correctly placed in their hypothesized group, minus the average proportion of incorrect items that were placed in the group. Correspondingly, one can calculate a grouping score specific to each metaphor (summed across the five cognitive processes) using the formula presented above, except not dividing the difference of proportions by the number of groups J.

The statistical properties of our individualized grouping measures are somewhat complex. If respondents group statements in extreme ways (e.g., put all statements into one pile, or divide all statements into individual piles), the measure yields a score of 0. A grouping perfectly consistent with our hypothesized metaphors yields a score of 1.0. If respondents were to group by placing together all statements from a given cognitive process, rather than a given metaphor, they would obtain a score of -.2.

Although the measure Ω seems a plausible way of scoring grouping compatibility at the individual level, it is important to validate its correlation with the Hubert-Levin Δ . To assess this relationship, we divided our sample into four groups: elites and three subsets of the public, based on the public's reported level of sophistication about health care reform (as described in the text). For each of the four subsamples, we calculated both a Δ and a mean Ω (averaged across the respondents in the subsample) for each of the five metaphors. This provided us with twenty scores for each measure (five metaphors times four subgroups). The correlation coefficient between the two measures calculated across these 20 scores was r=.975, which leads us to conclude that the two are measuring essentially the same reasoning process: Δ at the aggregate level, Ω at the individual level.

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Cabinet Terminations and Critical Events

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e present an empirical assessment of Lupia and Strøm's noncooperative bargaining model of cabinet terminations. We construct a stochastic version of the model and derive several testable implications. As the next mandatory election approaches: (1) the probability of an early election increases; (2) a cabinet's risks of being replaced without an intermediate election may be flat or even decrease; and (3) the overall chance that a cabinet falls (for whatever reason) increases. Using nonparametric duration analysis on a 15-country data set, we find qualified support for the Lupia and Strøm model. We conclude that the strategic approach is more promising than the nonstrategic alternative, but a more fully dynamic strategic model will be required to account for the dynamics of cabinet stability.

Gleiber's path-breaking work (1984, 1986), the concept of a "critical event," that is, an exogenous random shock that destabilizes an existing government, has been a central part of research on cabinet termination. The original work by Browne and his colleagues attempted to characterize the distribution of cabinet durations by relying solely on assumptions about how critical events are realized over time. Following Strøm (1983), King et al. (1990) and Warwick (1992, 1994) expanded the usefulness of that approach by studying how covariates affect cabinet duration by altering the distribution of failures implied by the critical events approach.

Lupia and Strøm (1995) recently presented a gametheoretic model of cabinet termination that sheds new light on the role of critical events in cabinet survival. Their key insight is to recognize that—contrary to Browne, Fendreis, and Gleiber—not all shocks will become critical events and destabilize the government. Rather than determine directly whether cabinets survive, the events are interpreted as inputs into a coalition bargaining process. They correspond to bargaining parameters that determine each party's outside option if the government falls. Whether a cabinet terminates depends on the attractiveness of this outside option compared to each party's payoff from the current government.

This interpretation of the effect of random events marks a potentially important shift in the study of cabinet termination. In particular, the specification of explicit models of interparty bargaining is likely to focus attention on the role of institutional features of the bargaining environment in determining cabinet failure rather than on a list of cabinet attributes (such as the number of formation attempts) or general measures of the political environment (such as the number of parties represented in parliament), which have been emphasized in the empirical literature (King

et al. 1990; Strøm 1985; Warwick 1994). For example, it may be more important to ask which actors have the power to dissolve the legislature and whether these differences have any consequences for the stability of cabinets. Due to the potential methodological and substantive implications of the bargaining approach, it is important to assess its empirical usefulness in comparison to alternatives, but some challenges arise in formulating and conducting such an assessment.

The first challenge is that the Lupia-Strøm framework (like most game-theoretic models) is deterministic, whereas the phenomenon (cabinet termination) is probabilistic. We solve this problem by embedding the Lupia-Strøm model in the stochastic environment used by Browne, Fendreis, and Gleiber. We can then derive theorems about the probability of cabinet survival from the model, interpret them as alternative hypotheses, formulate the (nonstrategic) Browne, Fendreis, and Gleiber model as the null hypothesis, and use classical hypothesis testing to assess whether the Lupia and Strøm strategic model significantly improves upon the nonstrategic approach.

The second challenge is that the model is intended to explain an empirical regularity, but on close inspection the claimed regularity is not an implication of the model. At issue is Warwick's (1992) finding of increasing hazard rates, that is, the conditional probability of a termination in the next period given the cabinet has survived thus far. In other words, the longer a government is in power, the more likely it is to terminate. As we discuss below, even the stochastic version of the Lupia-Strøm model does not imply Warwick's finding of increasing hazard rates, but it does imply a closely related empirical regularity. The difference is that Warwick's result refers to increasing hazard rates in elapsed time, whereas the stochastic version of the Lupia-Strøm model implies increasing hazard rates in the time remaining until the next regularly scheduled election. That is, the less time left until an election must be called, the more likely it is that the government terminates. This conceptual subtlety has important practical consequences, since we cannot use standard estimation techniques, designed for hazard rates in elapsed time, to test the implications of the model.

In addition to clarifying the empirical implications of the Lupia and Strøm model, our stochastic reconstruc-

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tion has important empirical implications that are new to the literature. In the Lupia and Strøm model each round of bargaining results in one of three possible outcomes: the status quo government persists, the chamber is dissolved and early elections are held (a dissolution), or the incumbent cabinet is immediately followed by a new one (a replacement). This distinction allows us to study not only cabinet termination but also the mode of termination. As Lupia and Strøm show, the factors that determine cabinet survival (irrespective of the termination mode) are in general different from those that dictate whether cabinets end in dissolution or replacement. Our stochastic version of their model takes this intuition farther: We demonstrate that dissolution hazards increase, but replacement hazards may be flat or even decrease. A further implication is that dissolution hazards may increase at a steeper rate than pooled hazards.

We use data on postwar cabinets to test these implications. The practical consequence of our theoretical analysis is that we cannot rely on the standard estimation techniques of duration analysis used in previous studies, which are all defined in terms of elapsed time and involve only one termination mode. Another implication is that we need to take a nonparametric approach because we make no assumption about the distribution of events themselves, only the rate at which they occur.

THE LUPIA/STRØM MODEL

Lupia and Strøm propose a framework in which three parties bargain over whether to replace the current cabinet, continue with it, or call new elections. Bargaining occurs in an environment of perfect information, and collective decisions are taken by majority rule.

Formally, each party $i \in \{0, 1, 2\}$ has a fixed share of seats, s_i . The game presupposes an initial governing coalition composed of two parties (1 and 2). The third party (0) forms the opposition. If party i and i' form a coalition, its value to party i is denoted g_i' . Hence, parties may value some potential coalition partners more than others. The distribution of the value associated with a governing coalition is indicated by parameter c, which indicates party 1's share. Party 2's share then equals 1 - c. Parties receive payoffs $u_i(\cdot)$ as a function of their respective seat share, the value of the governing coalition, and the share of power. In particular, we have for party $1, u_1(s, c, g_1^2) = s_1 + c \cdot g_1^2$; for party $2, u_2(s, c, g_2^1) = s_2 + (1 - c) \cdot g_2^1$; and for party $0, u_0(s, c, g_2^2) = u_0(s, c, g_1^2) = s_0$.

In addition to the payoffs associated with potential governing coalitions, bargaining depends on two exogenously given parameters for each party i: the benefits b_i and costs E_i of an "event." Events are interpreted as common knowledge information about what would happen if parliament were dissolved and an election were held immediately. Parameters b_i and E_i function as outside options in the bargaining process. It will be sufficient to consider the net benefits from holding elections, defined as $e_i := b_i - E_i$.

Coalitional bargaining proceeds as follows. Condi-

tional on observing an event and a governing coalition $\{1, 2\}$, the members of the governing coalition may make offers to the opposition party 0 or to their coalition partner. Party 1 makes the first offer. If it is accepted we have a new coalition, and the players incur a negotiation-related transaction cost, k_i . This coalition may consist of the previous members (but possibly with a new distribution of power) or may be composed of new members. If all offers are rejected, the chamber decides by majority rule on whether to dissolve and hold subsequent elections. If the motion to dissolve fails, the status quo prevails.

Using subgame perfect Nash equilibria, Lupia and Strøm give necessary and sufficient conditions for each of these outcomes and discuss some comparative statics implications of their model. Most of these implications, however, refer to changes in transaction costs that are notoriously hard to measure. In the discussion of their formal results, they also suggest that their model implies Warwick's (1992) finding of increasing hazard rates. Their argument proceeds as follows. At the beginning of every period a government has some expected life-span which is always less than the time to the next regularly scheduled election. During its time in office the government receives a per-period payoff from policy outcomes or from collecting distributive benefits. Consequently, at the very beginning of its term, the total benefit a government can expect to collect over the life of the cabinet is large, and an early election will seldom look promising. Early dissolutions will only occur if the expected electoral benefits are very high, an unlikely event. Yet, because governments must call elections at constitutionally mandated intervals, the expected benefits from staying in office will decrease over time. Thus, as a parliament approaches the end of its constitutional interelection period (CIEP), smaller and smaller events (i.e., smaller expected electoral benefits) will be sufficient for a dissolution. This, according to Lupia and Strøm (1995, 656, n. 24), implies increasing hazard rates.

There are at least two problems with Lupia and Strøm's assertion. First, the argument applies only to dissolutions, whereas Warwick's analysis does not differentiate among different termination types. To see why his finding is logically independent from Lupia and Strøm's argument, consider the opposite case, in which the hazard rate for dissolution decreases, while the hazard rate for replacement increases at a higher rate. In this case, Warwick's analysis nevertheless indicates an increasing pooled hazard rate. Thus, a finding of increasing pooled hazard rates alone does not confirm the Lupia and Strøm model. Second, the predicted change in hazard rates from period to period does not apply to elapsed time (as is the case in Warwick's analysis) but to time remaining until the end of the CIEP.

MODELING CRITICAL EVENTS

To address these problems, we present a probabilistic version of the Lupia and Strøm model. It differs from the original in three respects. First, we make explicit the assumptions implicit in Lupia and Strøm's model. Second, we specify a stochastic process that governs the occurrence of events. Third, we make a few symmetry and boundary assumptions that simplify notation and computation.

Assumption 1 formalizes the idea that an event consists of shifts in electoral prospects, not shifts in the current distribution of seats or transaction costs. Furthermore, the share of power, c_i , is assumed to be constant, since a change in coalition shares, by definition, would imply a different governing coalition. To save on unnecessary notation and case distinctions, we assume that negotiation costs are the same for all parties and not "too high."

Assumption 1. For all players, s_v c_i , and k_i are constant, with $k_1 = k_2 =: k$ and $s_1 > k$, $s_2 > k$.

Assumption 2 refers to the dynamics of office benefits. This is a critical component if the model is to imply increasing hazard rates. Specifically, we allow each g_t^t to vary over time and write $g_t^t(\tau)$, where τ denotes the time left until an election must be called, with $T \ge \tau \ge 0.1$ T thus denotes the CIEP, the maximally permissible time between two elections. Similarly, we write $u_t(\tau)(s_t, c, g_t^{t'}(\tau))$.

In the Lupia and Strøm model the size of the pie to be distributed varies depending on who is in the coalition. Since the distribution is already captured by c (and any other differences in utility by s_i), we simply write $g_{\{l,l'\}}(\tau) := g_l'(\tau) = g_l'(\tau)$. We then have the following assumption on how the benefits from governing change over time.

Assumption 2. For all parties i, i' and
$$\tau' < \tau$$
: $g_{\{i,i'\}}(\tau) - g_{\{i,i'\}}(\tau') = d(\tau, \tau') > 0$.

Assumption 2 says that as the end of the CIEP approaches, the overall value of the coalition to the parties in government will decline. The next assumption specifies the stochastic process that governs the occurrence of events.

Assumption 3. An electoral prospect is a three-dimensional continuous random variable $\tilde{e}:=(\tilde{e}_0,\,\tilde{e}_1,\,\tilde{e}_2)$ drawn from a distribution $H_{\tilde{e}}(e)$ with $h_{\tilde{e}}(e_0,\,e_1,\,e_2)>0$ almost surely, such that

$$E:=\sum_{i}e_{i}>\sum_{i}s_{i}+\sum_{i\neq i}g_{\{i,i'\}}(T), \text{ with } E<\infty.$$

Electoral prospects occur according to a Poisson process with arrival rate δ .

These assumptions capture the stochastic environment proposed by Browne, Frendeis, and Gleiber, and thus formally embed Lupia and Strøm's model in this framework. The second part of Assumption 3 captures the idea that events are about electoral prospects and, consequently, future office benefits. If electoral competition corresponds to a constant sum game (as most of the formal literature assumes), then an event is an element from a simplex whose size depends on the

magnitude of future office benefits. We assume that this sum is large enough to avoid unnecessary case distinctions.²

Our last assumption concerns the status of the initial cabinet (i.e., before the first event occurs).

Assumption 4. The initial governing coalition constitutes a Nash equilibrium. That is, $cg_1^2(T) \ge g_1^0(T) - k_1$ and $(1-c)g_2^0(T) \ge g_2^0(T) - k_2$.

Assumption 4 follows if we assume that the initial cabinet constitutes a Nash equilibrium of some (unmodeled) cabinet formation game. For our purposes, this means that in the absence of an event in the first period, each member of the governing coalition in place weakly prefers the current coalition to any other feasible coalition.

Lupia and Strøm identify necessary and sufficient conditions, denoted A-C, for a potentially critical event to lead to dissolution or replacement. Using our modified model we can rewrite these conditions as:

(A)
$$\exists S \subseteq \{1, 2, 0\}: \sum_{i \in S} s_i > 0.5 \text{ and } \forall i \in S: e_i$$

$$> u_{\iota}(\tau)(s_{\iota}, c, g_{\{1,2\}}(\tau))$$

(B)
$$\forall i \in \{1, 2\} : e_i > \max_{j \neq i} [s_i + s_j + g_{\{i,j\}}(\tau) - e_j - k]$$

(C)
$$\forall \tau : cg_{\{1,2\}}(\tau) \ge g_{\{0,1\}}(\tau) - k$$
 and
$$(1-c)g_{\{1,2\}}(\tau) \ge g_{\{0,2\}}(\tau) - k$$

Intuitively, condition A states that there is some decisive coalition that prefers an election over the current government. Condition B states that both members of the governing coalition prefer an election over the best offer they can receive in the bargaining process.³

Note that assumption 4 implies that condition C holds for $\tau = T$. But by assumption 2, condition C must also be true for all τ . Hence, condition C plays no role in the characterization of dissolutions and replacements. Therefore, the characterization of the possible outcomes of the bargaining process can easily be summarized as follows.

- A is TRUE and B is TRUE ⇒ Dissolution.
- A is TRUE and B is FALSE ⇒ Replacement.
- A is FALSE and B is TRUE ⇒ Status Quo.
- A is FALSE and B is FALSE ⇒ Status Quo.

Whether termination occurs depends only on condition A. This allows us to draw conclusions about the hazard rate of pooled cabinet terminations independent of the specific type of termination. Given Lupia and Strøm's assumed utility function, $e_i > u_i(\tau)(s_i, c, g_{\{1,2\}}(\tau))$ corresponds to one of the following inequalities:

¹ Note that we count time "backward."

² Assumption 3 does not preclude the possibility that a party expects to form a single-party majority government. In the Lupia and Strøm framework no party has a majority, so their model would not apply to that (new) single-party government.

³ Given subgame perfection, the ε-bound used by Lupia and Strøm in condition C is not necessary.

⁴ This formulation suggests how the Lupia and Strøm result could be generalized to more than three parties. For condition A to be true,

(i)
$$e_1 > s_1 + c \cdot g_{\{1,2\}}(\tau)$$
, (ii) $e_2 > s_2 + (1 - c)$
 $\cdot g_{\{1,2\}}(\tau)$, and (iii) $e_0 > s_0$.

Which inequality must be true in order to satisfy condition A depends on the set of decisive coalitions implied by the seat distribution. In the Lupia and Strøm model, any decisive coalition must have at least two members. That is, for condition A to hold, at least two of the inequalities must be true. Since this is also sufficient for a termination, we can without loss of generality focus on the coalitions with exactly two members.

Let $\lambda \epsilon(\tau)$ denote the probability that an incumbent government terminates in the small interval $(\tau, \tau + \epsilon)$. That is,

$$\lambda \varepsilon(\tau) := \Pr(\tau < \tilde{\tau} \le \tau + \varepsilon | \tilde{\tau} \ge \tau).$$

By assumption 3 this probability can be expressed as the product of two terms. The first is simply the probability that an event will occur in an ε interval. Given our Poisson assumption, this probability equals $\varepsilon \delta + o(\varepsilon)$, where $o(\varepsilon)$ is the probability of receiving more than one offer in a small time interval, with $\lim_{\varepsilon \to 0} o(\varepsilon)/\varepsilon = 0$. The second is the probability that condition A is true, that is, the probability that ε satisfies at least two of the inequalities. Intuitively, this captures the probability that an event leads to a cabinet termination. We denote this probability by $\pi(s, c, g_{\{\iota, \iota'\}}(\tau))$. We then have

$$\lambda \varepsilon(\tau) = (\varepsilon \delta + o(\varepsilon)) \cdot \pi(s, c, g_{(s, r)}(\tau)).$$

Using this formula we can easily identify the similarities and differences between the nonstrategic model of Browne, Fendreis, and Gleiber and the bargaining model proposed by Lupia and Strøm. In both cases events occur with the same probability and are drawn from the same distribution. The key difference lies in the probability with which an event becomes critical. In the Browne, Frendreis, and Gleiber approach, $\pi(s, c, g_{\{l,i'\}}(\tau))$ simply is a constant. That is, each event leads to a termination with a certain fixed probability. In the Lupia and Strøm model, $\pi(s, c, g_{\{l,i'\}}(\tau))$ may in general change over time.

To derive testable implications for the Lupia and Strøm model, we need to derive instantaneous termination probabilities or hazard rates. For pooled terminations we have

$$\lambda(\tau) := \lim_{\varepsilon \to 0} \frac{(\varepsilon \delta + o(\varepsilon)) \cdot \pi(s, c, g_{(\omega)}(\tau))}{\varepsilon}$$
$$= \delta \pi(s, c, g_{(\omega)}(\tau)).$$

The behavior of the hazard rate thus depends on the behavior of $\pi(s, c, g_{\{i,i'\}}(\tau))$. We can show that $\pi(s, c, g_{\{i,i'\}}(\tau))$ is strictly monotonically increasing for de-

creasing τ , which leads immediately to the following proposition.

Proposition 1. Hazard rates for pooled terminations are strictly monotonically increasing as the time left until the next regularly scheduled election decreases.⁵

The Lupia and Strøm model thus has implications that are qualitatively different from the nonstrategic approach of Browne, Frendreis, and Gleiber. According to the latter, we expect constant hazard rates. According to the former, hazard rates will increase as the next mandated election approaches.

It is worthwhile to point out that proposition 1 holds for any constant distribution of events $H_s(e)$ with $h_s(e)$ > 0 almost surely. Since events are interpreted as electoral expectations, any assumption about the distribution of events would translate into substantive assumptions about the nature of electoral competition in multiparty democracies. For example, in a model of elections in which incumbents loose support over time, one would need to specify a series of functional forms for $H_{\mathfrak{g}}(e)$ (with an appropriate time subscript) in which the probability of a good draw for an incumbent is monotonically decreasing. In other words, the model points out that any parametric or semiparametric specification of cabinet duration implicitly presupposes a particular model of electoral competition in multiparty democracies. Proposition 1, however, holds for any such model.

The intuition for proposition 1 is conveyed in Figure 1. Given assumption 3, the event space constitutes a two-dimensional simplex, the equilateral triangle in Figure 1. Each point corresponds to a realization of the electoral prospects of the two governing parties, e_1 and e_2 . Note that condition A imposes a set of linear constraints on this sample space. If an event in the dotted region occurs, condition A is satisfied, and the cabinet terminates. Otherwise, the status quo persists.

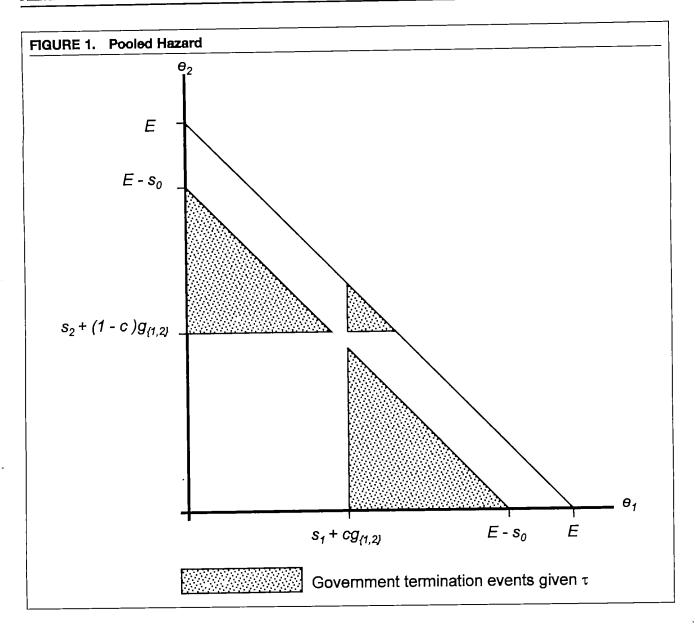
As the elapsed time until the next regularly scheduled election decreases, however, the horizontal line at $s_2 + (1-c)g_{\{1,2\}}$ drops, and the vertical line $s_1 + cg_{\{1,2\}}$ moves to the left, as shown in Figure 2. The new sample space (indicated by the gray area) thus includes the old (dotted) sample space. Hence, governments are more likely to terminate as the time until the next regularly scheduled election decreases. This result holds for any distribution of events.

CALLING ELECTIONS VERSUS REPLACING THE CABINET

The bargaining approach has implications for the probability that a dissolution or a replacement will occur. That is, we can draw inferences about termination-specific hazard rates. The random variable that a government terminates at time $\tilde{\tau}$ by dissolving the chamber and calling a new election is denoted by $\tilde{\tau}_d$, and the random variable that the government terminates and at this time is replaced by a new one, without

a decisive coalition must prefer the outside option. This is critical in making sure that the threat to call early elections is credible. Note that in the three-party model every coalition is decisive. This simplifies the analysis.

 $^{^{5}\,\}mathrm{All}$ proofs are omitted due to space limitations and are available from the authors.



an election, is denoted by $\tilde{\tau}_r$, with j=d, r. Note that the realized duration of a cabinet corresponds to $\tau=\min_{r} \{\tau_d, \tau_r\}$.

Let $\lambda_j \varepsilon(\tau)$ denote the probability that an incumbent government ends according to termination mode j in the small interval $(\tau, \tau + \varepsilon)$. That is,

$$\lambda_{j}\epsilon(\tau):=\text{Pr}(\tau<\tilde{\tau}\leq\tau+\epsilon\ \&\ \tilde{\tau}=\min_{j}\ \{\tilde{\tau}_{\textit{d}},\ \tilde{\tau}_{\textit{r}}\}\big|\tilde{\tau}\geq\tau).$$

We can then derive termination-specific probabilities. We denote the probability that both condition A and condition B are true (and the cabinet terminates in a dissolution) by $\pi_d(s, c, k, g_{\{i,i'\}}(\tau))$; the probability that condition A is true but not condition B (which corresponds to a replacement) is denoted by $\pi_r(s, c, k, g_{\{i,i'\}}(\tau))$. Then,

$$\lambda_{r} \varepsilon(\tau) = (\varepsilon \delta + o(\varepsilon)) \cdot \pi_{r}(s, c, k, g_{(r,r')}(\tau)).$$

Finally, we can derive risk-specific hazard rates as

$$\lambda_{j}(\tau) := \lim_{\varepsilon \to 0} \frac{(\varepsilon \delta + o(\varepsilon)) \cdot \pi_{j}(s, c, k, g_{\{\xi, t'\}}(\tau))}{\varepsilon}$$
$$= \delta \pi_{j}(s, c, k, g_{\{\xi, t'\}}(\tau)).$$

Note that $\pi(s, c, g_{\{l,l'\}}(\tau)) = \pi_r(s, c, k, g_{\{l,l'\}}(\tau)) + \pi_d(s, c, k, g_{\{l,l'\}}(\tau))$ and $\lambda(\tau) = \lambda_r(\tau) + \lambda_d(\tau)$. That is, termination-specific hazards are conditionally independent.

The Browne, Frendreis, and Gleiber model predicts no qualitative difference among termination modes. Moreover, the termination-specific hazard rates will be constant. This is not true in the Lupia and Strøm model, as indicated by the following proposition.

PROPOSITION 2. Hazard rates for dissolutions are strictly monotonically increasing as the time left until the next regularly scheduled election decreases.

 $^{^6}$ It is obvious from condition B that the termination-specific probabilities depend also on k.

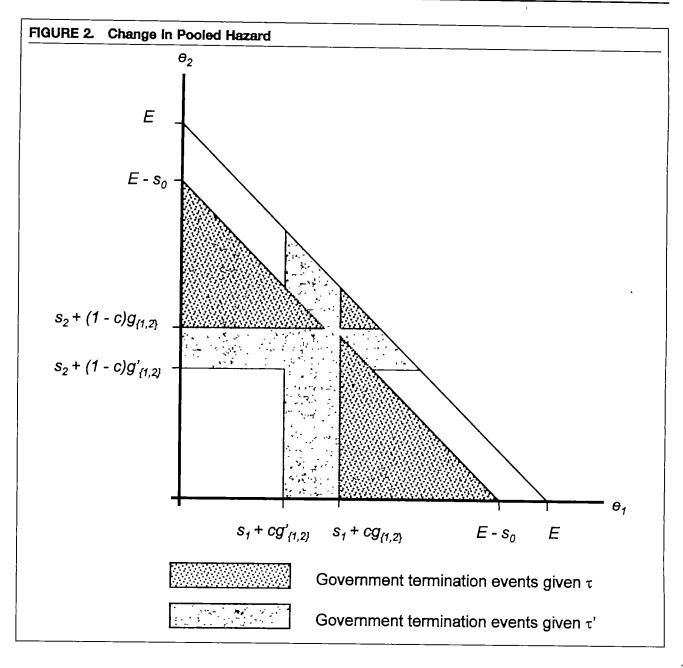
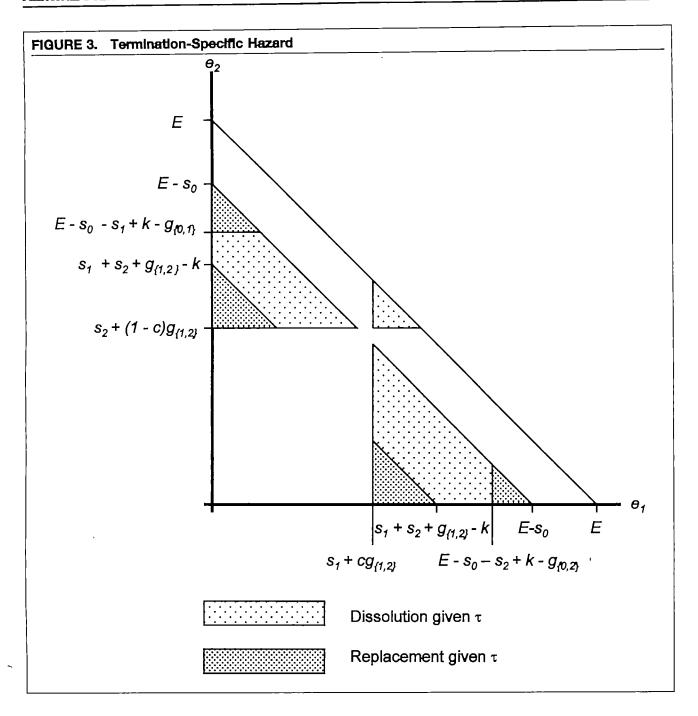


Figure 3 provides the intuition for the argument. The linear constraints associated with condition B now divide the termination region into two subregions. An event located in the lightly dotted region corresponds to a dissolution. An event located in the densely dotted region leads to a replacement. Otherwise, the status quo prevails. As in the discussion of proposition 1, the key to proposition 2 is the movement of the linear constraints.

As shown in Figure 4, five lines move simultaneously as τ becomes smaller. The two lines at $s_2 + (1 - c)g_{\{1,2\}}$ and $s_1 + cg_{\{1,2\}}$ move as in proposition 1. The upper horizontal line moves up, the line on the extreme right moves to the right, and the diagonal line moves toward the origin. Hence, as in proposition 1, the new sample space (indicated by gray) includes the old sample space leading to increasing hazard rates. In other words, there are no white (lightly) dotted regions.

Can we draw similar conclusions about replacement hazards? The answer is "no." From Figure 5 we can see that, in contrast to propositions 1 and 2, the sample space at $\tau' < \tau$ is not a subspace of the event space at τ . Hence, the shape of the replacement-specific hazard depends in general on the distribution of events, $H_{\delta}(e)$. Without an assumption about $H_{\delta}(e)$, we cannot predict the shape of replacement hazards.

There is an important implication for the comparison with the Browne, Frendreis, and Gleiber model. According to that approach, termination-specific hazard rates will not be qualitatively different. Indeed, they will be constant, although not necessarily at the same rate. This is not true in the Lupia and Strøm model, which predicts increasing hazard rates for dissolution (proposition 2) but replacement hazard rates of any shape, depending on the functional form of $H_{\delta}(e)$. An empirical finding of qualitatively different hazard rates

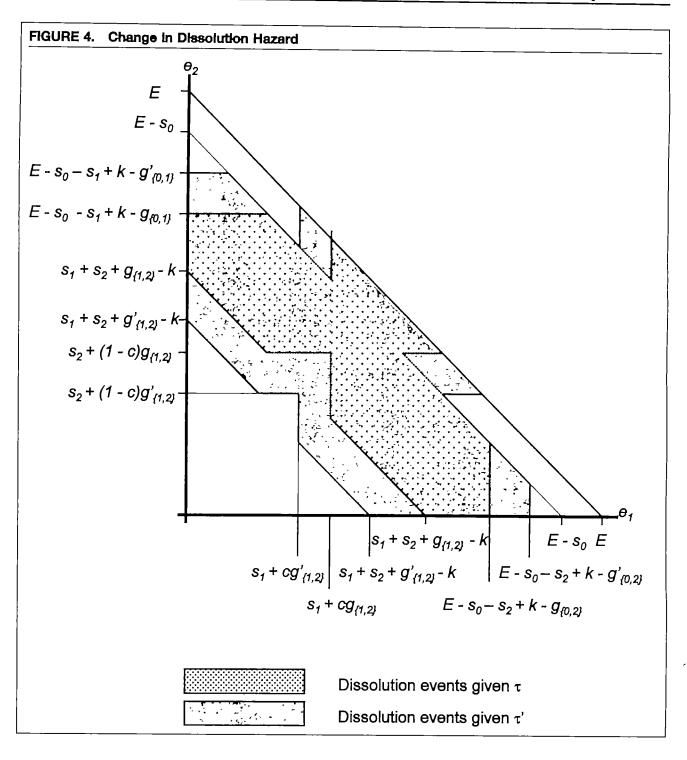


for the two termination types would thus be inconsistent with the Browne, Frendreis, and Gleiber model but would confirm the Lupia and Strøm model, albeit in a rather weak form.

FROM MODELS TO DATA

One advantage of specifying stochastic models of cabinet termination is the close connection between the theoretical output of the models (formal equations describing the shape of the predicted hazard rate for different modes of termination) and the empirical data used in testing (hazard rates calculated from actual

terminations). Even with this close connection, however, some problems remain. One concerns case selection. The data we use to evaluate our theoretical propositions are from King et al. (1990) and Warwick (1992), who include all the postwar cabinets of Austria, Belgium, Canada, Denmark, Finland, France (Fourth Republic), West Germany, Iceland, Ireland, Israel, Italy, the Netherlands, Portugal, Spain, and the United Kingdom. Note, however, that the Lupia and Strøm model does not apply to Norway and Sweden. The Norwegian constitution does not allow for an early chamber dissolution under any circumstances. In Sweden a chamber may be dissolved at any

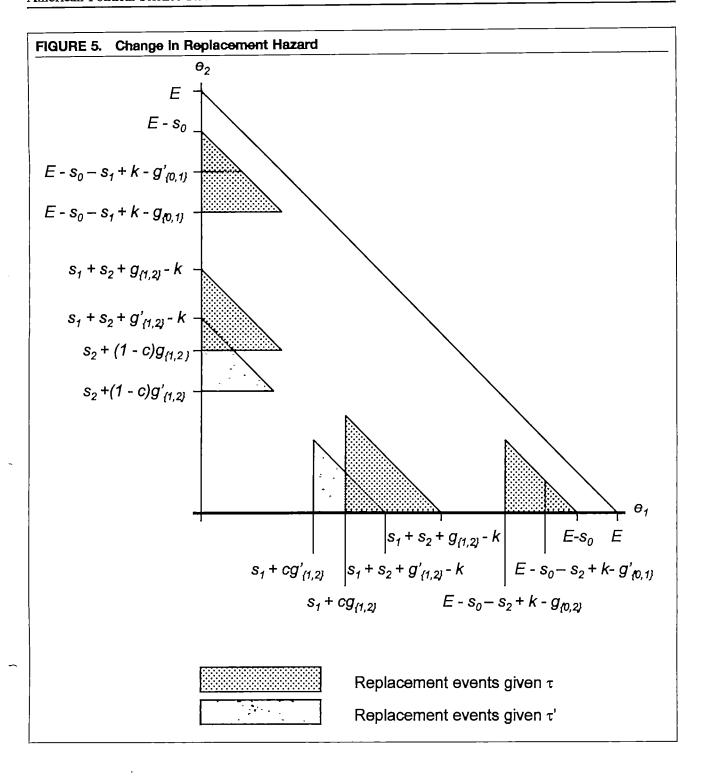


time, but the new parliament may only serve the remainder of the constitutionally mandated interelection period. In our model, this means that in addition to $g_i^{r}(\tau)$, E would change over time as well, which violates a key assumption.

Another problem is a special type of cabinet sometimes included in duration studies. We do not include caretaker cabinets in our data set. These are normally given only very limited powers, work on a limited agenda, and are often formed in anticipation of an upcoming election. More difficult issues are single-party majority cabinets and minority cabinets. Lupia

and Strøm assume that the government is composed of (at least) two parties, which excludes single-party majority cabinets. Consequently, in the empirical analysis we did not include these cases. Whether minority governments should be retained in the data set depends on whether membership in government is defined by which parties are included in the cabinet. Rather than taking a stance on this definitional issue, we performed empirical analyses with and without minority governments.⁷

⁷ If Lupia and Strøm were to identify their share of power index, c,



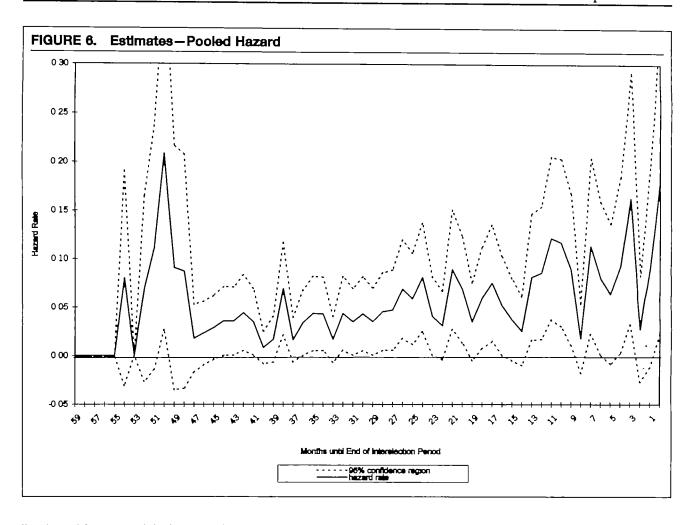
Finally, there is the issue of censoring.⁸ Some cases of censoring are uncontroversial, for example, when a

and/or the value of the cabinet to the parties, g, with the number of portfolios a party holds, then minority cabinets would correspond to the case in which some member of a majority coalition receives a c and/or g equal to zero. In this case, the propositions in the text do not hold, so these types of cabinets ought to be excluded from the empirical analysis. But Lupia and Strøm (1995, 663n) explicitly reject such a restrictive interpretation of c and g. These terms may also include noncabinet posts as well as policy concessions. In this case the propositions would apply to minority cabinets as well.

* Censoring, or more precisely "right censoring," is used when cases

cabinet terminates because the prime minister dies. King et al. (1990) argue, however, that cabinet terminations within 12 months of a mandated election also should be censored because cabinet parties, knowing that they must call an election within a year, may strategically decide to hold early elections if circumstances are advantageous (e.g., high opinion poll ratings). This sort of strategic behavior is exactly what the

fall out of the sample for reasons other than "failure," as defined by the researcher (Kalbflelsch and Prentice 1980).



Lupia and Strøm model tries to explain—not just as the election draws near but over the whole tenure of a cabinet. Censoring these cases would remove exactly the cases most likely to terminate due to dissolution. Therefore, we censor only cabinets that fall for technical reasons (such as the death of a prime minister) and those that actually reach the very end of the CIEP.

ESTIMATION AND RESULTS

The main empirical predictions of our stochastic formulation of the Lupia and Strøm model are given in propositions 1 and 2, which predict that both pooled and dissolution hazards will be strictly monotonically increasing. In contrast, the shape of the hazard for replacements cannot be predicted. Since our results hold for any distribution, we use nonparametric estimation methods.⁹

Figures 6-8 plot our estimate of the hazard function for pooled failures, replacements, and dissolutions, respectively. Each figure also includes 95% confi-

dence bounds (Kalbfleisch and Prentice 1980, 13–4).¹¹ It is important to remember that the time axis in these figures represents months until the end of the CIEP, not cabinet duration in elapsed time.

The first thing to notice about these three figures is that they seem to refute the main prediction of the Browne, Frendreis, and Gleiber model—that hazard rates are constant. Three different nonparametric tests (i.e., the Anderson-Darling, Cramer—von Mises, and Kolmogorov-Smirnov statistics) confirm the statistical significance of this result. Each of these statistics test the null hypothesis that two empirical distribution functions are the same, and each rejects the null hypothesis that the hazard rates in figures 6 and 7 are constant (at p < .05).

A second problem for Browne, Frendreis, and Gleiber's nonstrategic model is that the dissolution and replacement hazards are significantly different. All three of the nonparametric tests reject (p < .01) the null hypothesis that the dissolution and replacement

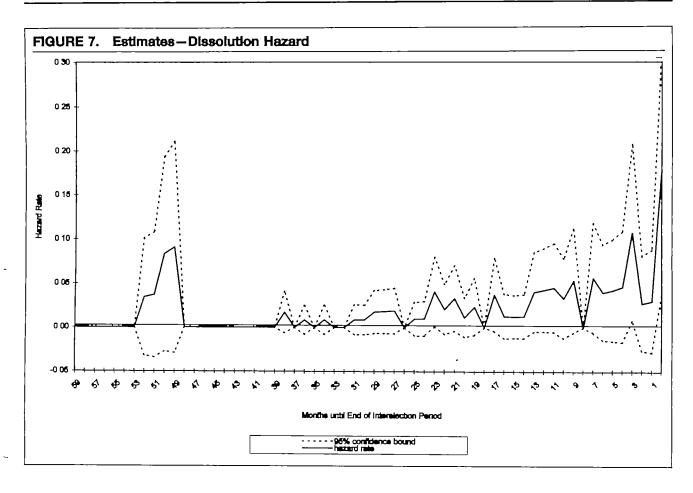
⁹ All our estimates were produced using Gauss. The programs are available, along with data and documentation, upon request.

¹⁰ Our rates are calculated following Meyer (1990) and are a variation on standard Kaplan-Meier estimates, with the time axis modified as discussed in the text. If the number of cabinets at risk of failure in any period is R_n and the number that fail in period t by failure type t are d_n then the hazard rate is simply d_s/R_n . Censoring is accommodated in the estimate of the hazard rate by calculating the

risk set at time t as $R_t = R_{t-1} - d_{t-1} - C_t$, where C_t is the number of cases censored in period t (Kalbfleisch and Prentice 1980).

11 The Kanlan and Meier technique for estimating hazard rates (and

¹¹ The Kaplan and Meier technique for estimating hazard rates (and Meyer's modification, see note 10) is readily generalizable to include different termination types (see Kalbfleisch and Prentice 1980, 168–9). For the case of independent failure risks, one simply calculates the hazard rate for failure type *i* by treating failures of type *j* as censored in the period in which they occurred.



hazard rates are the same. 12 This result is inconsistent with the nonstrategic model but perfectly consistent with a strategic one, since the Lupia and Strøm model is agnostic about the shape of the replacement hazard function. The estimates presented in Figure 8, however, suggest that cabinets face a constant threat of replacement, even as they approach the end of the CIEP.¹³ This is consistent with Alt and King (1994), who found constant hazard rates when they censored cabinet terminations within 12 months of the end of the CIEP.14 In addition to this insight, however, the estimates in Figure 8 show that the hazard rate for bargaining failures remains constant even when the CIEP is drawing near, which Alt and King could not show because of their choice of censoring regime, because they did not distinguish between cabinet replacements and legislative dissolutions, and because

¹² This test excludes the left "hump" (created by cabinets that terminate within a year after the last regularly scheduled election in countries with a 60-month CIEP; see the text discussion below). When it is included, only one of the three tests rejects the null hypothesis.

¹⁵ The three statistical tests referred to above do not let us reject (p < .05) the null hypothesis of a constant hazard rate for replacement over the range of 1-40 months until the end of the CIEP. Furthermore, for the full range, only one of the tests rejects the null of constancy.

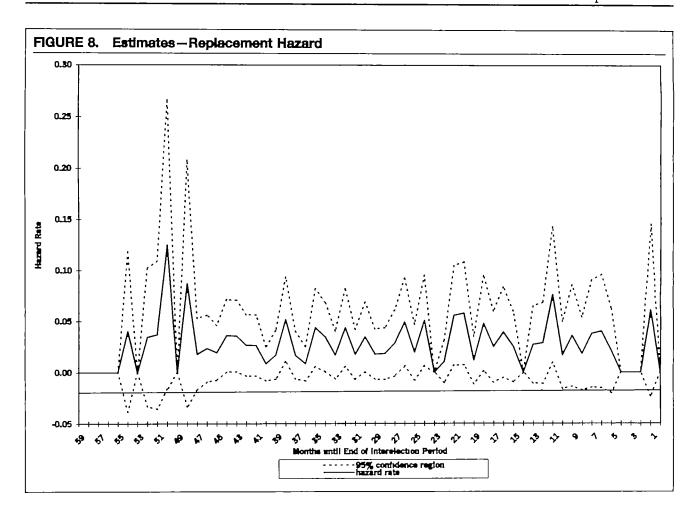
¹⁴ The comparison with Alt and King (1994) is appropriate because the hazard is constant. If the underlying hazard is truly constant, this should be revealed both in traditional hazard rates, calculated with respect to the elapsed age of the cabinet (which is what Alt and King do), and in the type of hazard rates in Figure 8.

they worked in elapsed time instead of time until the end of the CIEP.

For the Lupia and Strøm model, figures 6–8 contain both good and bad news. The good news is that after the initial "hump" (created by cabinets that terminate within 12 months after the last election), rates increase both for pooled and dissolution hazards, which confirms propositions 1 and 2. Moreover, replacement hazards seem to follow a distribution different from that of dissolution hazards, which is consistent with the Lupia and Strøm model. The bad news is that only in the case of pooled hazards is this increase strictly monotonic. Dissolution hazards are flat until about 24 months before the end of the CIEP. Moreover, there is the nonmonotonicity due to the initial hump. That is, the risk of cabinet failure is significantly higher in the first year after an election (in systems with 60-month CIEPs) than in the next several years.

The estimated hazard rates given in figures 6-8 were calculated with data that exclude single-party majority cabinets, caretaker cabinets, and cabinets from Sweden and Norway. The remaining data set is the one to which the theoretical model applies most clearly. A possible problem, however, is the model's ambiguous treatment of minority cabinets, as explained earlier. It is thus worthwhile to examine estimated hazard rates when minority cabinets are excluded from the sample in order to access the degree to which our results rely on this distinction.

Reproducing figures 6 and 8 for minority and ma-



jority cabinets separately does little to change the hazard shapes or our conclusions for the pooled sample and for replacements. For dissolutions, however, the hazard shapes for majority and minority cabinets differ from one another and from the graph in Figure 7. Figures 9 and 10 give the estimates for majority and minority cabinets, respectively. Note that the spike in Figure 7 at about 48 months until the end of the CIEP is due entirely to minority cabinets. What could account for this result? Looking more closely at the specific cases, we find that most are rather unusual in that they are "almost majority" cases. That is, they

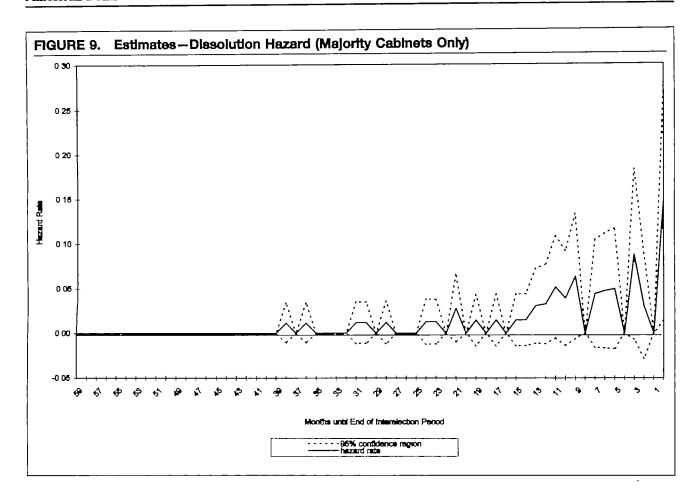
¹⁵ Plots of hazard rates are available from the authors. The claim in the text concerning minority government dissolutions refers to the point estimates. With only 31 cases of minority cabinet dissolutions out of 81 minority governments, strong conclusions about the hazard shape cannot be made.

16 Specifically, they are cabinets for Canada (three cases), Ireland (two cases), and the United Kingdom (one case). In the British case, the election of February 1974 did not produce, for the first time since the 1930s, a single-party majority in the legislature, a situation that quickly changed in October, when Labour won 50.2% of the seats. For Canada, two of the cases are single-party minority Conservative Party cabinets. The first followed the June 1957 election, in which the Conservatives and Liberals polled 42.3% and 39.6% of the vote, respectively. This was the first time since 1925 that one of these two parties did not have a legislative majority. In the next election, held in March 1958, the Conservatives won an astounding 78.5% of the seats. The second Conservative cabinet followed the June 1962 election, in which the two main parties again won substantially fewer seats than the number needed for a majority (43.8% and 37.7%). An

occur in countries in which elections almost always produce single-party majority governments, but sometimes the result is a "hung parliament," e.g., due to a bad electoral draw on polling day. In this case the minority cabinet may choose to go back to the electorate early, anticipating that the bad draw will not be repeated. Of course, in some systems a cabinet cannot dissolve the government and call new elections without a majority vote in parliament, so the support of some opposition parties is needed. When would they get such support? Most likely when there are two large parties that both lost seats to a third smaller party but expect to regain seats if a new election is held immediately (which is a good description of several of the actual cases, as described in note 16).

Figures 9 and 10 also lead us to qualify our early conclusion regarding support for proposition 2. On the one hand, Figure 9, although similar to the right side of Figure 7, does not show even the slight increase in the hazard rate before about 18 months that was apparent in Figure 7. Instead, the hazard is quite flat until, at 18

election in April 1963 gave the Liberals 48.7% and began to establish a new pattern in Canadian politics in which the Liberals, just short of a majority, would rule as a single-party minority cabinet. (It is interesting that one of the other short-lived Canadian cabinets, 17 months, came after an election in 1972 that once again gave the two major parties about 40% of the vote apiece.)



months, it starts to increase quickly.¹⁷ On the other hand, the case of minority governments portrayed in Figure 10 suggests a much longer increase than for majority governments. This conclusion needs to be qualified due to the limited number of cases, but it suggests that, surprisingly, the Lupia and Strøm model is most clearly confirmed in the case of minority governments.

We concluded our empirical analysis with a robustness check. Specifically, we needed to make sure that the introduction of further covariates did not alter the conclusions already drawn. To do this we analyzed the data after stratifying the sample according to various independent variables commonly found in the literature. We then reestimated the pooled and separate survival curves for each stratum and again used three nonparametric tests to evaluate our main hypotheses for each of the strata. In general, the results confirmed previous findings.¹⁸

CONCLUSION

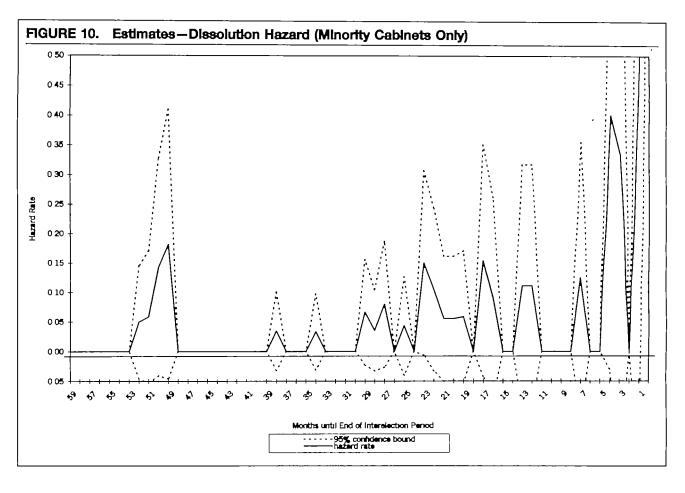
Lupia and Strøm's model of cabinet termination provides a strategic interpretation of critical events

(Browne, Frendreis, and Gleiber 1986), the fundamental theoretical concept in the study of cabinet duration. Whereas Browne, Frendreis, and Gleiber assume that the stochastic process governing the arrival of events determines the distribution of failures directly (i.e., all events are critical), Lupia and Strøm assume that a strategic bargaining process moderates between the arrival of events and the observed distribution of cabinet failures.

We assessed these different theoretical positions by first presenting a stochastic version of the Lupia and Strøm model that allows us to compare its predictions directly to those of Browne, Frendreis, and Gleiber. Furthermore, the stochastic model leads to some new implications about the shape of cabinet termination hazards. We then tested these implications using a sample of postwar cabinets. The results largely reject Browne, Frendreis, and Gleiber's nonstrategic approach but lend only qualified support to the Lupia and Strøm strategic model. We found evidence that the hazard rate for pooled terminations and dissolutions increases as a cabinet approaches the end of the constitutional interelection period, but this function increases monotonically only toward the end of the CIEP. The model works better in the case of minority governments, but it is questionable whether the Lupia and Strøm framework applies to these governments at all. Finally, the Lupia and Strøm model makes no clear prediction about hazard rates due to replacement and

¹⁷ This finding is independent of Alt and King's (1994) finding of flat hazard rates. First, our finding pertains to dissolutions only, a type of termination mostly censored by Alt and King. Second, they analyze hazard rates in clapsed time

 $^{^{18}}$ The programs and data that generated these results are available upon request.



provides no account of cabinet terminations in Norway or Sweden.

On balance, the introduction of a strategic approach seems highly promising, but a model that is explicitly dynamic and incorporates a richer set of institutional variables would be highly desirable. At the very least, it should differentiate between majority and minority cabinets. In general, stochastic bargaining models provide a way to combine strategic models with rigorous empirical analysis. Despite the added complexity of such an effort, our analysis suggests that the alternative—ignoring the bargaining process that intercedes between events and outcomes—would lead to incorrect predictions about the distribution of cabinet failures.

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The Dynamics of Collective Deliberation in the 1996 Election: Campaign Effects on Accessibility, Certainty, and Accuracy

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residential election campaign. In order for communication to be effective, messages conveyed through social interaction must be unambiguous, and the recipient must readily, confidently, and accurately perceive the intent of the sender. We address a number of factors that may influence communication effectiveness: the accessibility and extremity of political preferences, the distribution of preferences in the surrounding environment, disagreement between the senders and receivers of political messages, and the dynamic of the election campaign. The analysis is based on a study of the 1996 campaign, which interviewed citizens and discussion partners between March 1996 and January 1997. The citizens are a random sample of registered voters in the Indianapolis and St. Louis areas, and these registered voters identified the discussion partners as people with whom they discuss either "government, elections, and politics" or "important matters."

his article examines the dynamics of social communication among citizens during a presidential election. We are particularly interested in the campaign as an institution that stimulates effective communication among citizens, as well as the characteristics of individuals that lead to effectiveness in both the transmission and receipt of political messages (Berelson, Lazarsfeld, and McPhee 1954). During a campaign, the citizenry is engaged simultaneously as both senders and receivers of political information, and a primary issue is what factors contribute to clarity in both the transmission and receipt of messages. In other words, in the collective deliberations of democratic politics, who are the good listeners, and who are the good talkers?

The analysis focuses on the factors that enhance and impede effective and unambiguous political communication among citizens. What is the influence of the strength and extremity of political preferences held by the senders and receivers of political messages? Is the effectiveness of political communication compromised by disagreement between the senders and receivers? Does it depend on the distribution of preferences in the receiver's surrounding environment? Finally, does the effectiveness of political communication respond to the changing political-temporal context of a presidential campaign? We draw upon a study of political communication during the 1996 election, and we begin with a review of the conditions that may influence communication effectiveness.

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Support was received through a grant to Indiana University from the Political Science Program of the National Science Foundation. Data for this 1996 election study are available from the Inter-University Consortium for Political and Social Research (Study #2962).

CLARITY, AMBIGUITY, AND DISTORTION IN POLITICAL COMMUNICATION

What factors contribute to clarity, ambiguity, and distortion in socially communicated political information? First, the strength of individual preferences may influence the effectiveness of communication. People with strong attitudes should send clearer messages than those with weak attitudes (Latane 1981; also see Fazio 1990), and strong opinions held by recipients may impede effective communication due to conflict avoidance and selective perception (Festinger 1957; MacKuen 1990).

Second, the more frequently one person is exposed to the preferences of another, the more likely it is that distortion will be reduced and clarity enhanced (Latane 1981, 334). Some people are regular companions, and others are frequent and trusted sources of political information (Downs 1957; Finifter 1974). Moreover, and perhaps more important, a year-long election campaign creates an accumulation of frequent opportunities for political exchange.

Third, people may be less likely to recognize political preferences other than their own. Huckfeldt and Sprague (1995) and Huckfeldt et al. (1998a) demonstrate that judgments regarding the preferences of particular associates are biased by one's own viewpoint, that is, people tend to infer that associates hold opinions similar to their own. Thus, political messages are less likely to be perceived accurately when there is disagreement between senders and receivers.

Fourth, to the extent that people form political expectations based on patterns of social interaction, distortion is more likely when the sender's preference is less common within the receiver's total social network. Individuals generalize on the basis of the environment they know best, the environment they experi-

¹ We see a dyadic process similar to the "false consensus effect," whereby people project their beliefs onto their perceptions of aggregate behavior (Fabrigar and Krosnick 1995; Granberg 1987).

ence through their own patterns of social interaction (Huckfeldt and Sprague 1995). If Joe thinks that most of the people he knows are Clinton supporters, then he is likely to miss the fact that one of them actually supports Dole.

Fifth, the campaign itself may influence the effectiveness of political communication. Exactly how this occurs is problematic, due to campaign effects that may run in opposite directions, but the campaign may clarify political choices, encourage individuals to develop opinions on the candidates, and focus patterns of collective deliberation. Thus, to the extent that the development of more strongly held opinions encourages the more effective communication of preferences, campaigns should stimulate effective communication. Yet, people with strongly held opinions may be less likely to recognize the opinions expressed by others, either because they selectively perceive the message or because their strong opinions discourage the expression of disagreement by others. Thus, to the extent that strongly held opinions obscure or inhibit free and effective communication, campaigns may diminish communication effectiveness by encouraging people to develop the strong views that limit the free flow of political discourse.

DATA AND RESEARCH DESIGN

We address these issues with interview data from a 1996 election study conducted by the Center for Survey Research at Indiana University between early March 1996 and early January 1997. The sample of main respondents (N = 2,174) was drawn from lists of registered voters, and a one-stage snowball sample of discussants (N = 1,475) was obtained from these main respondents. Main respondent samples are drawn from the voter registration lists of two study sites: (1) the Indianapolis metropolitan area, defined as Marion County, Indiana; and (2) the St. Louis metropolitan area, defined as the City of St. Louis and the surrounding (mostly suburban) St. Louis County, Missouri. The data set for this analysis is based on the 1,475 dyads formed between an interviewed main respondent and an interviewed discussant, but we include only dyads for main respondents who name two or more discussants. (Response rates and other details are given in Appendix A.)

Main respondents were asked to provide the first name of not more than five discussion partners. As Appendix B documents, a random half of the sample was asked to name people with whom they discuss "important matters"; the other half was asked to name people with whom they discuss "government, elections, and politics" (Burt 1986; Huckfeldt and Sprague 1995; Huckfeldt et al. 1998a). The experimental condition embedded within the study design allows us to examine the extent to which political information networks are separate from social communication networks more broadly considered. By treating the question wording as a randomly applied experimental stimulus, we are able to address whether the "political" discussants are different from the "important matters" discussants and

whether these differences have consequences for the effectiveness of political communication.

Interviewers then asked a battery of questions about each discussant in the sequential order in which the discussants were named. Next, respondents were asked to identify the presidential candidate supported by each discussant, but the order in which this question was asked about each particular discussant was varied across main respondents. Immediately after asking the question about each particular discussant, the interviewer used a key stroke to start a computer clock. As soon as the respondent answered, the interviewer used a second key stroke to stop the clock before recording the answer. The Computer-Assisted Telephone Interview (CATI) system then asked the interviewer whether the timing was successful,2 and the interviewer asked the respondent a follow-up question: "How difficult or easy was it to say how [discussant's first name] voted?" At the end of the session, main respondents were asked for identifying information to use in contacting and interviewing their discussants. The analyses in this article draw on information taken from both the main respondents and their discussants, such that each observation in the data set reflects a dyadic relationship between a main respondent and a discussant.3

ASSESSING COMMUNICATION EFFECTIVENESS

If campaigns stimulate effective communication, then citizens should become increasingly aware of their associates' preferences over the course of the campaign. They should more readily make judgments regarding the preferences of others; they should become more confident in their own ability to make these judgments; and the accuracy of their judgments should increase. Of course, even effectively communicated messages may be received unsympathetically, so effective communication is no guarantee of influence. The hallmark of effectiveness is the extent to which the message receivers are able to render accessible, confident, and accurate judgments regarding the politics of the senders. Although all individuals are both senders and receivers in the collective deliberations of democracy, we will refer to the main respondent as the "listener" or "receiver" and the discussant as the "talker" or "sender." This terminology focuses attention on the message sent by discussants and received by

² Interviewers generally reported unsuccessful timings based on their own errors, typically in situations when they stopped the timer too quickly. We encouraged interviewers to report such errors, and they were assured that these errors would not affect their performance evaluations. The median response times for the five discussants were, for the first, 1.73 seconds (n = 462); the second, 1.27 seconds (n = 325); the third, 1.18 seconds (n = 250), the fourth, 1.04 seconds (n = 104); and the fifth, 1.43 seconds (n = 83).

Some main respondents (n = 872) appear in more than one dyad, 469 appear in one dyad, 250 in two, 112 in three, 35 in four, and 6 in five. We employ a standard error correction for clustering that takes account of this fact. The correction produces only minor changes (see Rogers 1993), an unsurprising result given the very modest degree of clustering. All results were obtained with Stata, Release 6.

main respondents, which is the only context we are equipped to address with our research. We first discuss accessibility, then confidence and accuracy.

Accessibility and the Ease of Judgment

When the interviewer asked a respondent how she expected one of her discussants to vote in November, she was being asked to connect that person with a particular political preference. Often this is an easy task; many individuals are quite vocal about their strongly held preferences, and their politically attentive associates can predict their behavior quite readily. The task may be more difficult if the discussant is less forceful and decisive in political discussion, has no preference, and so on. In the first instance we might say that the respondent possesses a strong association in memory between the discussant and a particular voting behavior, and this strong association yields a highly accessible perception of discussant preferences. In the latter case, we might say that the association in memory is weak, which yields a much less accessible perception.

In summary, accessibility is defined as the strength of association between two objects in memory. The least accessible perceptions are those that must be constructed on the spot, when there is no association between the objects in memory. When political communication between two people is effective, the cognitive association made between one partner and a particular political preference should be strong in the memory of the other partner, and the cognitive accessibility of that judgment would be enhanced (Fazio 1995).

How do we know whether judgments are accessible? Pioneering work (Bassili 1993; Fazio 1995) has shown that accessibility is revealed by the time required for a response to a question or stimulus. Thus, we measured the time it took respondents to provide a judgment regarding a discussant.

Two issues related to the use of response time (also known as response latency) measures need to be addressed: the baseline speed of response and the problem of learning curves (Fazio 1990). First, some people answer questions more rapidly than others, so it is important to take this into account. We do this by establishing an individually idiosyncratic baseline speed of response to be employed as a statistical control when assessing various effects on the speed with which respondents report judgments about discussants' political preferences. Our measure is based on the mean time that is required for respondents to report their judgments regarding the political preferences of discussants in their residual networks. This residual network is defined as all the respondent's discussants, except for the discussant involved in the particular dyad being analyzed. Thus, the size of the residual network is one less than the total number of discussants identified by a respondent, and not all the discussants in the residual network were interviewed.4

Second, as people progress through a battery of questions, their speed of response accelerates due to a learning curve. This is a potential problem for our research because the order in which discussants are named may be substantively meaningful (Burt 1986; Huckfeldt et al. 1998b). We anticipated this problem by altering the order in which we asked questions about discussant vote preferences. The battery always began with a question about the first discussant's preference. When the respondent named two people, we asked about the first and then the second. When three were named, we asked about the first, third, and second. When four were named, we asked about the first, fourth, third, and second. When five were named, we asked about the first, fifth, second, fourth, and third. Although this produces a perfect correlation between the first discussant named and the first discussant about whose preference the respondent is asked, the correlation drops to -.14 for the remaining discussants, so the learning curve is largely independent of factors related to the order of identification (spouse, close friend, colleague, and so on).

Baseline Speed of Response and Related Control Variables. In the first model of Table 1, we consider a range of factors that may affect the accessibility of the main respondent's judgment about a discussant's candidate preference.5 In the first set of variables, related to the baseline speed of response, are Mean Response Time in the Residual Network and the Question Order for the Perceptions of Discussants, as well as a dummy control variable, First Named Discussant. A faster mean response time in the residual network is associated with a more rapid response time for each of the individual judgments about discussant preferences, and therefore it needs to be controlled. Moreover, the measure for question order produces a substantial effect in terms of shorter response times for judgments regarding discussant preference questions asked later in the sequence. Finally, the dummy variable does not have a statistically discernible effect, that is, response times do not differ between first-named discussants and others once we control for question sequence and mean network response time.

Nature of Relationship. In the second set of factors are variables that concern the relationship between the main respondent and the discussant. Three dummy variables summarize the closeness of that relationship: Spouse, Another Relative, or Close Friend. (The excluded baseline category is a nonrelative who is not a close friend.) As the results show, these variables produce effects in the expected direction, but the standard errors for the coefficients are relatively large, which suggests statistically indiscernible effects.

⁴ Respondents who identified only one discussant were excluded from the analysis. For those who named only two discussants, the

baseline speed is based on a single response time. Response time data are typically akewed, with an extended tail of slow response times. Thus, in constructing a measure for the baseline speed of response, we first take the log for each time and then compute the mean of these logs.

⁵ Accessibility is measured as response time to the question, in hundredths of seconds.

Constant	Accessibility of Main Respondent's Judgment (OLS model)		Confidence of Maln Respondent's Judgment (OLS model)		3. Accuracy of Main Respondent's Judgment (logit model)	
	Coeff.	(S.E.)	Coeff.	(S.E.)	Coeff.	(S.E.)
Beseline Response Time and Other Controls	343.18**	(104.86)	.17	(.35)	4 .21**	(.79)
Mean response time in the residual network	53.67**	(12.29)				
Question order for perceptions of discussant preferences	−21.77**	(7.75)				
First-named discussant (dummy)	28.90	(25.97)				
2. Nature of Relationship						
Spouse (dummy)	-50.18	(27.70)	.37**	(.12)	.56	(.34)
Another relative (dummy)	-42.86	(23.11)	.28**	(.10)	.18	(.23)
Close friend (dummy)	-17.47	(23.43)	.11	(.10)	006	(.25)
3. Agreement Measures		` '		, ,		, ,
Agreement of main respondent with discussant in this dyad (dummy)	-13.05	(19.99)	.06	(.11)	1.00**	(.19)
Proportion of residual network that agrees with discussant	-33.31	(24.74)	.26 *	(.13)	1.91**	(.29)
4. Main Respondent Preferences					-	
Partisan extremity	-16.51	(10.90)	.097	(.055)	.12	(.11)
Candidate evaluation extremity	.26	(6.79)	.09*	(.04)	002	(80.)
Partisan accessibility (party l.d. response time)	.13**	(.04)	01	(.02)	07	(.04)
5. Discussant Preferences						
Partisan extremity	-36.70**	(12.06)	.11**	(.04)	.32**	(90.)
Candidate evaluation extremity	-11.06	(7.57)	.11**	(.03)	.29**	(80.)
Partisan accessibility (party i.d. response time)	.03	(.03)	01	(.02)	03	(.04)
8. Other Characteristics						
Imputed discussant knowledge	6.52	(17.08)	.10	(.06)	.13	(.16)
Reported discussion frequency						
with discussant	-26.74 *	(11.50)	.31**	(.05)	.23	(.12)
Political network (dummy)	-16.00	(15.91)	.16*	(.08)	.36*	(.18)
7. Campaign Dynamic						
Campalgn week	-3.44*	(1.51)	.022**	(.007)	.043**	(.016
Primary season (dummy)	−76.97*	(36.79)	.36*	(.18)	.66	(.41)
V (number of dyads) =	902		994		996	
₽ ⁸ =	.15		.18			
3 =	242		1.08			
$\chi^2/d\bar{t}/p$ -value =					227/16/00	
pseudo R ² =					.28	

644

Agreement Effects. The third set of factors is related to disagreement among the discussant, the main respondent, and the remainder of the respondent's communication network. The dummy variable Agreement in the Discussion Dyad is based on the self-reports of main respondents and discussants. Table 1 shows that agreement has no effect on the accessibility of the judgment as measured by response time. Similarly, the accessibility of the judgment does not depend on the respondent's perception regarding Proportion of the Residual Network that Agrees with the Discussant. We are interested in whether the respondent's ability to recognize a particular discussant's reported preference depends on the respondent's perception of the extent to which that preference prevails within the remainder of the network. There is no evidence here to suggest such an effect.6

Preference Effects. The fourth and fifth sets of variables cover factors related to the strength of political preferences held by main respondents and discussants. We regard extremity and accessibility as two separate elements of partisan strength (Krosnick and Petty 1995; Weisberg 1980),7 and we consider partisan extremity, the candidate evaluation extremity, and partisan accessibility for both sides of each dyad. For these purposes, Partisan Extremity refers to the distance from the midpoint on the party identification scale. Partisan Accessibility is the speed with which individuals specify their partisanship, measured as the response time for the initial response to the party identification question in hundredths of seconds.8 Candidate Evaluation Extremity is the absolute difference between the individual's rating of the two major party candidates on a five-point scale. (See Appendix B.)

How do the accessibility and extremity of partisan preferences, both for main respondents and discussants, affect the accessibility of respondent perceptions about discussant preferences? The results suggest that the characteristics of dyad members are differentially important. For respondents, only the accessibility of their party identification is associated with the accessibility of their judgment about discussants' vote preference. Respondents with accessible partisan orientations have more accessible perceptions of discussant preferences, even after we control for the baseline speed of response. In contrast, for discussants, only partisan extremity has a discernible effect: Vote pref-

erences of discussants who are extremely partisan are more readily perceived by the main respondents.

This pattern of effects is quite interesting and entirely plausible. Extreme partisans are likely to send unambiguous political messages, so their views are readily perceived. Also, people who hold highly accessible partisan loyalties are more readily able to perceive political preferences in others. Most important, there is no evidence here to support the argument that strong attitudes inhibit political communication, regardless of whether such attitudes are held by receivers or senders, and regardless of whether attitude strength is measured in terms of extremity or accessibility. In other words, we see no reason to believe that strong attitudes lead individuals to disguise, misinterpret, or misperceive political disagreement.

Other Political Characteristics. The sixth set of explanatory factors introduces several other political characteristics of the discussant into the analysis. Two variables are based on the perceptions of the main respondent: Reported Discussion Frequency with the Discussant and Imputed Discussant Knowledge. No effect is found for the latter, but as expected, more frequent discussion reduces the response time. Political Network is a dummy variable to indicate the category of discussant (political or "important matters"), but it has no discernible effect on the accessibility of discussant preferences. (For other analyses of the discussant name generator, see Huckfeldt et al. 1998b.)

Campaign Dynamics. Two variables are used to index campaign time: Campaign Week of the interview and a dummy that measures the Primary Season. The latter is defined to continue through June 30, and this variable indexes all respondents interviewed through that date. The campaign week of the interview begins (at 1) with the interviews in early March and ends (at 36) with election week. Respondents interviewed after the election are also given the maximum value. Thus, the week of the interview indexes the accumulation of opportunities for communication within networks.

During the primary season, we asked respondents whether they thought discussants would vote for a Democrat, a Republican, or an independent. Thereafter, we asked whether discussants would support Bill Clinton, Bob Dole, or some other candidate. (See Appendix B.) The coefficient for the primary season dummy variable (-76.975) suggests that asking about specific candidates rather than the more general party nominee increases the time required to answer by about three-fourths of a second. It seems easier to say in the abstract that an associate will vote Republican or Democratic than to say that the associate will vote for a specific candidate.

Apart from the effect that arises due to the specification of candidates at the end of the primary season, the model suggests a fairly strong, clarifying effect as the campaign progresses—measured as the week of interview—even after all the other factors are taken into account. If we ignore the candidate specification effect, response time decreases by 1.2 seconds across the 36 weeks (-3.44×35) , that is, the accessibility of

⁶ For respondents who named only two discussants, the measure is 0 if the respondent perceives that the other discussant in the network holds a preference different from the one reported by the discussant in the dyad; I if the respondent perceives that the other discussant holds the same preference. For respondents who named more than two discussants, the measure identifies the proportion of the residual network that is perceived to hold the preference reported by the discussant in the dyad.

⁷ In this way it is analytically possible to define an accessible independent, someone who quickly self-identifies as independent on the party identification scale, or an inaccessible strong identifier, someone who takes a long time to report strong attachment to one of the parties.

⁸ The median time of response for the party identification question was 1.2 seconds.

judgment increases. Alternatively, if we only consider the period after the end of the primary season—the first week of July and beyond—response time decreases by approximately two-thirds of a second (-3.44×19) .

In summary, one overall consequence of the election campaign is to make these judgments more accessible in memory. Other factors contribute, and none is more important than the extremity of the discussant's (sender's) partisan identification. The model estimates suggest that response time decreases by more than a full second if the discussant is a strong Republican or Democrat rather than an independent (-36.70×3) . The accessibility of the main respondent's partisanship is nearly as important: Response time for the judgment increases by approximately three-fourths of a second across the lower 95% of the range of the main respondent's response time on the party identification measure $(.13 \times 615)$, 10 even after we take account of the baseline speed of response. In short, strong attitudes, measured either in terms of accessibility or extremity, serve only to enhance rather than impede the effectiveness of communication.

Reflective Confidence In Political Judgments

Accessibility is only one important aspect of effective communication. It is entirely possible for an individual to hold a highly accessible opinion about how someone will vote that is not sustained on further reflection. For example, a person may have a strong association in memory between "Jane" and "support for Republican candidates," but on further reflection he may recall that Jane dislikes politicians from Kansas, which shakes his confidence in his judgment about Jane's support for Dole. Alternatively, a person may have a highly inaccessible opinion about Joe's voting behavior, but further reflection based on a variety of factors—a dimly recalled conversation, a generalization based on other people she knows—may lead her to express a great deal of confidence in her judgment.¹¹

After the main respondent provided a judgment about the discussant's voting preference, the interviewer asked the respondent "how difficult or easy it was to say" which candidate the discussant supports.

⁹ If we include the primary season coefficient in the calculation, predicted response time still decreases across the campaign, from week 1 to week 36 or beyond, by approximately 4 seconds. The combined effect in week 1 is $-76.97 - (3.44 \times 1)$; in week 36 it is $-(3.44 \times 36)$

We treat this question as a measure of the Main Respondent's Confidence in the Judgment, ranging from 0 for those who offered no judgment to 4 for those who found the judgment "very easy." (See Appendix B.)

Nature of Relationship. Unlike the first model of Table 1, the second model shows statistically discernible effects due to the relationship between the discussant and the main respondent. In particular, respondents are more confident in their judgments about the political preferences of spouses and other relatives. People may not have more accessible judgments about a spouse or relative, but they are more confident in the judgments they render. (It remains unclear whether such confidence is justified, which is discussed further below.)

Agreement Efects. The second model of Table 1 also shows no effect on judgmental confidence due to agreement or disagreement with the discussant. Neither accessibility nor judgmental confidence is affected by disagreement within the dyad. Confidence is enhanced if the respondent perceives that a higher proportion of the residual network agrees with the self-reported preference of the discussant in the dyad, but the effect is relatively modest.

Preference Efects. In terms of partisan preferences, the results of the second model show that the extremity of candidate evaluations—for both discussants and main respondents—appears to enhance judgmental confidence. That is, people are more likely to trust their judgment if they or their discussants hold an extreme position regarding candidates. Confidence is also enhanced if the discussants report a more extreme position on the partisan identification scale.

Other Political Characteristics. The second model in Table 1 reveals that the confidence of respondents' judgments is unaffected by the imputed knowledge of the discussant. In contrast, effects are produced by whether the discussant is named as part of the political network (as opposed to the more general "important matters" network) and by the reported frequency of political discussion; this latter effect is quite pronounced.

Campaign Dynamics. The effect of campaign week noted in model 1 is sustained in model 2. Across the campaign, respondents become three-quarters of a point more confident in their judgments (.022 × 35), but this is partially offset by the effect of the primary season, when respondents are only asked whether their discussants will support the Democrat, the Republican, or some other candidate. ¹² Although we interpret this as a consequence of question wording, it is worth noting that reality lurks behind the choice of words. During the primary season, it was not feasible to ask about specific candidates, because no one knew with certainty whether Bob Dole would be the Republican

¹⁰ To avoid overstating the effect of partisan accessibility, the range of the response time measure is truncated in calculating the magnitude of this effect by eliminating the top 5% of the distribution. The elongated tail of the original distribution represents uncharacteristically long response times among a very small part of the sample. After truncation, the range of response times is from 17 (or .17 seconds) to 632 (or 6.32 seconds).

¹¹ Bassili (1996) refers to these sorts of evaluations as meta-attitudes, or attitudes regarding attitudes. We anticipate that people will, in general, have more confidence in judgments that are more accessible. Indeed, the correlation between accessibility and confidence is .48. The correlation between accessibility and accuracy is .28, and the correlation between confidence and accuracy is .43.

¹² If we include the primary season coefficient in the calculation, confidence still increases across the entire campaign (from week 1 to week 36 or beyond), but the net effect is reduced to .4.

nominee or whether Ross Perot would run. As noted earlier, respondents might be confident about a response to a relatively abstract question but less so when asked about particular candidates discussants would support.

The Accuracy of Political Judgments

Fundamentally, the effectiveness of political communication depends upon the accuracy of one citizen's judgment regarding the preferences of another (Huckfeldt and Sprague 1995; Huckfeldt et al. 1998a). If Jane thinks that Sam supports Dole but Sam is actually a Clinton voter, the collective deliberations of democracy have malfunctioned. Indeed, accurate perception is a defining ingredient not only of effective communication but also of collective deliberation in democratic politics.

The Accuracy of the Main Respondent's Judgment is defined as agreement between the main respondent's perception of the discussant's preference and the discussant's self-reported preference. A binary response—accurate or inaccurate—is produced, so a nonlinear logit model is employed. Because of the model's nonlinear parameters, the effect of any single variable is best evaluated with respect to a particular point on the probability distribution for accuracy. We generally examine the magnitude of change in the probability of an accurate perception across the range of one variable while other variables are held constant at typical values.

Nature of Relationship. With respect to the relationship between the main respondent and the discussant, the pattern of effects for accuracy is different from the pattern for confidence. People are confident about their assessment of a spouse's or relative's political preferences, but that does not mean they are correct. The lack of effect on accuracy parallels the lack of effect on accessibility. People are likely to believe they make accurate judgments about family members' political views, but their confidence appears to be misplaced!

Agreement Efects. The results on agreement show even more divergence from earlier models. Accuracy is dramatically enhanced by agreement within the dyad, as well as by agreement between the discussant's self-reported preference and the main respondent's perception of preferences within the residual network. The magnitudes of these effects are shown in Figure 1A, where we can see that particularly strong effects arise due to the residual network. Although there was little evidence of an agreement effect on accessibility or judgmental confidence, there is strong evidence for such an effect on accuracy.

Preference Efects. The political preferences of main respondents and discussants give rise to a pattern of effects that parallels many of the earlier results. In particular, accuracy is enhanced to the extent that discussants' preferences are extreme—both with respect to partisanship and with respect to candidate

evaluations. As Figure 1B shows, discussants are much more likely to be perceived accurately if they send clear and unambiguous messages; extreme partisans with extreme views about the candidates are more than 40 percentage points more likely to be perceived accurately than independents who are neutral. Finally, there is no evidence to suggest that the extremity or accessibility of main respondent preferences diminish the likelihood of perceiving discussant preferences accurately.

Other Political Characteristics. Main respondents are more likely to perceive preferences accurately for "political" discussants than for "important matters" discussants. In contrast, neither the imputed political knowledge of the discussant nor the frequency of political discussion affects accuracy.

Campaign Dynamics. The election campaign has a substantial effect on the accuracy of main respondent perceptions of discussant preferences. The magnitude of the effect is shown in Figure 1C, both for "important matters" discussants with whom the main respondent reports infrequent political conversation and for "political" discussants with whom the main respondent reports frequent political discourse. Discussants are about 30 percentage points more likely to be perceived accurately at the end of the campaign than at the beginning.

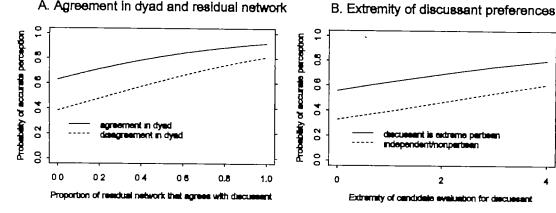
As the campaign progresses, people develop more readily accessible perceptions about associates' preferences, and these campaign effects are directly reflected in the accuracy of their judgments. It appears that the campaign stimulates deliberative democracy not only by making respondents more confident in more highly accessible judgments but also by improving the quality of those judgments, as measured in terms of accuracy. ¹³ In contrast, some variables affect only accuracy, most notably political disagreement within both the dyad and the residual network. Disagreement does not seriously compromise the confidence or accessibility of judgments, but it does compromise the accuracy of judgments.

CONCLUSION

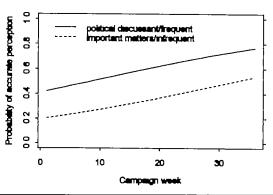
Our analysis offers little support for the importance of selective perception or conflict avoidance in patterns of political communication among citizens. If the data had revealed that extreme partisans are less likely to make accurate judgments about others' preferences, then we might conclude that either (1) the discussant is sending an ambiguous political signal in order to avoid disagreement and conflict with a highly opinionated associate, or (2) the highly opinionated main respondent is selectively perceiving the discussant's preference in order to avoid the psychic distress of political

¹⁵ Indeed, the response time for the respondent's own intended/reported vote decreased by one-half second from the period before the election to the period after the election, controlling for candidate evaluations, strength of their partisan identification, and their baseline speed of response.

FIGURE 1. The Effect of Various Factors on the Predicted Probability That the Main Respondent Accurately Perceives the Discussant's Vote Preference



C. Campaign week and discussant type/discussion frequency



Source: Table 1, column 3, logit estimates.

Noter In each part of the figure, the discussant is held constant as a nonrelative who is not a close friend. Partisan accessibility for the main respondent and the discussant is held constant at 1.2 seconds. Main respondent partisen extremity is held constant at 2, and candidate evaluation extremity is held constant at 2.1. Imputed discussant knowledge is held constant at 2.3. The primary season dummy variable is held constant at 0.

In A, dyadic agreement and readual network agreement are vaned across their ranges. The campaign week is held constant at 25. Frequency of discussion is held constant at 3. The discussant is held constant at a political discussant. Partisan extremity of the discussant is held constant at 2. Candidate evaluation extremity of the discussant is held constant at 2.1.

In B, the partieun extremity and candidate evaluation extremity of the discussant are varied across their observed ranges. The dyad is held constant at disagreement, and the residual network is held constant at a disagreement level of .5. Other variables are held constant as in A.

in C, the campaign week is varied across its range for political discussants with whom the respondents often discuss politics and for important matters discussants with whom they never discuss politics. Discussant parties extremity is held constant at 2, and candidate evaluation extremity is held constant at 2.1. Other variables are held constant as in B.

disagreement. But we see no effect of main respondent extremity on either accuracy or accessibility. The extremity of the main respondent's candidate preference does enhance judgmental confidence, but in the absence of any distorting effect on accuracy, such an effect offers little support for the important influence of selective perception or psychic distress on political communication.

Moreover, whereas there is no evidence that strong partisans are bad listeners, it is clear that they are more effective communicators. Main respondents are more likely to offer accessible, confident, and accurate judgments regarding the political preferences of discussants who hold more extreme views. This is another way of saying that people's political perceptions of one another are subject to the actual flow of information that occurs through social communication, and people with extreme preferences are more likely to communicate unambiguous infor-

mation. Once again, political judgment is not an artifact of selective perception but depends on the quality of political information.

The analysis encourages us to rethink the nature of disagreement and selective perception with respect to political communication. If disagreement produces the psychic discomfort and distress that leads to selective perception, it might be expected to produce political judgments that are hesitant or uncertain. Instead, we see no effects on accessibility and confidence due to disagreement between the respondent and the discussant, and only relatively weak and inconsistent effects arise due to the perceived distribution of preferences among the remaining members of the residual network.

In contrast, the distribution of preferences within the dyad and the residual network register profound effects on accuracy. If a person does not hold a particular preference, and if she does not believe that others hold

the preference, her own personal experience tells her that a given individual in her network does not hold the preference either. For that reason, people are much less likely to recognize rare preferences, that is, preferences they perceive to be uncommon in their network of social relations. Hence, an individual's own political preference may be primarily important as a piece of information to use in judging the likely preference of someone else.

Rather than cognitive dissonance and selective perception, it appears that a model of political communication should emphasize the heuristic strategies people use in reaching political judgments about the preferences of others (Sniderman, Brody, and Tetlock 1991; Tversky and Kahneman 1974). When an associate's preference is unclear, individuals might employ an inferential device to resolve the uncertainty, and a personal experience heuristic appears particularly important. Thus, disagreement effects on accuracy arise as the consequence of judgmental errors that occur when people use a reasonable decision-making device which happens to fail in a particular circumstance.

Our findings do not affect theories of cognitive dissonance and selective perception as they relate to the perceptual biases that arise due to discomfort and personal distress over inexplicable differences of opinion (see Ross, Bierbrauer, and Hoffman 1976). Rather, these results lead one to question the extent to which political disagreement produces psychic discomfort in most citizens. For several reasons, it may produce very little of the personal distress associated with selective perception. Many people do not care enough to be upset. Others see political opinions as idiosyncratic expressions of personal preference that do not require explanation, similar to the choice between a Chevrolet and a Ford. Moreover, differences in preference and opinion often are explicable: Tom votes for Democrats because he is a liberal; Jane voted for Dole because she is rich; and so on. In short, most political differences are easily reconciled, readily accommodated, and unlikely to be personally distressing.

Finally and most important, this analysis sharpens our focus on the positive consequences of election campaigns. They improve the quality of political deliberation by stimulating the development of strongly held opinions regarding the candidates and by enhancing the effective communication of these preferences among and between citizens. American presidential elections are often criticized because they start so early and last so excruciatingly long, but it is this length that provides the opportunity for extended public discussion. The outcome may be highly predictable even before the campaign takes place (Gelman and King 1993), but this does not diminish the role of campaigns as an important deliberative device in democratic politics. The successful candidate must survive the collective process of public scrutiny, and stimulating such a process is arguably the most important function of democratic elections.

APPENDIX A: SAMPLE

The main respondent sample (n = 2,174) was drawn from lists of registered voters in the city and county of St. Louis, Missouri, and in Marion County, Indiana (Indianapolis). The response rate was 58%, calculated as the ratio of completions to the sum of completions, refusals, partials, and identified respondents who were persistently unavailable to complete the interview. The response rate drops to 53% if we add households in which no one ever answered the phone despite repeated call backs. The cooperation rate—ratio of completions to the base of completions, refusals, and partials—is 64%. Comparable rates for the discussant sample (n = 1,475) were 59%, 56%, and 72%.

The weekly preelection interview target was 20 main respondents and 15 discussants at each of the two study sites, and discussant interviews were completed within three weeks of the relevant main respondent interview. After the election, interviews were completed as rapidly as possible without weekly targets, and the time spacing of main respondent and discussant interviews was not controlled. On this basis, 612 main respondents and 452 discussants were interviewed from March through June; 732 main respondents and 384 discussants were interviewed from July through the election; and 830 main respondents and 639 discussants were interviewed after the election through early January.

Eighty percent of the main respondents responded to the request in their interviews for first names of discussants. Of those who provided this information, 60.8% agreed to give additional contact information (surnames, phone numbers, and so on), 25.7% refused, and the remainder agreed to a call back to collect the information. Ultimately, at least one discussant was interviewed for 872 main respondents, or 40% of the entire main respondent sample of 2,174.

APPENDIX B: MEASUREMENT

Timing Responses

The interviewers activated timers to record the time elapsed between the end of the question and the beginning of the respondent's answer. In addition, latent timers recorded the time elapsed between the answers to two sequenced questions, and they were used in data verification procedures.

Several problems may occur in the use of activated timers. First, respondents sometimes answer before the question is completely read. In such instances, the interviewer recorded the response time as simultaneous by instantaneously turning the activated timer on and off. Second, a "trigger happy" interviewer may stop the timer too quickly. After recording the answer to a question, the interviewers had to indicate whether the timing was successful. We redefined unsuccessful timings as missing data. Third, respondents sometimes ask for clarification, and interviewers were instructed to let the timer run during these questions. To avoid a highly skewed distribution of response times, we defined the timing data as missing when the recorded time was more than three standard deviations above the sample mean. Finally, interviewers occasionally and inadvertently erase valid timings by using a skip command to return to an earlier question at the respondent's request. In these cases, the recorded time is artificially low—faster than the time actually required to turn the timer on and off. Interviewers were instructed to reset the timer to missing, but we could verify the data with the latent timer, which recorded elapsed time between the answers to sequenced questions. If the comparison revealed a time too short to allow a question to be read (one second), the time data were set to missing.

Measuring Preferences and Judgments across the Campaign

Candidate evaluations are moving targets during campaigns, even in the 1996 race, which lacked the volatility of more dramatic contests. When interviews began in early March, there was a long list of Republican candidates, and no one knew what role Ross Perot would play. By early summer, the major party candidates had been determined, although Perot's intentions were still ambiguous. Thus, we adopted several different questions to ascertain the voting intentions of the main respondents and their judgments about discussants' political preferences.

Before the first week of July, we used an open-ended question for the respondent: "If the election was held today, whom would you like to be elected?" We used a closed-choice question for perceptions about a discussant: "As things currently stand, do you think [discussant] will vote for the Democratic candidate, the Republican candidate, an independent candidate, or do you think [discussant] probably won't vote?"

Beginning in the first week of July, we asked respondents: "Thinking about the presidential election, will you vote for Bill Clinton, Bob Dole, some other candidate, or haven't you decided?" The perception question was: "As things currently stand, do you think [discussant] will vote for Bill Clinton, Bob Dole, some other candidate, or do you think [s/he] probably won't vote?"

After the election, the two questions were: "Thinking about the presidential election, did you vote for Bill Clinton, Bob Dole, or did you vote for some other candidate?" "Do you think [discussant] voted for Bill Clinton, Bob Dole, some other candidate, or do you think [s/he] didn't vote?"

Before the major party candidates became clear, only a trivial number of respondents claimed to support an independent other than Perot, but a significant number supported some other Democrat or Republican. Therefore, in the first wave of the study—when respondents were asked the openended version of the question—we distinguished the answers as support for Democratic candidates, Republican candidates, or Perot. Once the major party candidates became clear, only a very insignificant number of respondents named someone other than Dole, Clinton, or Perot, so we set these "other" responses to missing.

Dependent Variables and Measures

Accessibility of main respondent's judgment regarding discussant's vote preference: See above for question text. Response time in hundredths of seconds.

Main respondent's confidence in judgment: "How difficult or easy was it to say how [discussant] voted/will vote?" Scale: 0 = does not know discussant vote; 1 = very difficult; 2 = somewhat difficult; 3 = somewhat easy; 4 = very easy.

Accuracy of the main respondent's judgment: See above for question text. Recorded 1 if main respondent's judgment agrees with discussant's self-reported preference; 0 otherwise.

Explanatory Variables and Measures

Mean network response time in the residual network: See above for question text. Mean of logged response times to preference questions about all discussants other than the one in the particular dyad.

Question order for perception of discussant: The sequence (1-5) in which the respondent is asked about each discus-

sant's candidate preference; for example, a value of 2 means the question about a particular discussant was asked second in the sequence.

First named discussant: Recorded 1 if the discussant was the first name generated by the respondent, 0 otherwise.

Spouse: "Is [discussant] a spouse or partner? other relative? or unrelated by blood or marriage?" Recorded 1 if discussant is a spouse; 0 otherwise.

Another relative: See preceding for question text. Recorded 1 if discussant is a nonspouse relative, 0 otherwise.

Close friend: "Would you say [discussant] is a close friend, a friend, or just someone that you regularly come into contact with?" Recorded 1 if discussant is a close friend, 0 otherwise.

Agreement with discussant in this dyad: Recorded 1 if there is agreement in the self-reported candidate preferences of main respondent and discussant, 0 otherwise.

Proportion of residual network that agrees with discussant: The residual network is defined as all the discussants identified by the respondent, except for the discussant in a particular dyad. Scored as the proportion of the residual network perceived to hold a candidate preference that agrees with the self-reported preference of the discussant in a particular dyad.

Partisan accessibility: "Generally speaking, do you think of yourself as a Republican, a Democrat, an Independent, or what?" Response time to the question.

Partisan extremity: Coded on the basis of response to the party identification question. Scale: 0 = independent or nonidentifier; 1 = leans toward Democrats or Republicans; 2 = not strong identifier, 3 = strong identifier.

Candidate evaluation extremity: "Using any number from 1 to 5, how unfavorable or favorable do you feel toward Bill Clinton/Bob Dole?" Scored as the absolute value of the difference between evaluations of the two major party candidates; range is 0 (indifferent) to 4 (most opinionated).

Imputed discussant knowledge: "Generally speaking, how much do you think [discussant] knows about politics?" Scale: 1 = not much at all; 2 = an average amount; 3 = a great deal.

Reported discussion frequency with discussant: "When you talk with [discussant], do you discuss political matters: often [4], sometimes [3], rarely [2], or never [1]?"

Campaign week: Week of interview. Scored from 1 = first week in March to 36 = election week and beyond.

Primary season: Scored 1 if week of interview is earlier than first week in July, 0 otherwise.

Network Name Generator

The political network variable was scored 1 if the respondent answered the politics name generator question, 0 if the respondent answered the important matters question.

Politics: "From time to time, people discuss government, elections and politics with other people. I'd like to know the people you talk with about these matters. These people might or might not be relatives. Can you think of anyone?"

Important matters: "From time to time, people discuss important matters with other people. Looking back over the last few months, I'd like to know the people you talked with about matters that are important to you. These people might or might not be relatives. Can you think of anyone?"

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The Effects of Canvassing, Telephone Calls, and Direct Mail on Voter Turnout: A Field Experiment

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e report the results of a randomized field experiment involving approximately 30,000 registered voters in New Haven, Connecticut. Nonpartisan get-out-the-vote messages were conveyed through personal canvassing, direct mail, and telephone calls shortly before the November 1998 election. A variety of substantive messages were used. Voter turnout was increased substantially by personal canvassing, slightly by direct mail, and not at all by telephone calls. These findings support our hypothesis that the long-term retrenchment in voter turnout is partly attributable to the decline in face-to-face political mobilization.

uring the last half-century, a dramatic transformation has occurred in the manner in which voters are mobilized. The election campaigns described by Gosnell (1937), Sayre and Kaufman (1960, chap. 6), and Wolfinger (1974, chap. 4) relied heavily on face-to-face contact between voters and those seeking their support. Notably absent from such accounts are professional campaign consultants, direct mail vendors, and commercial phone banks, all of which have gradually replaced work performed by party activists. The advent of modern campaign tactics (Broder 1971; Ware 1985) has coincided with a decline in the proportion of adults who report working for a political party. Based on an annual aggregation of Roper surveys between 1973 and 1994, Putnam (2000, 41) reports a steady decline in this proportion: Whereas 6% of the public reported working for a political party in the early 1970s, just 3% did so in the mid-1990s.

At the same time, there has been a marked decline in the size and vitality of nonpartisan organizations. In the mid-1960s, 2.4 of every 1,000 women over the age of 20 belonged to the League of Women Voters, compared to .79 in 1998 (Putnam 2000, 438–44). A similar fate has befallen such civic organizations as the Lions, Rotary, and Kiwanis Clubs, which have experienced sharp membership declines since the 1960s. Due to the changing character of both partisan and nonpartisan organizations, voter mobilization has become increasingly impersonal, and messages that once might have been delivered in person are now communicated using mass marketing techniques.

The decline of personal mobilization has arguably contributed to the erosion of voter turnout in the United States since the 1960s. This hypothesis is related to, yet distinct from, Rosenstone and Hansen's (1993) contention that diminishing rates of turnout are a result of a decline in the volume of mobilization

activity. As Abramson, Aldrich, and Rohde (1998, 85) point out in their discussion of Rosenstone and Hansen, there has been no clear trend over time in the proportion of American National Election Study (ANES) respondents who report some form of contact with political parties or campaigns, whether personal or impersonal. When read in conjunction with the trend lines compiled by Putnam, the ANES data are consistent with the view that campaigns are reaching as many people as ever but through less personal means.

Our hypothesis about declining turnout rates rests on the claim that personal canvassing mobilizes voters more effectively than other modes of contact that have taken its place, such as direct mail or telephone appeals. The literature on collective action and prosocial behavior supports this conjecture. Studies of blood donations, recycling, and "good deeds" underscore the importance of delivering urgent requests and making vivid the obligation to act (Christensen et al. 1998; Wang and Katzev 1990), and these blandishments seem particularly effective if delivered in person (Spaccarelli, Zolik, and Jason 1989). The special force of face-toface contact is illustrated by Reams and Ray (1993) and Jason et al. (1984), whose experiments demonstrate that recycling and blood donations are particularly responsive to in-person appeals.

There is good reason to suspect that personal canvassing is an effective means by which to mobilize voter turnout, but its effects have seldom been gauged reliably. Nonexperimental studies, beginning with Kramer (1970), tend to rely on survey data to examine the relationship between turnout and reported contacts with political organizations or candidates (Blydenburgh 1971; Cain and McCue 1985; Caldeira, Clausen, and Patterson 1990; Kramer 1970; Lupfer and Price 1972; Price and Lupfer 1973; Wielhouwer and Lockerbie 1994; see also Huckfeldt and Sprague 1992). Rosenstone and Hansen (1993), for example, regress reported voter turnout on reported contact with candidates or political parties. An important drawback to this approach is that political contact may not be an exogenous predictor of turnout. If parties direct their appeals disproportionately to committed partisans, those most likely to vote will also be most likely to receive contact, and the apparent link between contact and turnout may be spurious. Regression analyses generally include a host of control variables, but it is

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Study	Date	Type of Election	Number of Subjects (Including Control Group)	Treatment	Effect on Turnout
Gosnell (1927)	1924	Presidential	3,969	Mall	+1%
Gosneli (1927)	1925	Mayoral	3,676	Mail	+9%
Eldersveld (1956)	1953	Municipal	41 43	Canvass Mali	+42% +26%
Eldersveld (1956)	1954	Municipal	276 268 220	Canvass Mali Telephone	+20% +4% +18%
Adams and Smith (1980)	1980	Special election, City Council	2,650	Telephone	+9%
Miller, Bositis, and Baer (1981)	1981	Primary	79 80 81	Canvass Mall Telephone	+21% +19% +15%

Note The subjects in all experiments were registered voters. In Eldersveid (1966), subjects were selected in 1953 from those who opposed or had no opinion about charter reform, in 1964 from those who had voted in national but not local elections. This table includes only attudies that used random assignment to treatment and control groups (or near-random assignment in the case of Goenell 1927).

"These are the effects given in the tables of the research report. They are not adjusted for contact rates.

unclear whether these covariates eliminate the problem. Another important limitation of survey-based studies is that the researcher has no control over, and often little knowledge of, the frequency or nature of the political contact.¹ The measure of party contact drawn from the ANES, for example, makes no distinction between telephone calls and personal canvassing. If these two modes of contact have different effects, measures that fail to differentiate them will produce distorted results.

More methodologically defensible, although rare, are experimental studies of mobilization, the results of which are summarized in Table 1. The experimental tradition harks back to Gosnell's (1927) studies in Chicago, which assigned certain city blocks to receive nonpartisan mail reminders to register and vote. Gosnell found that turnout increased by 1% in the presidential election of 1924 and 9% in the municipal election of 1925. Adams and Smith (1980) examined the effectiveness of one mode of voter mobilization, telephone calls, and found that partisan appeals had significant effects in a city council election in Washington, D.C. The principal experiment to examine the effects of personal canvassing in conjunction with mailings that used varying types of nonpartisan appeals was conducted by Eldersveld (1956; Eldersveld and Dodge 1954) in two Ann Arbor, Michigan, municipal elections.2 In both cases the effects of canvassing and mail were statistically significant, but total sample size for all conditions was just 453; the effects can only be inferred

¹ The fact that the independent variable is reported political contact raises further concerns. A number of studies indicate that respondent reports are subject to potentially serious measurement error stemming from faulty memory or deliberate misreporting.

with a 95% confidence interval of plus or minus 20 percentage points, so no reliable inferences can be made about the relative effectiveness of different forms of mobilization. The problem of statistical power is even more acute in the Miller, Bositis, and Baer (1981) study of primary election voting in Carbondale, Illinois. It compared the effectiveness of partisan telephone, mail, and personal appeals, but the total sample size was only 215. As did Eldersveld, these authors found personal canvassing to be highly effective, but the range of uncertainty causes the effects to fall short of conventional levels of statistical significance. An additional source of concern is the fact that Eldersveld (1956), Adams and Smith (1980), and Miller, Bositis, and Baer (1981) fail to adjust for their inability to reach some people in the treatment groups. As we explain below, their practice of folding uncontacted subjects originally assigned to treatment groups into the original control group tends to overstate the effectiveness of mobilization campaigns.

Due to the significant limitations of previous research, students of voting behavior do not have a clear sense of either the current magnitude of mobilization effects or the relative effectiveness of different mobilization strategies. Experimental studies have generated the most convincing evidence of causality but are either dated or small in scale. For these reasons we launched a series of turnout experiments in which randomly selected households were exposed to mailings, telephone calls, or personal appeals before the November 3, 1998, general election. Following Eldersveld, we varied the content of appeals, stressing civic duty, neighborhood solidarity, or the prospect of a close election. As did Miller, Bositis, and Baer (1981), we examined the effects of repeated mail contact. Because our sample sizes are roughly 100 times larger than the Eldersveld (1956) or Miller, Bositis, and Baer (1981) studies, we can draw far more precise inferences about the effectiveness of various mobilization tactics. Our

² The finding that participation can be stimulated by mobilization efforts is consistent with evidence that people subjected to a preelection interview are also more likely to vote, even though the interviews are not designed to encourage voting. For details, see Anderson, Silver, and Abramson 1988; Granberg and Holmberg 1992, Kraut and McConohay 1973; Traugott and Katosh 1979; and Yakch 1976.

	Number of Direct Mallings Sent				
	None	One	Two	Three	Total
No Telephone Call: Personal canvassing					
No In-person contact attempted	10,800	2,406	2,588	2,375	18,169
In-person contact attempted	2,686	519	625	627	4,457
Telephone Call: Personal canvassing					
No in-person contact attempted	958	1,451	1,486	1,522	5,417
In-person contact attempted	217	385	352	383	1,337
Total	14,661	4,761	5,051	4,907	29,380

findings indicate that personal canvassing is highly effective, much more so than the direct mail and telemarketing campaigns that have come to displace it. The implication is that the decline in voter turnout may be due to the changing character of American campaigns. Although the volume of mobilization activity remains considerable, its increasingly impersonal nature draws fewer people to the polls.

EXPERIMENTAL DESIGN

This field experiment was conducted in New Haven, Connecticut, which has a population of approximately 100,000. In September 1998 we obtained a complete list of registered voters, from which we created a data set of all households with one or two registered voters. To eliminate students from the sample, all names with post office box addresses were excluded, as was one voting ward that encompasses a university and student housing. We were left with 29,380 individuals (22,077 households), whose participation in the 1998 election could be determined from public records.

Our study was designed to measure the effect of personal canvassing, telephone calls, and direct mail appeals on voter turnout. Through a series of random assignments, the sample was divided into control and experimental groups. Table 2 shows the sample sizes of each group for the $2 \times 2 \times 4$ design.³ The treatment and control groups for the three experiments overlap, such that 10,800 people were assigned no intervention; 7,369 were sent at least one mailing but received no other appeal; 2,686 were slated only for personal contact; and 958 were assigned to receive only telephone reminders. The remainder of the sample, 7,567 people, was assigned to two or more treatments. Assignment to the personal canvassing experiment was designed to be uncorrelated with the telephone and mail experiments, so that it could

be analyzed separately. Random assignment to each of the telephone/mail treatments was performed in a manner that made calls more frequent among those who received mail. Thus, these two treatments are correlated, and their effects must be estimated using multivariate methods.

Overall, the treatment group for personal canvassing contained 5,794 people, the control group 23,586. For the direct mail experiment, 14,719 people were in the treatment group, and 14,661 were in the control group. The effectiveness of randomization was checked using voter turnout data from the 1996 presidential election. Based on a chi-square test with 15 degrees of freedom for the 16 groups defined in Table 2, we cannot reject the null of independence between treatments and past voting behavior (p > .10).

Personal Canvassing Procedure

During each Saturday and Sunday for four weeks before the election, canvassers were sent to contact randomly selected registered voters. They were paid \$20 per hour and were primarily graduate students. New Haven has a substantial minority population and a significant proportion of non-English speakers. More than half the canvassers were African American or fluent in Spanish, and when possible they were matched to the racial and ethnic composition of the neighborhoods they walked.

For safety reasons, all canvassing was done in pairs and ceased at sunset. This procedure constrained both the pool of available canvassers and our ability to contact people not at home during the day. In contrast to conventional canvassing efforts, we targeted certain households rather than entire streets, which meant that more time was devoted to locating specific addresses and walking from one to the next. Consequently, canvassers were able to contact only 1,615 (28%) of the 5,794 people in the personal canvassing treatment group. Examination of the data showed a fairly even

³ Random assignment was done at the household level. The results we present treat individuals as the unit of analysis; as we point out below, however, the results are very similar when we look separately at households containing one or two registered voters. Also, the standard errors we report are very similar to the ones obtained using statistical methods that allow for unmodeled similarities between household members, such as generalized least squares or resampling.

⁴ For the subset of persons not contacted, two supplementary experiments were performed. In certain wards, 719 were randomly chosen to receive a mailer, along with a refrigerator magnet that had the election date printed on it. A separate analysis indicated that this

contact rate across the 29 wards of the city. The average contact rate by ward was 28%, with a standard deviation of 9%.

To bolster the external validity of the experiment, we used a variety of messages when encouraging people to vote. These were developed after a series of conversations with four professional campaign consultants, each of whom suggested effective nonpartisan appeals.5 The final versions reflect both themes used in actual mobilization efforts and social science explanations of voting: civic duty, close election, and neighborhood solidarity. Upon contacting one or both of the registered voters at each address, canvassers read the following introduction: "Hi. My name is _. I'm part of Vote New Haven '98, a nonpartisan group working together with the League of Women Voters to encourage people to vote. I just wanted to remind you that the elections are being held this year on November 3d."6

In the civic duty version, the script continued: "We want to encourage everyone to do their civic duty and exercise their right to vote. Democracy depends on the participation of our country's citizens." The close election version stated: "Each year some election is decided by only a handful of votes. Who serves in important national, state, and local offices depends on the outcome of the election, and your vote can make a difference on election day." The neighborhood solidarity version stated: "Politicians sometimes ignore a neighborhood's problems if the people in that neighborhood don't vote. When politicians see a lot of people turning out to vote, they know they should pay attention to issues important to people who live around here."

The civic duty script appeals to a sense of obligation. It states a norm that citizens are expected to vote and contends that democracy depends on political participation. This appeal parallels a central explanation of large-scale collective action, the notion that citizens derive intrinsic satisfaction from participation (Riker and Ordeshook 1968). In contrast, the last two messages emphasize two ways in which voting may be instrumental. First, the close election appeal reminds citizens that a single vote can determine the outcome. The odds of casting a pivotal vote are remote, but the stakes are high, and elections are close from time to time. Alternatively, by voting, one calls politicians' attention to the concerns of one's neighborhood. The

subsidiary treatment had a negligible effect that is swamped by a large standard error. For another subset of those not reached (683 persons), canvassers left leaflets if no one was home. This treatment was not strictly random and occurred only in certain wards. We found no differences in canvassing effectiveness between wards with or without leafleting (p > .10, one-talled test). Even if the true effects of leafleting and mallers were equivalent to three mailings, the overall bias to our estimate of the effect of personal canvassing would be less than 1 percentage point.

⁵ The League of Women Voters, New Haven Chapter, also helped in the development of the messages and canvassing procedures. neighborhood solidarity theme was developed in light of the special political circumstances in the East Shore section of New Haven, where a secession movement has been active for more than a decade. In sum, these appeals encompass leading themes of voter mobilization campaigns.

Direct Mall

The direct mail experiment was intended to measure the turnout effect of both the number of mailings received and the message conveyed. To gauge the first effect, we divided the treatment group into three subgroups and sent one, two, or three mailings, respectively. As shown in Table 2, each subgroup contained approximately 4,900 persons. The mailings were sent out at three intervals: 15 days, 13 days, and 8 days before the election. The subgroup that received two mailings was sent mail on the two dates closest to the election, and the single mailing was sent 8 days before the election. Within each of these groups, we randomly generated three additional subgroups, one for each type of political message. (Subjects who received a certain appeal through personal canvassing received the same type of appeal by mail.) To avoid sending anyone the same mail piece twice, nine different postcards were required, three for each form of appeal. The postcards were prepared by a professional political consulting firm that specializes in direct mail. All nine treatments were three-color postcards. A description of each card is given in the appendix.

Telephone Calls

During the three days before and including the election, a random subset of registered voters received calls urging them to vote. These took place on Sunday evening and all day Monday and Tuesday. A large out-of-state telemarketing firm, which conducts several such get-out-the-vote campaigns during each election cycle, conducted the 30-second calls. Although the telephone numbers taken from the voter registration files were cross-checked using data obtained from Survey Sampling, Inc., many wrong numbers remained. And, as is the norm, only a portion of the potential respondents could be contacted during the three days, despite repeated attempts.7 Of the 6,754 people in the treatment group, only 2,166 (32.1%) resided in a household in which at least one registered voter completed a conversation with the phone bank. The telephone scripts mirrored those for personal canvassing, but only the civic duty and close election appeals were used. We excluded neighborhood solidarity because that might be an implausible appeal coming from an out-of-state phone bank (it was expected that those called would note nonlocal accents). Subjects other-

⁶ The closing line of the script was varied randomly. With 40% probability, canvassers ended with: "We hope you'll come out and vote." Otherwise, they closed with a question: "Can I count on you to vote on November 3d?" We found the "question effect" nonsignificant (p > .05) and collapsed these experimental categories for purposes of analysis.

⁷ Since the treatment group and control group are formed by random assignment, the failure to contact the entire treatment group due to wrong or unlisted numbers will not lead to biased results if the estimator in equation 6 is used.

Unadjusted Relationship between A	Actual (as Opposed to Attempted) Contact ar	nd Voter Turnout
	Not Contacted In Person	Successfully Contacted In Person
Percentage voting	44.5%	59.0%
Number of persons	27,765	1,615
Unadjusted Relationship between I	Experimental Subgroups and Voter Turnout Assigned to the Control Group (no personal contact)	Assigned to the Treatment Group (attempted personal contact)
Percentage voting	44.8%	47.2%
Number of persons	23,586	5,7 94
Number actually contacted		1,615
Contact rate		27.9%
Estimated Effect of Personal Conta Turnout Differential (2.43%)/Corr	act on Voter Turnout tact Rate (27.87%) = 8.7% Standard Error (2.6)	

wise in the neighborhood solidarity condition were given an appeal based on civic duty.

RESULTS

In presenting the experimental results, we first provide tables that show the basic findings on personal canvassing. These convey the main findings in an accessible fashion and also illustrate our statistical method for handling contact rates. We then present a regression analysis that confirms the results but allows us to estimate these and other experimental effects with more statistical precision.

The Personal Canvassing Experiment

To underscore the importance of methodological nuance in interpreting the effects of canvassing, Table 3 shows the effect of in-person contact on turnout in more than one way. The upper half of the table compares the turnout rate of those who were contacted face-to-face with those who were not. The group not contacted, however, is a combination of the control group (whom we made no attempt to reach) and those in the treatment group whom the canvassers were unable to reach. In some previous experimental studies (e.g., Adams and Smith 1980; Eldersveld 1956), the treatment effect is calculated only by this method of measuring the difference in turnout rate between those who were contacted and those who were not. Nonexperimental studies using survey data implicitly take a similar approach as well, since these compare the voting rates of those who report contact and those who do not. In our study the difference between the voting rate of those contacted and those not contacted is very large, 14.6 percentage points.

This number doubtless overestimates the effect of canvassing. If voters who are easier to reach are also

more likely to vote, then the canvassing effect is partly spurious. To estimate the effect of the treatment properly, we must distinguish the treatment effect from the higher probability of voting among those easier to contact. One way to do this is to augment the experimental design. We might have added another control group—those with whom canvassers make contact and deliver a nonpolitical message. An alternative is to derive an estimator making use of the fact that the group we intended to treat is a random subset of the entire sample, and therefore the proportion of easy-to-contact voters is the same in the treatment and control groups. We adopted this second approach, which is elaborated by Angrist, Imbens, and Rubin (1996).

To isolate the treatment effect, we reason as follows. Suppose that a subset of the experimental group is contacted. For any given level of canvassing effort, the population can be divided into two groups, those who are reachable and those who are not. Let α be the proportion of the population that is reachable. Let p_m be the probability that a nonreachable person votes, let p_r be the probability that a reachable person votes without the experimental treatment, and let $p_r + t$ be the probability that a reachable person votes after being exposed to the experimental treatment. Our aim is to estimate the value of t. The probability that a randomly selected member of the control group will vote equals:

$$P_C = \alpha p_r + (1 - \alpha) p_{rr} \tag{1}$$

This probability equals the probability that an individual member of the control group is "reachable" (α) times the probability a reachable person votes (p_r) plus the probability the person is not reachable (1 $-\alpha$) times the probability a person of this type votes (p_{rr}). The probability that a randomly selected member of the treatment group will vote equals:

TABLE 4. Effects of Personal Canvassing on Voter Turnout, by Type of Nonpartisan Appeal Number of Registered Voters Number of Persons Type of Appeal Turnout Rate In Treatment Group **Actually Contacted** Unadjusted Turnout Rates among Experimental Subgroups CMc duty 47.2% 1,985 534 Neighborhood solidanty 46.3% 1,881 546 Election is close 48.1% 1,928 535 Control 44.8% 23,586 N/A Implied Effects of Personal Contact on Voter Turnout Turnout Differential (2.43%)/Contact Rate (26.90%) = 9.1% Standard Error (4.3) Neighborhood solidartty Turnout Differential (1.48%)/Contact Rate (29.03%) = 5.1% Standard Error (4.1) Election Is close Turnout Differential (3.36%)/Contact Rate (27.75%) = 12.1% Standard Error (4.2)

$$P_E = \alpha(p_r + t) + (1 - \alpha)p_{rer}, \qquad (2)$$

where the difference between equations 1 and 2 is due to the effect of the experimental treatment. Combining equations 1 and 2, we derive an expression for t:

$$t = \frac{P_E - P_C}{\alpha}. (3)$$

Although the population probabilities are not observed, sample data can be used to obtain an estimate of t. First, using the law of large numbers,

$$plim V_E = P_E, \qquad plim V_C = P_C, \qquad (4)$$

where V_E is the percentage of the treatment group that votes, and V_C is the percentage of the control group that votes. Similarly,

$$\frac{N_r}{N_E} = \alpha, (5)$$

where N_r is the number of subjects in the treatment group who were reached for the experimental treatment, and N_E is the number of subjects in the treatment group overall. Using equations 4 and 5, we obtain a consistent estimator of t:

$$plim \frac{V_E - V_C}{\frac{N_r}{N_E}} = t. ag{6}$$

Equation 6 says that, to find the treatment effect, subtract the turnout rate of the control group from the turnout rate of the experimental group and divide this difference by the observed "contact rate," which is 28%. Using this formula, we find that personal contact raises the probability of turnout by 8.7 percentage points, with a standard error of 2.6. The null hypothesis that canvass-

ing does nothing to increase turnout can be decisively rejected, using a one-tailed test (p < .01).8

Table 4 suggests that the effects of personal contact do not vary significantly across messages. The close election message boosts turnout rates by 12.1%, which is slightly better than the 9.1% associated with the civic duty appeal and substantially better than the 5.1% for neighborhood solidarity. These findings are suggestive, but the standard errors associated with the estimates are far too large to reject the null hypothesis that the messages have equal effects. Looking ahead to the experiments using direct mail and telephone calls, we find a similar pattern of insignificant differences across messages. Since we cannot rule out the view that any plausible mobilization appeal works equally well, the analysis that follows focuses exclusively on the relative effectiveness of delivering the appeal in person, by telephone, or through the mail.

Regression Results

Regression analysis permits us to conduct a more comprehensive analysis, taking into account all the treatments in our experiment. Regression analysis has the further virtue of introducing covariates, such as past voting history, that reduce the unexplained variance in voting rates and allow for more efficient estimation of the experimental effects. For reasons cited above, however, any regression analysis must attend to the possibility that subjects with a higher propensity to vote are easier to reach in person.

Consider the following simple model of how the experimental treatment affects turnout. Suppose again, for purposes of illustration, that the population can be divided into those who are easy to contact and those who

⁸ These results remain unchanged when we disaggregate the data according to whether the household contains one or two registered voters. For single-voter households, the effect of personal contact is estimated to be 10.0% (SE = 3.7), compared to 8.2% (SE = 3.6) for two-voter households. These estimates are too similar to be differentiated statistically (p > .10).

TABLE 5. Linear and Nonlinear Regression of Voter Turnout on Mode of Contact, with and without Covariates

	Two-Stage Least Squares		Two-Stage Probit	
independent Variables	Coefficient (SE)	Coefficient (SE)	Coefficient (SE)	
Personal contact	.087 (.026)	.098 (.022)	.323 (.074)	
Direct mallings (0 to 3)	.0058 (.0027)	.0063 (.0023)	.0214 (.0067)	
Telephone contact	047 (.023)	035 (.020)	130 (.056)	
Registered as Democrat or Republican		.064 (.006)	.217 (.015)	
Voted in 1996 general election		.229 (.007)	.589 (.018)	
Abstained in 1996 general election		231 (.008)	824 (.024)	
Age		.0188 (.0008)	.0649 (.0022)	
Age squared		000133 (.000007)	000467 (.000020)	
Number of registered voters in household (1 or 2)		.058 (.005)	.188 (.014)	
Constant F	.445 5.86	296.66		
Degrees of freedom	29,376	29,342	29,342	

Note: The base category for past voting behavior is the set of people who were not registered in 1996. Not reported in this table are the coefficients associated with each of the 29 werds. The first-stage equations include dummy veniables representing the intent-to-treat groups associated with canvassing, phone calls, and direct mail. The first-stage equation also includes covariates for columns 2 and 3 Standard errors for the two-stage probit estimates were obtained using jackknifting.

are not. The probability that a given person in the experiment votes may be expressed as

$$Y = a + b_1 X_1 + b_2 X_2 + e,$$

where Y=1 if the subject votes, $X_1=1$ if the subject is difficult to contact, and $X_2=1$ if the subject is actually contacted; 0 otherwise. Given that X_1 is not observed, we might ignore this variable and regress Y on an intercept and X_2 . This will yield a consistent regression coefficient estimate only if X_1 and X_2 are uncorrelated, or if b_1 equals 0. These special conditions cannot be expected to hold. Unless everyone in the

treatment group is contacted, there will be some correlation between how easy it is to reach a subject and the likelihood they are actually reached. It is also quite reasonable to assume that those who are very hard to reach may also be less likely to vote (i.e., b_1 does not equal 0). Although these points seem straightforward, they have eluded previous research in this area.⁹

The standard solution to the problem of correlation between a right-hand-side variable and the regression error is to find a suitable instrumental variable. In this case, an ideal instrument is at hand. Recall that a valid instrument satisfies two criteria: The variable must be uncorrelated with the regression error, and it must be correlated with the endogenous variable. The probability that subjects are contacted is a function of whether they are randomly selected for the treatment group. This implies that a dummy variable which equals 1 for subjects in the treatment group will be correlated with the endogenous variable. Because the treatment group is generated through random assignment, there is no reason to suppose that those who are easy to contact will be overrepresented. Thus, the expected correlation between the instrumental variable and the regression error is zero.

Table 5 presents two-stage least-squares regression estimates of the effect of each experimental treatment. As indicated earlier, the instrumental variables used in the regressions indicate whether the person was in a given treatment group. For example, the variable Personal Contact equals 1 if the subject was contacted, and the instrumental variable equals 1 if the person was in the group that we intended to treat. Note that the instrumental variable will be correlated with the included variable (being in the intent-to-treat group predicts the likelihood that one is contacted), but the instrumental variable is not correlated with the regression error (treatment group status is due to random assignment). A similar procedure applies to the telephone experiment, with intent-to-treat serving as an instrument for actual contact. For the mail experiment, the instrumental variable and the independent variable are the same, since the assumed contact rate is 100%.10

Official voting and registration records contain useful information about the sample. For example, we know whether a person voted, abstained, or was absent from the voter rolls in the 1996 general election. We also know an individual's age, party registration, voting ward, and whether s/he is the sole registered adult in the household or is one of two. Each of these covariates contributes significantly to the predictive accuracy

⁹ Consider some of the seminal work in this area. Kramer (1970) interprets the higher turnout rate among those reached by a party or candidate as the marginal effect of contact. In the classic study by Eldersveld (1956), those unavailable for personal contact were moved into the control group. This practice results in overestimation of the treatment effect.

¹⁰ Our calculations assume that all of the households we intended to treat by mail received the treatment, an assumption implicitly made in all previous mail experiments. In our case, the voter lists were very current and fewer than 1% of the mailings were returned. To adjust the estimated effects for any failure to receive the mail, divide the coefficients in Table 5 by the supposed contact rate.

of the model, which makes for more precise estimates of the experimental effects. Moreover, this background information affords an opportunity to look for interactions between treatment effects and individual attributes.

The multivariate model enhances slightly the apparent effect of personal and mail appeals. Face-to-face contact raises turnout by 9.8 percentage points, and direct mail raises turnout by .6 percentage points for each mailing. Because our most intensive treatment involved only three mailings, we cannot reliably extrapolate out very far. Direct mail vendors informed us that a regimen of 4 to 9 mailings is common in political campaigns. If this practice is grounded in a correct assessment of how mailings stimulate turnout, we might have drawn even more voters to the polls with additional mailings.

One of the most surprising results to emerge from our experiment is the ineffectiveness of telephone appeals. The commercial firm we retained routinely does this kind of work on behalf of parties, campaigns, and interest groups, often on a very large scale, so it was well qualified. Nevertheless, we find no indication whatsoever that telephone appeals raise turnout. The turnout rate for the treatment group (44.5%) was lower than the rate for the control group (45.5%). (The turnout rate among those actually contacted by phone was 60.7%, but this number is evidently a spurious reflection of the unobserved characteristics of people who are reachable by phone. This result reinforces our concerns about previous work based on nonexperimental data.) Taking contact rates into account and controlling for other experimental features, telephone calls would seem to have diminished turnout slightly, a conclusion that remains unchanged when we look separately at the civic duty and close election messages. Given our initial expectation that telephoning increases turnout, we take this result to mean that the null hypothesis of no effect cannot be rejected using a one-tailed test.

Although negative results such as these are often viewed with some disappointment, they are not uninformative. Both the phoners and personal canvassers communicated the same information, but only the latter influenced subsequent behavior. Reminding people that an election is imminent has no discernible effect per se on turnout. Precisely what distinguishes personal contact is open to speculation. It may make information more salient and memorable, may trigger a feeling of connectedness to the electoral system, or may more credibly convey the urgency of the request.

In analyzing our mode of contact by experiment, we have focused on main effects associated with the randomly manipulated regressors. Augmenting the regression models to allow for interactions, we find them to be jointly insignificant (p > .05). For example, mail campaigns do not seem to amplify the effectiveness of personal canvassing, or vice versa, regardless of the content of the appeal. Telephone calls neither increase nor decrease the effectiveness of mail or personal appeals. We find no evidence of a second-order interaction among telephone, mail, and personal contact.

The additive nature of these effects runs counter to the hypothesis that get-out-the-vote drives, in and of themselves, signal the importance of the election, thereby raising turnout. If this were so, then those who received a mailer and a telephone call might have been especially impressed by the scope of our Vote '98 campaign. Instead, they reacted much as would be expected based on the behavior of those who received only mail or telephone calls.

Interpretation of the nonexperimental coefficients is clouded by the fact that these regressors gauge voting propensities in an overlapping and incomplete fashion. The effect of party registration, for example, must be understood in light of the fact that we controlled for participation in the 1996 election but not for education. For this reason, we must be cautious about interpreting these coefficients or comparing them to survey-based results. As expected, voter turnout tended to be higher among those who voted in the previous election, were registered with a major party, were older (but not very elderly), and were part of a two-voter household. Taken together, these control variables predict approximately one-quarter of the variance in voter turnout.

We do not find any significant interactions between these background characteristics and the effectiveness of various get-out-the-vote interventions. For example, augmenting the two-stage least-squares regression model to include interactions between personal canvassing and age, past voting history, and major party registration does not significantly enhance the fit of the model (p > .10).¹¹

Finally, the pattern of main effects we report in Table 5 is amplified somewhat when we use the two-stage probit estimator proposed by Rivers and Vuong (1988) to model the probability of voter turnout. Holding all the covariates constant, the probit regression coefficient of .32 implies that personal canvassing raises turnout from 44.5% (the control group in all experimental conditions) to 57.3%. This change of 12.8 percentage points is somewhat larger than the 9.8 percentage points obtained using least squares. Probit regression also suggests slightly stronger effects for direct mail. Again, taking 44.5% as a baseline, the probit estimate of .02 implies that three mailings increase turnout by 2.5 percentage points. As before, telephone calls did nothing to increase turnout.

Nonlinear models thus reinforce the central finding that personal forms of mobilization tend to be more effective. Consider the effects of mobilizing a set of average households in the control group, whose baseline probability of voting is 44.5%. At fifty cents per

¹¹ When we use all of the covariates to generate predicted probabilities of voting and divide the sample into five groups according to their baseline probability of voting (below 20%, 20–40%, 40–60%, 60–80%, or above 80%), the data do not suggest that personal contact has differential effects across these categories. Granted, the contact rate is much higher for those with the highest propensity to vote (38.7%, n = 2,209) as opposed to those whose likelihood of voting is less than one-in-five (21.5%, n = 7,544), but the effectiveness of personal contact (in logistic units) does not vary significantly. By the same token, we have no evidence that the effectiveness of mail or telephone calls varies depending on the individual's baseline likelihood of participation.

mailer, sending three mailings to each household (containing an average of 1.5 voters) nets roughly one additional voter for each \$40 spent. Similar calculations, using \$1.50 as the cost per personal contact (10 contacts per hour at \$15 per hour), produce an estimate of approximately one more voter for each \$8 spent. To be sure, organizing and supervising a canvassing campaign involves significant fixed costs, but even if the effective marginal costs of canvassing were doubled, face-to-face mobilization would still be cost effective.

CONCLUSION

Due to its size and randomized design, our experiment lends precision and nuance to the extensive literature on voter mobilization. In particular, our findings suggest the importance of differentiating between personal and impersonal modes of political contact. Face-toface interaction dramatically increases the chance that voters will go to the polls. In our study, personal canvassing had a far greater influence on voter participation than three pieces of professionally crafted mail delivered within two weeks of election day. Less effective than direct mail were calls from professional phone banks, 12 Despite our efforts to encourage callers to deliver messages in an engaging, conversational style, the telephone appeals were unmistakably routinized and scripted, and it is possible that recipients detected the fact that the calls were from another state. (The use of out-of-state phone banks in campaigns is by no means atypical. In the New Haven mayoral election of 1999, phone callers had distinctive out-of-state accents.) Whatever personal touch might be conveyed over the telephone undoubtedly diminishes as callers plow through long lists. Our findings concerning voter turnout mirror previous research on blood donation, which shows mail and telephone appeals to be much less effective than face-to-face requests (Jason et al. 1984).

The magnitude of the canvassing effect coupled with the limited influence of direct mail and telephoning lends credence to our hypothesis that falling rates of voter turnout reflect a decline in face-to-face political activity. This hypothesis has been overlooked in previous analyses, which have largely relied on survey data. Surveys rarely draw a distinction between face-to-face and telephone contact; even if they do, they are ill-equipped to gauge the relative effectiveness of personal and impersonal modes. Unlike nonexperimental studies, our research establishes a causal link between canvassing and turnout, and it can survive the charge that canvassing is directed at people with a greater propensity to vote or that the apparent correlation between contact and voting is due to misreports of one or both variables. Contacts were manipulated on a random basis, so that we did not have to rely on respondents to recall whether or how they were contacted. Since the study used public documents to measure voter turnout, none of the experiment relied on self-reports.

Although the size and scope of our study represents a significant advance over previous experimental and nonexperimental research, it leaves certain questions unanswered. First, the messages we employed were strictly nonpartisan. Our findings are consistent with the hypothesis that declines in voter turnout reflect massive retrenchment in the size and vitality of nonpartisan and civic organizations (Putnam 2000), but they speak only indirectly to claims concerning partisan mobilization. Although previous canvassing experiments found both partisan (Miller, Bositis, and Baer 1981) and nonpartisan (Eldersveld 1956) appeals effective, no studies compare the two directly. Whether both types of mobilization have similar effects is thus a question that awaits future experimentation. It may be that the negative tone of certain partisan appeals, particularly when communicated through impersonal means, actually demobilizes certain segments of the electorate, although the evidence is currently mixed (Ansolabehere and Iyengar 1995; Lau et al. 1999).

Second, the small but discernible effect of direct mail implies that the growth in impersonal mobilization may offset some of the decline in personal mobilization. (A similar offsetting pattern may apply to phone banks if, as the results from Adams and Smith [1980] and Miller, Bositis, and Baer [1981] imply, partisan appeals are more effective than the nonpartisan messages we examined.) Our findings are consistent with the notion that declining turnout reflects the disappearance of face-to-face mobilization, but a more thorough understanding of the effects of alternative forms of mobilization is required in order to gauge the historical significance of transformations in both partisan and nonpartisan organizations. In particular, students of electoral politics need to investigate the cumulative consequences of increasing reliance on mail and telephone (as well as the Internet). It may be that over time these modes of contact face the same problem as conversations in a noisy cafeteria: As campaigns must raise their voices louder and louder in order to be heard, voters pay less attention to each message. In 1925, direct mail sent by Harold Gosnell increased turnout by 9 percentage points. Today, the volume of direct mail is vastly greater, and the effect of each piece is much smaller, less than one percentage point per mailer.

Further research also is needed to confirm the generality of our results. It remains to be seen whether they hold in other settings and types of elections, and we are currently engaged in efforts to extend the experiments. Studying the effects of personal canvassing and other forms of contact across different electoral contexts should be viewed as much more than mechanical replication. The distinction we have drawn between personal and impersonal modes of contact suggests the following cross-sectional or cross-temporal hypothesis: Personal contact is more influential when campaigns and organizations emphasize impersonal mobilization. To establish this proposition requires attention to the interaction between the political environment and various mobilization strategies.

Even if our findings have external validity, an impor-

¹² A parallel get-out-the-vote experiment using the same telemarketing campaign in a neighboring city also produced slightly negative effects, based on a sample size of 5,855 in the treatment group and 8,883 in the control group, with similar contact rates.

tant gap remains: We know very little about the mechanism by which personal contact influences voting behavior or why impersonal forms of contact have less effect. Our experiments do not tell us whether personal contact enhances interest in politics, feelings of connectedness to the electoral system, the belief that elections are important, or a sense of obligation to participate. Now that we have a clear indication that canvassing does indeed affect behavior, we plan to augment future experiments with a postelection survey in order to assess the psychological imprint left by canvassers.

Despite limitations, this experiment provides important new clues in the ongoing mystery of why turnout has declined even as the average age and education of the population has risen. A certain segment of the electorate tends not to vote unless encouraged to do so through face-to-face contact. As voter mobilization grows more impersonal, fewer people receive this kind of encouragement. This point is of great practical significance for those who seek to reverse the declining trend in turnout. Many of the recent policy innovations designed to encourage voter participation (e.g., absentee balloting) focus on reducing the costs of voting. Our findings suggest the importance of focusing as well on the personal connection between voters and the electoral process. Face-to-face mobilization efforts have a demonstrable effect on voter turnout. The question is whether the long-term decay of civic and political organizations has reached such a point that our society no longer has the infrastructure to conduct face-to-face canvassing on a large scale.

APPENDIX: DESCRIPTION OF MAILERS

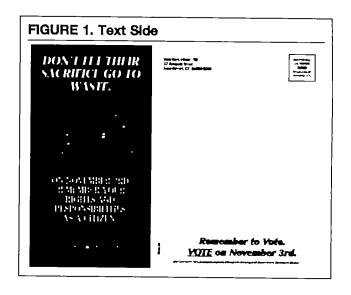
Postcard Texts

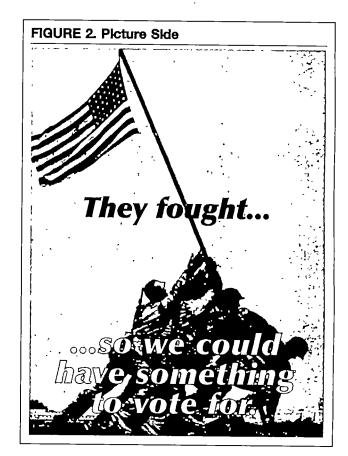
The cards were designed and written (in consultation with the authors) by a professional firm that specializes in political direct mail. All nine cards had the same basic layout. Each was 8×12 and had a different picture that covered the entire front side, with some text superimposed on the picture. The reverse side contained only text. As described below, for each of the three messages, a large portion of the text was the same for all cards in the same message category. A description of the nine cards used in the experiment is provided here, along with the text for each. In addition, we include a reproduction of one card to show the layout.

Civic Duty

The card mailed 8 days before the election is shown in figures 1 and 2. The card mailed 13 days before the election used a picture from the civil rights "March on Washington," of the Washington Monument and the Mall filled with people. The text read: "What Were They Fighting For? They Were Fighting for Our Right to Vote. The whole point of democracy is that citizens are active participants in government, that we have a voice in government. Your voice starts with your vote. On November 3d remember your rights and responsibilities as a citizen. Remember to vote."

The card mailed 15 days before the election had a picture of the first page of the U.S. Constitution. The text read: "We The People... Have a Duty. We have a Duty to Vote." It also carried the same text as the previous card, beginning with "The whole point...."





Neighborhood Solidarity

The card mailed 8 days before the election used a picture of a crowd of people holding up their hands. The text read: "There is Strength in Numbers. Stand Up and Be Counted. When people from our neighborhood don't vote we give politicians the right to ignore us and concentrate their energies elsewhere. But you can make sure that doesn't happen. By joining your neighbors and voting on election day, you'll send a message to our elected leaders: that you care, and that they should care about your concerns. On November 3d Vote to ensure that we are not ignored. Remember to vote."

The card mailed 13 days before the election had a picture of several houses on a city block. The text stated: "Stronger Neighborhoods Start With You. They Start With Your Vote. Our elected officials listen to one thing only—our votes." It also carried the same text as the previous card, beginning with "When people from "

The card mailed 15 days before the election showed people crossing a city street. The text read: "Are Your Values Going to be Represented on Election Day? Only if You Vote." It repeated text from the previous card, beginning with "When people from "

Election is Close

The card mailed 8 days before the election had a picture of two children at a table in a classroom. The text stated: "Their Future Starts with One Vote. Yours. In an election, anything can happen. This year many elections will be decided by only a handful of votes will yours be the deciding vote? Don't miss your opportunity to make a difference, don't miss your chance to make an impact in our elections. On November 3d make sure your vote is included, because no election is ever a certainty and every vote counts. On November 3d don't miss your opportunity to make a difference. Remember to vote.

The card mailed 13 days before the election showed Harry Truman holding up the headline "Dewey Defeats Truman." The text read: "History has Shown the Importance of Your Vote. Vote and Be a Part of History." It used the same text as the previous card, beginning with "In an election '

The card mailed 15 days before the election used a picture of a street sign with an arrow pointing in opposite directions. The text read: "What direction will the country head in? It's up to you and your vote." It carried the same text as the previous card, beginning with "In an election "

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Congressional Voting over Legislative Careers: Shifting Positions and Changing Constraints

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The proposed model predicts that voting behavior of legislators is more variable early in their career and that junior members are more likely to vote with their party than senior members. The results from the analysis of voting patterns in the House of Representatives and the Senate are consistent with the hypotheses: Party line voting and variability of voting decisions decline with increasing seniority. Changes in voting behavior are also induced by redistricting. The empirical results show that legislators subject to redistricting change their voting behavior to accord better with altered constituency preferences.

his article examines changes in voting behavior over legislative careers by contrasting veteran and junior legislators. I also examine variations in voting behavior that follows redistricting and constituency changes.¹

Junior legislators often do not have enough information about what their constituency expects from them and how it will respond to their voting decisions. In a process of trial and error, those decisions are adjusted, which leads to variability in voting records. Guides to voting include party policy and cues from colleagues. As time progresses, however, legislators receive more information about their constituency and rely less on the party line and their colleagues. Thus, their voting behavior may change over time.

One event that may alter voting behavior is a change in constituency caused by redistricting, which can be viewed as an exogenous variation in constituency preferences. Such events allow the examination of legislators' responses to changing constraints, that is, constituency preferences, while holding other legislator characteristics constant. In the absence of such an exogenous change, empirical work faces the problem of distinguishing between legislator and constituency preferences.² In particular, the researcher would not know whether a change in voting behavior is attributable to legislator idiosyncracies or to constituency factors. Redistricting that substantially alters the con-

stituency provides a natural experiment for determining whether legislators change their voting behavior as the result of representing a new electorate.

This article uses roll call voting data from recent election cycles and constructs a panel data set to estimate the parameters of interest. It controls for legislator characteristics and nontime-varying constituency characteristics via fixed effects, as well as for observable time-varying constituency characteristics, and it tests whether changes in seniority and alterations in district boundaries cause alterations in legislators' positions. The results show that voting decisions do change systematically over the course of legislative careers. Early on, legislators follow the majority of their party, but party line voting declines as they gain seniority. In addition, as measured by absolute changes in the Americans for Democratic Action (ADA) voting index, voting behavior is more variable early in careers and more stable as tenure increases. With respect to redistricting, the analysis shows that legislators change their voting behavior to accord with the preferences of the new median voter.

Redistricting provides a way to test for the responsiveness of legislators to changing constituency characteristics. We cannot simply include constituency characteristics in roll call vote regressions because, if voters elect representatives whose beliefs are identical to theirs, we could not determine whether legislators are voting their own preferences or those of the constituency. A test of the effects of redistricting on voting behavior overcomes the bias in the estimated constituency coefficients.

Historically, redistricting has led to an attrition of incumbents, either because they choose not to run or because they lose the election. A loss also may be interpreted as evidence that they were unwilling to adjust their positions. This study helps determine whether the winning incumbents adjusted their positions in response to the new electoral environment.

In the next section I present a model that illustrates the effect of changing constraints on legislators' voting behavior and derive implications about the effect of seniority and redistricting that will guide the empirical work. The empirical models and data sources are discussed in the following section. I then present results, and conclusions are drawn in the final section.

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¹ Previous studies of change in voting behavior over legislative careers include Bernstein 1988; Francis and Kenny 1996; Glazer and Robbins 1985a, 1985b; Grofman, Griffin, and Berry 1995; Grofman, Griffin, and Glazer 1990, 1991; Poole and Daniels 1985; Rothenberg and Sanders 1999; and Thomas 1985. Most of these found no or very little change. Francis and Kenny (1996) show that House members who run for the Senate move to the ideological center in the years before running. Other work addresses whether legislators vote differently in the term prior to retirement (see, e.g., Lott 1990; Lott and Reed 1989).-

² If legislators' preferences are correlated or coincide with constituency preferences, it is difficult to determine unambiguously how constituency preferences affect their voting behavior. A simultaneous equation bias arises when unobserved legislator characteristics are correlated with constituency characteristics and voting behavior.

RESEARCH HYPOTHESES

Implications from Informational Constraints

Just as factory workers learn to perform tasks more efficiently, over time legislators learn about constituency preferences. They acquire this information through trial-and-error voting, election campaigns, and feedback from their electorate. Also, junior legislators are more likely than senior legislators to seek cues on voting from colleagues, because they often have little information about how their votes will affect their reelection chances (Kingdon 1989; Matthews and Stimson 1975).³

The process of trial and error also operates in finding the colleague who gives the best cues. Legislators may follow advice on how to vote that results in disapproval from their constituency. In such cases, they have an incentive to adjust subsequent voting decisions (assuming they want to maximize reelection chances), and this leads to variability in their voting record. In general, if it is assumed that junior legislators have an informational disadvantage, then it follows that they will adjust their position more frequently, and their voting behavior will be more variable than that of senior legislators.

The proposed learning model also has implications for party line voting. Democratic freshmen know that their reelection constituency's preferences are close to the platform of the Democratic Party, and Republican freshmen have similar information about their reelection constituencies. Therefore, legislators who are uncertain about the specific preferences of their reelection constituency have an incentive to vote the party line, because it will be, on average, closer to the position of their reelection constituency than will be the position of the other party. Junior legislators are more likely to be uncertain about preferences and thus have an incentive to vote with their party. They learn about the party position through cues from same-party colleagues (Kingdon 1989).

³ The hypothesis of learning about constituency preferences is drawn from probabilistic voting models. In Downs's (1957) model, candidates know exactly where voters are located in space, and they adjust their platforms accordingly. The probabilistic models assume that candidates are unsure of voter positions and choose a platform that maximizes expected votes. Although the probabilistic framework has not been extended to multiple periods, it is reasonable to hypothesize that winning candidates improve their knowledge about constituency preferences while in office, and they thus have an advantage in choosing their platform in the next election relative to a challenger who has had less opportunity to learn about constituency preferences. Incumbents can adjust their platform and increase the probability of reelection.

⁴ For the influence of parties on congressional voting decisions, see, for example, Cox and McCubbins 1993 and Rohde 1991.

Less informed members have an incentive to take an "average position," rather than an ideologically extreme position. Given the distinct characteristics of the two major parties' constituencies, following the party line in the absence of information is probably the lowest cost way, on average, to make decisions that maximize the chances of reelection. A position that is "typical" for the party or the home district minimizes the likelihood of strong negative feedback from constituents. Although they may start from the party position, as legislators gain more information with increasing seniority, they have an incentive to move closer to the position of the electorate in order to maximize reelection chances.

If legislators learn with increasing tenure that their constituency's position is similar to that of the party, then they will continue to vote along party lines. If they learn this is not the case, then they will break step in order to maximize reelection chances. Therefore, more party line voting is predicted for junior than senior members. This hypothesis is consistent with Kingdon's (1989) finding that cues come heavily from own-party members, but senior legislators also turn to informants outside their party, which in turn may lead to less party line voting. The hypothesis that junior members have an informational disadvantage is also consistent with evidence from the 1960s that they tried to "learn the process" by relying on more senior members for information (Fenno 1966).

Some institutional features of the U.S. Congress also may prompt junior legislators to vote along party lines. Party support is important not only for winning an election but also for obtaining committee assignments, which are decided by each party's congressional caucus.8 Partisan criteria determine that process (e.g., Rohde and Shepsle 1973), and legislators who vote with the party are more likely to obtain coveted assignments. In addition, leadership endorsement is important (Shepsle 1978), and legislators who vote more often with leaders have a higher probability of being transferred to preferred committees (Coker and Crain 1994; Cox and McCubbins 1993). Sinclair (1995) reports that in nominating speeches for exclusive committees, the nominee's party loyalty is praised by the leadership. In summary, empirical findings and anecdotal evidence show that it pays for legislators to vote with their party and its leaders if they want an exclusive committee assignment. Once it is obtained, however, promotion to higher ranks in the committee is via the seniority rule. Because senior legislators do not have to rely on their party for promotion, they are less dependent on party approval than are junior members, who are still seeking preferred assignments or transfers. Therefore, the incentive to vote along party lines weakens with increasing seniority.9

⁵ Democratic and Republican legislators come from significantly different constituencies (Kingdon 1989; Stratmann 1996), so their voting decisions differ.

⁶ Truman (1959) hypothesizes that junior legislators stay close to the orthodox party position because they are new to the chamber.

⁷ The party leadership is perhaps not a major influence on voting decisions, at least to the extent of forcing a legislator to vote against his or her constituency, but it has an informational advantage relative to junior legislators, especially on issues of low salience. Consequently, the leadership may have some influence on junior members' voting decisions.

Before 1910 (the Cannon revolt) the Speaker made all majority and minority party assignments.

⁹ The predictions from this model and the information-based model are in contrast to the model that postulates voters elect ideologues (i.e., legislators with fixed platforms). In other words, legislators' voting behavior is fixed, and no changes occur over time.

Implications from Downs's Median Voter Model

Downs's (1957) model, which is based on the key assumption that candidates will maximize their chances of reelection, has implications for voting behavior. Candidates are political chameleons who adopt whatever position is necessary to win the maximum number of votes. The model predicts that the voting decisions of incumbents are a function of the preferences of their constituency, not of the representatives themselves. The link between constituency preferences and legislative voting decisions is broken once a legislator is freed from reelection constraints.

Incumbents typically do not seek election in different districts, but redistricting, which changes geographical boundaries, offers an opportunity to test the Downsian hypothesis. The Downsian framework also implies that legislators will change their voting behavior in accord with preferences of their electorate and will adjust their platform if redistricting results in constituency preferences that differ from those of the old district. In the previous section, it was assumed that junior legislators' knowledge of their constituency grows at a decreasing rate over time. The Downs model suggests that legislators have perfect knowledge of their constituency as soon as they run for election in a new district. Although the Downs model and the learning model both predict that voting behavior will change upon redistricting, the Downs model predicts a full adjustment in the election cycle immediately after redistricting, while the learning model implies an adjustment over several election cycles. The median voter is the theoretical basis for both models, but the predictions of the informational model are based on cue-taking and on the hypothesis that reelection constituencies differ between the two parties, whereas Downs focuses on the median voter of the entire constituency. Empirical evidence shows that roll call vote decisions are influenced by the preferences both of the reelection constituency and the median voter (Stratmann 1996).11

EMPIRICAL MODEL AND DATA

The models in the previous section predict that legislators change their voting behavior over time. The hypothesis that junior legislators are less likely to vote with the party as they gain tenure is tested with the following:

PartyVote_{$$\mu$$} = β_1 Seniority _{μ} + $\beta_2 X_{\mu}$ + μ_i + γ_j + ϵ_{μ} , (1)

where the unit of observation is legislator i in cycle j. PartyVote is the party unity measure of the Congressional Quarterly Almanac. This variable is calculated from votes in which the majority of Republicans oppose the majority of Democrats, and it measures the percentage of roll call votes in which legislators side with their party majority. Two measures of the party unity vote are used. In the first, failure to vote lowers the score. In the second, failure to vote does not lower the score. As does the literature that measures the frequency of "yes" votes, I will use the minimum χ^2 method to estimate the regression model (see, e.g., Greene 2000; Kalt and Zupan 1984). ¹² Covered in this analysis are each election cycle from 1983–84 to 1993–94.

Equation 1 states that party line voting is a function of seniority, legislator indicators (μ_i) , year indicators (γ_j) , and constituency and legislator characteristics that change over time (X_{ij}) . The seniority variable is measured as a logarithmic transformation of the number of election cycles a legislator has served in Congress, because the incentive to vote with the party leadership should diminish for a legislator over time. The proposed model predicts a negative coefficient on seniority.

Legislator fixed effects (μ_i) in equation 1 control for observed and unobserved characteristics of legislators as well as constituencies. Because a district can be represented by more than one legislator over the election cycles analyzed, legislator fixed effects subsume district fixed effects. They also allow for some change in constituency preferences at the date of the turnover from the previous to the new representative. For example, if one legislator represents a specific district in the first years and another represents the same district in later years, two fixed effects are associated with that district. Year cycle effects (γ_I) control for particularities in each session of Congress. The X_n vector includes an indicator for redistricting, an indicator for legislators who have announced retirement, the percentage of the popular vote received in the general election, and constituency characteristics that change over time.

The effect of the percentage of the popular vote on party unity is ambiguous. Unlike competitive seats, a safe seat makes party line voting low risk, because a loss of some votes is less likely to spell defeat in the general election. Thus, legislators with safe seats may cast more party line votes. Yet, safe seats require less financial help from the party, and this independence may lead to less party line voting.

Since unobserved or unmeasured constituency characteristics can create safe seats and also can lead to high party line voting, the percentage of votes received by the legislator or party in the general election may be endogenous in equation 1. Although this variable is not of central interest here, I correct for the potential

¹⁰ Studies on the relative importance of legislator ideology and constituency preferences include Bender 1991; Bullock and Brady 1983; Coates and Munger 1995; Dougan and Munger 1989; Kalt and Zupan 1984, 1990; Kau and Rubin 1979; Levitt 1996; Lott 1990; Lott and Davis 1992; Peltzman 1984; Poole and Romen 1993; Poole and Rosenthal 1985, 1997; and Zupan 1990. A partial list of research on other issues surrounding roll call votes includes Amacher and Boyes 1978; Crain, Leavens, and Tollison 1986; Fiorina 1974; Kingdon 1989; Krehbiel 1991; Snyder 1992; and Stratmann 1995, 1996.

¹¹ Stratmann's (1996) measure of reelection constituency preferences is the residual from a regression equation that explains legislators' party affiliation. This residual explains differences in legislators' voting behavior.

¹² The robustness of this specification is examined in the next section.

endogeneity by including the predicted general election percentage of the popular vote for each candidate, rather than the actual percentage in the party unity regressions, by following a two-stage least-squares procedure. The rank of the percentage serves as the instrument (Koenker and Bassett 1978).¹³

Redistricting may affect party line voting because it implies a new constituency. In my sample, redistricting occurred for the 1992 election. Legislators are classified as running in a different district if the geographic area is more than 50% different from previous boundaries.¹⁴ For these legislators, the redistricting variable equals 1 in the 1993-94 cycle, 0 otherwise. The net effect of the redistricting indicator on party line voting in equation 1 is not predictable,15 except in some special cases. For example, if a Republican legislator's constituency is more conservative after redistricting. s/he may vote more often with the party than previously. Nonetheless, more party line voting is not necessarily implied by this example. Because that constituency could now perhaps be less typical for the Republican Party, less party line voting could result. To test whether changed preferences of the electorate alter a legislator's voting behavior, I estimate a modified version of equation 1:

PartyVote_n =
$$\beta_1$$
Seniority_n + $\beta_2 X_\mu$
+ β_3 (Redist Δ PresV_n) + β_4 (Redist Δ PresV
× Republican_n) + μ_1 + γ_1 + \in_n .

(2)

Redist $\Delta PresV_{j_1}$ measures the change in the presidential vote in each congressional district affected by 1992 redistricting, that is, its percentage of the electoral vote for Clinton in November 1992 minus the corresponding percentage for Dukakis in 1988 (calculated as 100 minus the district percentage received by Bush). Since this variable is designed to measure the change in constituency preferences associated with redistricting, it is zero for the years before 1993. Large positive values of Redist $\Delta PresV_{j_1}$ indicate more liberal preferences. The coefficient β_3 is predicted to be positive: Democrats are more likely to vote with their party if their constituency becomes more liberal due to redistricting. The sum of β_3 and β_4 is predicted to be negative: Republicans are less likely to vote with their party if their constituency becomes more liberal.

The information-based voting hypothesis predicts less variability in voting records and less party line voting as seniority increases. The Downsian model predicts a change in voting behavior when district boundaries change and when the preferences between the old and new constituencies differ. Furthermore, the direction of the change is predicted: Legislators who run in more liberal districts will cast more liberal votes after redistricting. Equations 3 and 4 test these hypotheses.

ABS(
$$\Delta \text{Vote}_{\pi}$$
) = $\beta_1 \text{Seniority}_{\pi} + \beta_2 \text{Redistricting}_{\pi} + \beta_3 X_n + \mu_7 + \in \pi$. (3)

 $\Delta Vote_n = \beta_1 Seniority_n + \beta_2 (Seniority_n \times Republican)$

+
$$\beta_3(\text{Redist}\Delta\text{MedInc}_{\mu}) + \beta_3 X_{\mu} + \mu_i + \in_n$$
. (4)

The $\Delta Vote_{ii}$ variable measures the change in voting behavior from one election cycle to the next.16 The change is calculated as the difference in a legislator's ADA rating. This was selected because of its frequent use in previous studies, and it is the only rating that adjusts for "inflation" and "stretching" (Groseclose, Levitt, and Snyder 1999). The adjustment corrects for the changing rating mean over time, so no cycle effects are required in the estimation. The rating ranges from 0 to 100, and a lower score indicates a more conservative legislator. The ADA score is measured in two ways. In the first, failure to vote lowers the score. In the second, failure to vote does not lower the score. I will focus in the results section on the second measure, which seems the most pertinent for this analysis.17 Moreover, I will focus on the ADA scores of the first session of each Congress. Legislators become aware of any new boundaries in the second session of Congress preceding the first election with redistricting. This knowledge may give them an incentive to alter their voting behavior in the second session (Rothenberg and Sanders 1999). In this case it is preferable to compare the voting records of 1991 (the first session of the 102d Congress) to those of 1993 (the first session of the 103d Congress), as I do here.

The seniority variable, the redistricting indicator, the X_{ji} vector, and the legislator fixed effects, μ_{ij} , are defined as previously. Covered by this analysis are election cycles from 1983–84 to 1993–94.

In equation 3, the dependent variable ABS(ΔV ote_{μ}) is the absolute value of the change in ADA ratings from election cycle to election cycle, and β_2 is predicted to be positive.¹⁸ In equation 4, larger values of

¹⁵ Percentages are ordered from highest to lowest and divided into three equally large groups. A rank of 1 is assigned to the highest group, a 2 to the middle group, and 3 to the lowest group. By construction, the rank is positively correlated with the percentage; if a change in the percentage does not alter the rank, the rank is independent of the error term in the second stage. A low number of ranks, as used here, reduces the likelihood of such a correlation

¹⁴ Using the congressional district maps of the U.S. Department of Commerce, which show the geographic areas in the 1980s and 1990s, I visually examined whether the boundaries changed by more than 500c.

¹⁵ Redistricting increases uncertainty about new constituency preferences, which gives the legislator an incentive to take cues from the party. Most legislators who experienced redistricting in this sample were more senior, however, so they already had learned how to assess efficiently the opinions of constituents. The mean number of cycles for legislators who were not redistricted is 3.6, compared to 5.9 for redistricted legislators. The difference in means is statistically significant at the 1% level.

¹⁶ Relative to equations 1 and 2, the number of observations is reduced in equations 3 and 4. Since I examine the change in the constituency preferences from one cycle to the next for a given legislator, the legislator has to be in Congress in two adjoining cycles. ¹⁷ I will note in the next section whether the results presented are robust when I use the inflation-unadjusted scores and when I use the inflation-adjusted but not absence-adjusted scores.

¹⁸ One particularly important factor diminishes the magnitude of the redistricting coefficient. If a district is divided into two by redistricting, the incumbent has a choice about the one in which s/he will run. Legislators have an incentive to select the district most likely to elect them. In this case the effect of redistricting on their voting behavior

TABLE 1. Data Description of Variables Used in the Regression Analyses: Means (Standard Deviations)

	Senate	House
Absolute change in ADA rating (adjusted for absences)*	9.915 (9.143)	8.144 (7.556)
Change In ADA rating (adjusted for absences) ^a	0.125 (13.511)	-0.676 (10.335)
Party unity (measured in levels) ^b	77.508 (15.034)	78.364 (14.448)
Party unity (measured in levels and adjusted for absences) ^b	82.381 (14.001)	84.531 (14.113)
Seniority measured in number of two-year cycles ^b	10.456 (7.910)	5.575 (4.027)
Redistricted (1 if redistricted, 0 otherwise)°	_	0.0365 (0.1876)
Percentage of popular vote in general election (measured in levels) ^b	60.817 (10.545)	68.811 (14.337)
Retirement (1 if retirement announced, 0 otherwise) ^b	0.036 (0.186)	0.0957 (0.2943)
Absolute change in median household income in redrawn districts in 1993-94 ^d	_	4,369 (4,084)
Absolute change in median household income in districts not redrawn in 1993–94 ^d	_	2,913 (2,878)
Absolute percentage change in median household income in redrawn districts in 1993-94 ^d	_	17.6 (17.4)
Absolute percentage change in median household income in districts not redrawn in 1993–94 ^d	_	11.3 (10.7)
Overall change in median household income in 1993-94d		2,584 (3,975)

Note. The unit of observation is a legislator in a given election cycle. The data cover stx two-year election cycles ranging from 1983-84 to 1993-94. For the Senate there are 1,168 observations for the unadjusted party unity variable and 778 observations for the adjusted unity variable. Except for the ADA variable, the descriptive statistics are based on 1,168 observations. The number of observations on the change in the Senate ADA rating is 432. For the House, there are 5,148 observations for the unadjusted party unity variable and 3,418 observations for the adjusted unity variable. Except for the ADA variable, the descriptive statistics are based on 5,148 observations. The number of observations on the change in the House ADA rating is 1,819. Income is expressed in real 1985 dollars.

Data were obtained from Americans for Democratic Action.

b Data were obtained from annual neues of Congressional Quarterly Almanac from 1983 to 1994.

The redistricting variable is constructed as described in the text.

the dependent variable, $\Delta Vote_{jl}$, imply a more liberal position. Redist $\Delta MedInc_{jl}$ measures the change in the median household income from cycle to cycle, ¹⁹ and the Republican variable is defined as equaling 1 if the legislator is from the GOP, 0 otherwise. The β_1 coefficient measures the change in voting behavior of Democratic legislators (equation 4), the sum of β_2 and β_1 measures the change in voting behavior of Republicans, and β_3 measures the change in voting behavior due to redistricting. The Downsian model predicts that β_3 will be negative: As median district income increases, legislators who run in districts with altered boundaries will vote less liberally.

Between the 1980s and the 1990s the U.S. economy expanded, and the median income in all districts increased. To account for this trend, the RedistAMedInc variable measures the change in income in reapportioned districts relative to the average change in districts that were not redrawn. This variable is expressed in dollar amounts. An alternative measure for the change in constituency preferences due to redistricting will be the percentage change in median income in redrawn districts, again deflated by the

average percentage change in median income. This variable is expressed in percentage terms.²⁰

The analysis of how redistricting affects voting behavior only applies to the House of Representatives, not the Senate. Senators do not face redistricting and run for reelection less frequently than House members. Consequently, they have fewer reelection constraints and are considered more independent from their party. Although these facts may imply that the predicted career changes in voting pattern are less strong for the Senate than for the House, similar but perhaps weaker effects are predicted for the Senate. Therefore, I will test whether the voting patterns in the Senate are similar to those in the House.

Data for the analysis were obtained from annual issues of the Congressional Quarterly Almanac dating from 1983 to 1994 and from the 1980 and 1990 Census of Population and Housing, published by the Bureau of

Data were obtained from the 1980 and 1990 Census of Population and Housing, Bureau of the Census, U.S. Department of Commerce.

is smaller than when they have no choice about the district in which to run.

¹⁹ It remains zero when there is no redistricting but takes positive or negative values for the 1993–94 election cycle.

²⁰ The income measure is adjusted for income growth; relative income, as opposed to absolute income, is more likely to determine whether a person is liberal or conservative. The change in the presidential vote is not adjusted in this manner because I expect that absolute levels of conservative or liberal district sentiments (measured by the presidential support variable) are likely to influence legislators' voting decisions. I will examine, however, whether the results are robust when I adjust the presidential support variable in a similar way to my adjustment of the income variable.

TABLE 2. Differences in Party Unity Averages by Seniority

	Election Cycles In Congress			t-Statistic for
	1 to 3	4 to 6	More than 6	Equality of Means
House	80.1 (13.4) [2,050]	77.5 (14.8) [1,588]	_	5.4
House	_	77.5 (14.8) [1,588]	75.9 (19.8) [1,530]	2.8
Senate	84.8 (10.0) [122]	79.1 (13 <i>.2</i>) [186]		4.3
Senate	_	79.1 (13 <i>.2</i>) [186]	78.2 (15.6) [868]	2.7

Note Standard deviations are given in parentheses. Number of observations is given in brackets.

the Census, U.S. Department of Commerce. Means and standard deviations of all variables are reported in Table 1 and the Appendix.

RESULTS

As a first step in the party line vote analysis, Table 2 shows various levels of party unity voting for three groups of legislators of roughly equal size. In one group are the most junior legislators, in Congress for one to three election cycles; in the second group are legislators with four to six cycles of seniority; and in the third group are the most senior members of Congress. Comparison of party unity averages reveals that junior members are more likely to vote with the party than are senior members. The fourth column of Table 2 shows that these differences are statistically significant.

At first glance it appears odd that the senior legislators, who seemingly represent what the party stands for, are less likely to vote along party lines. This raises the question of who is "the party." The issue can be resolved by recognizing that the party unity measure is closely correlated with the party leadership measure and essentially is a leadership measure (the percentage of identical legislator/party leadership voting). The correlation coefficient between the two is 0.98 in 1986. Thus, to a large extent the party unity measures indicate the frequency with which legislators vote with their leaders, and unity scores should be higher for legislators who are leaders than for legislators who are not leaders. Table 3 shows a higher unity score for the leadership than for the average legislator. The difference in scores between leaders and nonleaders in the House is statistically significant at the 5% level. The difference in scores between leaders and nonleaders in the Senate is somewhat smaller. This finding is consistent with the hypothesis that leaders are "the party," as opposed to nonleaders or senior members in general.²¹

TABLE 3. Differences in Party Unity Averages between Leaders and Nonleaders

	Leaders	Nonleaders	t-Test for Equality of Means
House	84 1 (7.7) [48]	78.0 (18 <i>-2</i>) [5,120]	5.3
Senate	81.1 (14.9) [48]	77.4 (15.0) [1,140]	1.73

Note: Standard deviations are given in parentheses. Number of observations is given in brackets. For each party, the leader and the whip are defined as leadership. The reported means are the unadjusted party unity scores. Results for the adjusted unity scores are very similar to the reported scores.

The first two columns of Table 4 show the effect of seniority on party line voting for the House of Representatives. The findings support the proposition that representatives are less likely to vote with their party majority as seniority increases. As a robustness check, I replaced log seniority with separate indicator variables for legislators with one, two, three, and four cycles in Congress. The coefficients on the seniority indicators are positive, are statistically significant, and decline monotonically with higher levels of House seniority, which indicates that junior legislators are more likely to vote with the party. For example, the estimated coefficients on freshmen in the adjusted unity regressions imply that their unity scores are approximately 22% higher than the party unity measures for legislators with more than four cycles in Congress. The results in Table 4 are robust, regardless of whether the adjusted or unadjusted party unity scores are examined.22 The results were also similar when I followed one class of legislators over the course of their career. In particular, I examined the voting behavior of the 1983 freshmen, and the findings were very similar to those reported in Table 4.

The negative effect of seniority on party line voting cannot be explained by some omitted variables that are fixed over a career, since all constituency preferences that are constant throughout a legislator's tenure are controlled via legislator fixed effects. Moreover, the results are essentially the same if the regression analysis focuses only on the period before redistricting, which means that some unobserved change due to redistricting cannot account for the findings either. They could be explained, however, through a model that stipulates systematic change in constituency preferences, such that the electorate prefers less party line voting as the tenure of their representative increases. What might cause such a shift in preferences, however, is not obvious.

The model makes no predictions about the influence of redistricting on party loyalty, and the findings show that redistricting does not affect party line voting. Impending retirement leads to less unity voting in the absence-unadjusted regression, which is caused by lower attendance rates of legislators in their last term (Table 4, column 1).²³ Yet, legislators in their last term are more

²¹ That junior members have higher party unity scores than senior members does not mean they are leaders. Leaders are drawn from senior members (in this data set, leaders have average tenure of nine election cycles), but of course they are a very small subset even of senior members (48 of 1,188 senators in this sample and 48 of 5,168 members in the House).

²² An alternative specification of the regressions underlying Table 3 is to calculate the average unity rating per cycle and use this as the unit of observation. The regression results were very similar, qualitatively and quantitatively.

²³ Lott (1990) shows that average attendance rates are lower for legislators in their last term. The unadjusted party unity score counts

TABLE 4. Effects of Seniority and Redistricting	Seniority and Redist	Hoting on Party Unity Voting	/ Voting			,
	House	98		Senate	£	
	Party Unity Voting Score*	Party Unity Voting Score**	Party Unity Voting Score*	Party Unity Voting Score**	Party Unity Voting Score*	Party Unity Voting Score**
Sentority = 1 cycle	, i		1	1	0.3343 (0.0861)	0.4182 (0.1018)
Seniority = 2 cycles	1	I	I	l	0.4575 (0.0807)	0.4075 (0.1004)
Senlority = 3 cycles	1	ĺ	í	ţ	0.0215 (0.0816)	0.1772 (0.0890)
Sentority = 4 cycles	I	l	1	I	0.2497 (0.0722)	0.3626 (0.0886)
Log-senlortty	-0.1214 (0.0327)	-0.1705 (0.0482)	-0.1901 (0.0588)	-0.2267 (0.0619)	I	I
Redistricted	-0.0736 (0.0438)	-0.0872 (0.0407)	1	I	1	I
Retirement	-0.3457 (0.0231)	0.0505 (0.0303)	-0.1971 (0.0772)	-0.0245 (0.0821)	-0.1872 (0.0764)	-0.0371 (0.0804)
Percentage In popular election	0.0008 (0.0008)	0.0020 (0.0009)	-0.0062 (0.0033)	-0.0069 (0.0041)	-0.0055 (0.0033)	-0.0051 (0.0036)
Constituency variables	¥8€	X	X88	× 88	Yes	Yes
Legislator fixed effects	Yes	, Yes	98 ,	X88 X	¥88	Yes
Year effects	Yes	Yes	88 ∕	Yes	Y 88	× × ×
H2	0.81	0.91	0.75	0.86	0.76	0.86
Note: Robust standard errors are given in perentheses. Column 1 has 3,418 observations, 840 legislator effe	e given in perenthesee. ons, 640 legislator effects, and	ober Robust standard errors are given in parentheses. Column 1 has 3,418 observations, 840 legislator effects, and 8 year effects (1987 to 1984). Column 2 has 5,148 observations, 741 legislator effects, and 12 year effects (1983 to 1994).	Column 2 has 5,148 observetton	e, 741 legislator effects, and 1.	2 year effects (1963 to 1994).	

For the Strategy in a popular election in the value predicted from a first-stage equation, as described in the taxt.

**Absence lowers scores, "Adjusted for absence."

TABLE 5. Differential Effects of the Presidential Vote after Redistricting on Party Unity Voting: House of Representatives

	Party Unity	Party Unity
	Score*	Score**
Log-Senlority	-0.2141	-0.1780
,	(0.0465)	(0.0613)
Change In		
presidential vote*	0.0125	0.0134
	(0.0036)	(0.0003)
Change In		
presidential vote*		
Republican ^b	-0.0203	-0.0251
	(0.0053)	(0.0045)
Rettrement	-0.3339	0.0528
	(0.0241)	(0.0252)
Percentage In		
popular election ^o	0.0006	0.0013
	(0.0006)	(0.0006)
Constituency		
varlables	Yes	Yes
Legislator		
fixed effects	Yes	Yes
Year effects	Yes	Yes
R²	0.82	0.91

Note Since I examine the change in the presidential vote from cycle to cycle in a legislator's district, some observations are lost, but 4,407 (3,168) observations remain. The overall mean (standard deviation) for the change in the presidential vote is -.78 (4.07), for the 1993-94 cycle, the corresponding numbers are -4.69 (9.02). Standard errors are given in perenthesis.

* Absence lowers acores, **Adjusted for absence

This variable measures the 1992 percentage of the popular vote in a congressional district received by Clinton minus the 1988 percentage of the popular vote in a congressional district received by Dukakle.

b This variable is defined as the "change in presidential vote"-variable, and is interacted with a indicator which equals one if the legislator is a Republican and zero if the legislator is a Democrat.

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likely to vote along party lines once the dependent variable is corrected for absences, and the corresponding coefficient is statistically significant at the 10% level (Table 4, column 2). In sum, legislators in their last term are absent on a significant number of party line votes, but when they vote, they are more likely than their colleagues to side with the party. Finally, a higher winning percentage in the previous general election is associated with more party line voting, but for the most part these coefficients are not statistically significant. To save space, the coefficients on the eight constituency variables (see Appendix) are not reported, since these are of little interest in this analysis.

The last four columns of Table 4 show the results from the Senate party unity regressions. The results on log-seniority reveal that senators, similarly to House

failure to vote as a vote against the party. Thus, scores for retiring legislators who fail to attend roll calls but who vote with the party on all other occasions are lower than for legislators who vote with the party and attend all votes.

members, are less likely to vote along party lines as they gain seniority. The last two columns have indicator variables for specific amounts of seniority. The omitted category is legislators who have spent more than four two-year House election cycles in the Senate. The indicator variables in the adjusted unity regressions reveal an interesting pattern. Senators in their first two cycles and in their fourth cycle vote more along party lines than legislators with five or more cycles.²⁴ Senators in the third two-year cycle, the one in which they run for reelection, adjust their voting behavior significantly: They vote the same as legislators with more than eight years in the Senate. For the adjusted unity score, a t-test for the equality of coefficients shows that party line voting in the third cycle is significantly lower than in the second and fourth cycle.25 Interestingly, after the election is won, senators are more likely to vote with the party than they were in the previous two years.26 It appears that the upcoming general election gives senators an incentive to adjust their voting behavior in the direction of more independence from party influence.27

Table 5 shows the regression results that correspond to equation 2. They reveal that legislators alter their party line voting in response to changing constituency preferences. When redistricting makes the constituency more liberal, Democrats are more likely to vote with their party, and Republicans are less likely to vote with theirs. The coefficients imply that a 1% increase in the presidential vote for the Democratic candidate makes House Democrats 1.3% more likely to vote with their party, whereas House Republicans are 1.8% less likely to vote with theirs. The findings on retirement, seniority, and the other explanatory variables are consistent with the results presented in Table 4.

Before presenting the ADA regression results that correspond to equations 3 and 4, I will examine how the median income variable in these regressions relates to party affiliation and redistricting. The Downsian model predicts that legislators who represent a more liberal redrawn district will have a more liberal voting record. I use median income as a measure of constituency preferences because that is a good predictor of

²⁴ Freshmen and sophomore senators are also most likely to vote along party lines, as measured by the unadjusted unity score, and there is an increase from the first to the second cycle, although the difference is not statistically significant. This finding may be due to some "measurement error" in the unadjusted score.

²⁵ Previous work on the effect of Senate elections on senators' policy positions includes Elling 1982; Fenno 1982; Hibbing 1984; and Thomas 1985. Wright and Berkman (1986) examine the effect of policy issues on the selection of members of Congress.

policy issues on the selection of members of Congress.

To test for robustness of the results, I ran the regression with data only on the second session of an election cycle. The coefficients on log-seniority were negative and statistically significant at the 1% level, and the indicators for seniority revealed a similar pattern.

²⁷ The finding of increased loyalty after reelection indicates that senators are aware of their constituency preferences, that they trade off party unity against constituency support, and that the trade-off changes as election nears. Thus, in addition to informational constraints, this trade-off is another important determinant of voting behavior.

^{**}The results are robust regardless of the change in the presidential vote for the Democratic candidate or the "deflated" change, that is, when I control for the fact that, on average, the percentage received by the Democratic candidate in 1992 was lower (because of the three-way race among Clinton, Bush, and Perot) than in 1988.

party affiliation, the likelihood of a liberal legislator being elected by the district, and his subsequent voting behavior. In this sample, the median household income in districts represented by Republicans was approximately 12% higher than in districts represented by Democrats (\$24,000 versus \$21,500). A simple t-test for differences of means revealed a statistically significant difference at the 1% level. Republicans also have a significantly lower ADA rating than Democrats. The correlation between the ADA rating and median income was -0.10, which is statistically significant at the 1% level. Thus, it appears reasonable to use median income as a measure of whether an electorate becomes more liberal after redistricting.

As noted previously, I created an indicator for those legislators whose district boundaries changed by more than 50%, and 94 representatives fell in this category. I then compared the change in median income before and after redistricting (in districts which had boundaries altered by more than 50%) to the change in median income in all other districts. A t-test showed that the change in absolute median income for the redrawn districts was significantly larger than for the other group (Table 1).29 This test examines the absolute differences because approximately half the legislators ran in a new district with a lower median income than the one they represented previously, when one controls for the general growth in income over the decade.³⁰ In sum, they represented a significantly different constituency relative to their previous district.

The first four columns of Table 6 look at voting variability. They show the results that correspond to equation 3, which uses the absolute change in the ADA rating as the dependent variable. The first two columns relate to the House, the third and fourth to the Senate. In the House, variability declined as seniority increased, which is consistent with the hypotheses outlined above. In the Senate regressions for vote variability (Table 6, cols. 3 and 4), the coefficients on

²⁹ The median income in nonreapportioned districts grew somewhat between 1983 and 1993. log-seniority are negative, and it is statistically significant in column 3, which implies that senators' vote variability declines as seniority increases.³² No coefficient on redistricting is reported in the Senate equations because state boundaries do not change.

For variability of voting, all the coefficients on the redistricting variable are positive, and they are statistically significant in all specifications. It was expected that inclusion of constituency variables would lower the magnitude of the coefficient on redistricting in column 2, because those variables capture some of the changing composition of the district. The results support the hypothesis that legislators adjust their platform in response to the altered preferences of their constituency.³³ In this case, the preference change is caused by redrawing the district boundaries. The mean absolute ADA rating change from cycle to cycle is 8.2. Thus, the coefficient on redistricting implies a change in legislative voting behavior of about 25% relative to the mean of the dependent variable.

The coefficients on the retirement indicators are not statistically significant. Therefore, the outgoing legislators did not miss a significant number of ADA votes; if they had, their ADA scores would have been lower, and this would have been detected by the regression analysis.³⁴

To save space, the coefficients on the eight constituency variables (see the Appendix) in Table 6, columns 2 and 4, are not reported. None was statistically significant, which is not surprising since the model includes legislator fixed effects, and the only variation left in the constituency variables is due to redistricting.

Columns 5 and 6 of Table 6 answer the question of whether the change in legislative voting behavior is consistent with altered preferences in redrawn districts. The dependent variable in these columns is the net change in the ADA rating, which indicates the direction of change. Legislators whose new district has a higher median income than the old vote more conservatively, which supports the hypothesis that they adapt to the preferences of their constituency.³⁵ The coefficient on median income in Table 6, column 5, implies

²⁰ Even accounting for growth in income, one-fifth of the legislators subject to redistricting represented a new constituency with a median income in 1990 lower than the 1980 median in their old district.

³¹ The qualitative results hold, regardless of whether the standard errors are estimated via least squares or a robust estimator of the variance-covariance matrix. Also, the results are similar when the dependent variable is the inflation-unadjusted ADA measure or the absence-unadjusted ADA measure. As a robustness check, I substituted indicator variables for legislators with one to five years of semority. The omitted category was legislators with more than five years' seniority. The results supported the hypothesis that voting behavior is more variable among junior than senior legislators. Substantial change occurred between the first and second term in Congress, and the change from the second to the third term was greater than in subsequent terms. Testing for equality of the seniority indicator coefficients revealed a significantly greater coefficient on the freshmen indicator than on the two-, three-, and four-cycle seniority indicators. In addition, the coefficient on the sophomore indicator was significantly larger than the coefficient for three-term members of the House.

This result is also consistent with a model that posits the development by legislators of specific positions favoring or opposing particular special interests over time (Kroszner and Stratmann 1998, 1999). Thus, voting behavior is variable early in their career, when they are determining with which interest to ade, and variability declines once this decision is made.

²² The results are less strong in the Senate than in the House when I include individual seniority indicators in the regression equation. ²³ This result is consistent with the finding by Glazer and Robbins (1985b) for the 1970s and 1980s and by Rothenberg and Sanders (1999), who use a different method and do not control for legislator-specific characteristics via fixed effects.

³⁴ If junior members who lose reelection or, alternatively, decide to retire have a more variable voting record, then the results on the seniority variable would be due to a selection bias. Because of this concern I estimated the regressions in Table 6 only with those legislators who ran for reelection and won. The results were quantitatively and qualitatively very similar to the results reported.

 $^{^{25}}$ I estimated the model with two median income variable specifications. In one, the median income variable takes only nonzero values for districts whose boundaries changed by more than 50%. In the others, I examined the change in median income for all districts because almost all had at least some boundaries redrawn. The results were robust in both cases. The *t*-statistic was a bit smaller in the first specification, but the results were statistically significant. The second specification is reported in Table 6.

A similar finding was obtained when the presidential vote was substituted for median income The absolute difference in the presidential vote share (for Clinton in 1992 versus Dukakis in 1988) was significantly larger in districts more than half redrawn than in

		Variability	bility		Direction	tlon
	Absolute Change In ADA Ratings, House	In ADA Ratings, 189	Absolute Change In ADA Ratings, Senate	inge in ADA Ratings, Senate	Change in ADA	Change in ADA Batings, House
Log-senlortty	-2.8072 (0.4784)	-2.8072 (0.4784) -3.0881 (0.5175) -2.4546 (1.3181) -1.7733 (1.6014)	-2.4546 (1.3181)	-1.7733 (1.6014)	4.6555 (1.1835)	4.7118 (1.1821)
Log-senlority* Republicans	I	1	l	[-6.0710 (1.4791) -6.2070 (1.4768)	-6.2070 (1.4768)
Change in district median income	I	1	1	1	-0.0702 (0.0196)	l
Percentage change in district median income	I	1	I	I	I	-0.1789 (0.0485)
Redistricted	2.4616 (0.7542)	1.6045 (0.8299)	l	Į	I	I
Retirement	0.7884 (0.6508)	0.7699 (0.6514)	1.8391 (1.5931)	1.8695 (1.6013)	0.3726 (1.1677)	0.4016 (1.1700)
Percentage In popular election	-0.0193 (0.0173)	-0.0152 (0.0176)	-0.0099 (0.0602)	-0.0317 (0.0626)	-0.0056 (0.3128)	-0.0055 (0.0313)
Constituency variables	N _O	Y988	S O	¥ 98	o N	о Х
Legislator fixed effects	Yes	X-98	Yes	Yes	Y 888	Yes
R ²	0.45	0.46	0.40	0.42	0.17	0.17

Noter Robust standard errors are given in perentheses. At ratings are "inflation" adjusted according to Grosectose, Levitt, and Snyder (1999) and absence adjusted.
In the first four columns, the dependent variable is the cycle-to-cycle absolute change from the first assesson's ADA rating, measured as the absolute of the difference in the ADA accres between two adjoining

election cycles
In the last two columns, the dependent variable is the cycle-to-cycle difference in the ADA acces implies that the legislator has a more liberal voting record. Thus, if the ADA difference is positive, the number indicates that the legislator adopted a more liberal position. If it is negative, the legislator adopted a more conservative position.

Each House regression has 666 legislator fixed effects and 1,819 observations. The mean (attandend deviation) for the Republican variable is 0.3864 (0.4824). The Senate regressions have 441 observations and 126 legislator fixed effects.

that when this increases by \$1,000, the legislator's ADA rating drops by .7 points.³⁶

The results on log-seniority imply that, from their first cycle in the House, Republicans vote more conservatively and Democrats vote more liberally. But the coefficients also suggest that both develop more extreme voting behavior over time (Table 6, cols. 5 and 6). The results are consistent with the view that first-term legislators, lacking sufficient information about how to vote on each issue, take "average" positions. With increasing seniority, they move away from this "average" position.³⁷

CONCLUSION

This, article investigates whether legislators change their voting behavior over the course of their career.

districts with no or little boundary change. Moreover, when the new constituency is more liberal (measured by the presidential vote), legislative voting behavior becomes more liberal, as measured by changes in the ADA ratings.

³⁷ The results are very similar when the inflation-adjusted, but not absence-adjusted, ADA scores are used in the regression analysis. They also are similar when the noninflation-adjusted ADA scores are used, but the coefficients are not quite as precisely estimated.

³⁷ Estimating the models in the last two columns of Table 7 reveals that the variability in senators' voting behavior is not caused by a systematic move to the extremes, as in the House, but by unsystematic changes.

The analysis of party unity votes shows that junior legislators are more likely to vote along party lines than senior legislators. With increasing tenure, senators as well as representatives vote less along party lines. These results are consistent with the hypothesis that junior legislators rely more on their party for voting advice and with the hypothesis that they are more susceptible to party pressure than are senior members.

Redistricting is an event that affects a subgroup of legislators and is one potential cause for changes in voting behavior. It is also a natural experiment that allows us to test the Downsian hypothesis that legislators alter their voting behavior in response to changes in constituency preferences. The results show that this is the case in the House of Representatives. When districts become more conservative, House members tend to become less liberal in their voting.

In summary, legislators change their voting behavior over the course of their career. Their voting decisions are shaped by responsiveness to their constituency and by legislative seniority. This article stresses an informational rationale for changing party line voting and for variability in voting behavior. Future research should explore the relative importance of informational constraints, changing constituency preferences, leadership and party pressure, and institutional arrangements in explaining variable voting behavior throughout legislators' careers.

APPENDIX: Constituency Variables: Means (Standard De-	vlations)	<u>, '</u>
	Senate	House
Percentage of population over 65 years	11.69 (2.12)	11.91 (3.29)
Percentage of population with college education or more	17.87 (3.27)	18.10 (7.46)
Percentage of population employed in manufacturing industry	21.59 (7.77)	[,] 23.40 (8.42)
Percentage of white population	84.70 (11.76)	81.93 (17.89)
Median household Income	\$22,702 (4,291)	\$26,189 (5,520)
Percentage of blue-collar workers	30.84 (4.40)	30.41 (7.70)
Percentage of home ownership	63.59 (5.18)	61.98 (13.06)
Percentage of urban area	67.57 (14.53)	.74.40 (22.25)

Note: The unit of observation is a state for the Senate and a congressional distinct for the House. Means are calculated from levels of constituency characteristics spanning the 1983–84 to 1993–94 election cycles. Data were obtained from the 1980 and 1990 Census of Population and Housing, Bureau of the Census, U.S. Department of Commerce.

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Buying Supermajorities in Finite Legislatures

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analyze the finite-voter version of the Groseclose and Snyder vote-buying model. I identify how the optimal coalition size varies with the underlying preference parameters; derive necessary and sufficient conditions for minimal majority and universal coalitions to form; and show that the necessary condition for minimal majorities found in Groseclose and Snyder is incorrect.

common feature of numerous rational choice theories of politics, such as the size principle of Riker (1962) or the stationary equilibria of Baron and Ferejohn (1989), is that minimal winning coalitions are likely to form. This prediction runs counter to the empirical regularity that such coalitions are rarely if ever observed (Browne 1993). To remedy this situation Groseclose and Snyder (1996) develop a model of competitive vote buying in which the equilibrium path of play, for certain parameter values, has one group bribing a supermajority of voters and the second group bribing no one; the supermajority votes in favor of the policy preferred by the former group, whereas a simple majority would have sufficed. The incentives underlying this apparently excessive vote buying are found in the sequential structure of the moves: One group bribes a sufficiently large number of voters at the first stage so as to prevent a successful bribe attack by its opponent at the second stage. That is, the pressure to build a supermajority coalition is driven by the "unseen" competitive response that would have occurred had the first group attempted to secure only a bare majority.

Most of the analysis in Groseclose and Snyder (1996) assumes a continuum of voters, which makes certain types of results easier to identify but others more difficult. In particular, their characterization of the optimal coalition size requires the stringent assumption that voter preferences are linearly related. This assumption is also present in their one finite-voter result, on the optimality of minimal majority coalitions. I will consider only the finite-voter model and replace the linearity assumption with a bound on voter preferences. Using fairly elementary methods, I will generate a characterization of the optimal coalition size and identify how this size varies as the underlying parameters of the model change. I show that the optimal coalition size is weakly increasing in the value voters place on the winning group's preferred alternative. That is, as voter preferences shift in favor of that alternative, the winning group does not decrease, and may actually increase, the number of voters bribed. I also show that the result of Groseclose and Snyder

(1996) on the optimality of minimal majorities is not correct.

THE MODEL

There are two alternatives (x and y), two interested parties (A and B), and a set $N = \{1, \ldots, n\}$ of voters, with n assumed odd. Party A prefers x to y and is willing to pay up to $W_A > 0$ to see the former prevail; B prefers y to x and is willing to pay $W_B > 0$. For each $i \in N$, let $v_i \in \Re$ denote the intensity of i's preference for voting for x over y, measured in money, and let $\mathbf{v} = (v_1, \ldots, v_n)$ denote a preference profile. Thus, voter preferences are defined by how they vote rather than by the alternative that prevails; $v_i > 0$ means that i prefers x to y, and $v_i < 0$ means that i prefers y to x. Since the voters are indistinguishable to A and B save for these preference intensities, without loss of generality I can restrict attention to preference profiles of the form $v_1 \geq v_2 \geq \cdots \geq v_n$.

The sequence of decisions is as follows. Initially, A offers a bribe schedule, $a=(a_1,\ldots,a_n)\in \Re_+^n$, after which B, with knowledge of a, offers a bribe schedule, $b=(b_1,\ldots,b_n)\in \Re_+^n$. All $i\in N$ vote for either x or y, and majority rule determines the outcome. Solving this game via backward induction, given bribe schedules (a,b), voter i will prefer to vote for x if $a_i+v_i>b_i$ and for y if $a_i+v_i< b_i$; we assume that an indifferent i votes for y. Given bribe schedule a and a constraint W_B , B seeks the least-cost majority to bribe. Since indifferent voters choose y, B need pay no more than a_i+v_i to secure the vote of i, and if this amount is nonpositive, she gets i's vote with no bribe at all. Thus, B solves

$$\min_{C} \left\{ \sum_{i \in C} \max\{0, a_i + v_i\} : \left| C \right| > n/2 \right\}$$

as long as this amount is strictly less than W_B ; otherwise, she chooses $b=(0,\ldots,0)$, that is, B bribes no one. Finally, A sets his bribe schedule so as to have x prevail in the least-cost manner (if affordable), taking into consideration B's predicted reaction.

As do Groseclose and Snyder (1996), I restrict attention to situations in which W_A is large enough relative to W_B and \mathbf{v} so that, in equilibrium, x prevails over y. For the latter to occur, the schedule selected by A must be such that for every majority coalition C, $\sum_{i \in C} \max\{0, a_i + v_i\} \ge W_B$; I refer to bribe schedules satisfying these inequalities as unbeatable. Let $U(\mathbf{v}, W_B) \subseteq \mathfrak{M}_B^+$ denote the set of unbeatable bribe sched-

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ules, and for any schedule a let $S(a) = \sum_{i=1}^{n} a_i$ be the expenditure associated with a. The above assumption on W_A , W_B , and \mathbf{v} is that there exists an unbeatable bribe schedule affordable for A; that is, $\tilde{a} \in U(\mathbf{v}, W_B)$ such that $S(\tilde{a}) \leq W_A$. A then solves

$$\min\{S(a): a \in U(\mathbf{v}, W_B)\}. \tag{1}$$

The set $U(\mathbf{v}, W_B)$ is evidently closec, whereas the set $\{a \in \Re_+^n : S(a) \leq S(\tilde{a})\}$ is compact. Their intersection, within which any solution to expression 1 must reside, is compact (and nonempty by \tilde{a}); and since S is continuous, a solution to expression 1 exists.

Characterizing a solution to expression 1 is made easier by the following observation. For any $a \in \mathfrak{R}_{+}^{n}$, let $C(a) = \{i \in N : a_i > 0\}$ denote the set of individuals who receive a bribe from A. Then one can show that there exists a solution a' to expression 1 in which $a'_i + v_i = a'_j + v_j$ for all $i, j \in C(a')$; that is, under schedule a' all voters bribed by A are equally expensive for B to bribe. The intuition is that A has no incentive to bribe voters, which make them differentially expensive for B to bribe, as B will simply ignore the higher cost voters in constructing a least-cost majority. Groseclose and Snyder (1996) refer to this as a leveling schedule; let $U'(\mathbf{v}, W_B) \subseteq U(\mathbf{v}, W_B)$ denote the set of unbeatable leveling schedules, that is, bribe schedules $a \in U(\mathbf{v}, W_B)$, such that $a_i + v_i = a_j + v_j = t(a)$ for all $i, j \in C(a)$. The bribe $a_i = t(a) - v_i$ made to $i \in C(a)$ can be thought of as the sum of two terms. The first (t(a)) is a positive "transfer" common among all members of C(a), and the second $(-v_i)$ can be positive or negative and is individual-specific. The latter term brings all the members of C(a) to being indifferent between x and y, absent any bribe from B; the former term represents the per-capita amount necessary to make C(a), together with any unbribed voters, unaffordable to B.

To simplify the analysis further, I make the following pair of assumptions:

A1:
$$\nu_{(n+1)/2} < 0$$
;

A2:
$$v_1 < 2W_B/(n+1)$$
.

One implication of A1 is that in the absence of any bribes y will defeat x, so in equilibrium A must bribe at least one voter. In fact, A2 implies that A must bribe at least a majority of voters; otherwise, B will have sufficient resources to bribe a majority of voters, and y will defeat x. A2 also implies that for all $a \in U'(\mathbf{v}, W_B)$ it must be that $t(a) \ge 2W_B/(n+1)$; otherwise, B can bribe a majority from the coalition C(a) itself and have y defeat x. More substantively, A2 says that B cares a great deal more about defeating x than any of the voters care about x prevailing. Propositions 2, 3, and 4 of Groseclose and Snyder (1996) assume A1 holds; A2 is new.

For any $a \in \mathfrak{R}_{+}^{n}$ let k(a) = |C(a)|, and suppose $a \in U^{l}(\mathbf{v}, W_{B})$ is such that $v_{i} \geq v_{j}$ and $j \in C(a)$ but $i \notin C(a)$; that is, i is at least as favorable to x as is j, but j is bribed and i is not. Then, under A2, there exists $a' \in U^{l}(\mathbf{v}, W_{B})$ with $S(a') \leq S(a)$, k(a') = k(a), and $i \in U^{l}(\mathbf{v}, W_{B})$

C(a'), but $j \notin C(a')$ by simply swapping the roles of i and j: $a'_i = t(a) - v_i$, $a'_j = 0$, and for all $m \notin \{i, j\}$, $a'_m = a_m$. Repeating this logic, and recalling that $v_1 \ge \cdots \ge v_n$, we see that for all $a \in U^l(v, W_B)$ there exists $a' \in U^l(v, W_B)$ such that $S(a') \le S(a)$ and $C(a') = \{1, \ldots, k(a)\}$, so we can without loss of generality restrict attention to schedules a by A, which bribe the first k(a) voters. Call these monotonic schedules, and let $U_m^l(v, W_B) \subseteq U(v, W_B)$ denote the set of unbeatable schedules that are both monotonic and leveling.

Therefore, when A2 holds,

$$\min\{S(a): a \in U(\mathbf{v}, W_R)\}\$$

$$= \min\{S(a) : a \in U_m^l(\mathbf{v}, W_B)\}.$$

Because the constraint set in the latter is more manageable than the constraint set in the former, I will focus on solving the latter optimization problem.

For any $a \in U_m^l(\mathbf{v}, W_B)$, the expenditure S(a) can be written

$$S(a) = \sum_{i \in C(a)} a_i = \sum_{i \in C(a)} [t(a) - v_i] = k(a) \cdot t(a)$$
$$- \sum_{i \le k(a)} v_i.$$

Furthermore, the parameters k(a) and t(a) completely characterize any schedule $a \in U_m^l(\mathbf{v}, W_B)$, so A's optimization problem can be reformulated as the choice of parameters k and t. That is, A now solves

$$\min_{k,t} k \cdot t - \sum_{i \leq k} v_i$$

subject to the constraint that the induced schedule, call it $a(k, t, \mathbf{v})$, lies in $U_m^l(\mathbf{v}, W_B)$. This induced schedule is defined as $a_i = t - v_i$ if $i \le k$; $a_i = 0$ otherwise. Using A1 and A2, I can reformulate this as an unconstrained problem involving simply the choice of k, as follows. If $a(k, t, \mathbf{v})$ is unbeatable, then by A2 we know that $k \ge (n+1)/2$, so by A1 it must be that if $a_i(k, t, \mathbf{v}) = 0$, then $v_i < 0$; that is, all nonbribed voters prefer (in the absence of bribes) y to x. Therefore, B receives the votes of all $i \in \{k+1, \ldots, n\}$ for "free." For $a(k, t, \mathbf{v})$ to be unbeatable it must then be that B cannot afford to bribe the additional (n+1)/2 - (n-k) = k - (n-1)/2 voters needed to form a majority, or

$$t \cdot [k - (n-1)/2] \ge W_R.$$

Solving this for equality gives the optimal transfer from A to members of $C(a) = \{1, \ldots, k\}$, conditional on k:

$$t(k, W_B) = \frac{W_B}{k - (n - 1)/2}$$
 (2)

Defining

$$E(k, \mathbf{v}, W_B) = k \cdot t(k, W_B) - \sum_{i \le k} v_i$$
 (3)

¹ A2 guarantees that a' is nonnegative.

as the minimal "winning" expenditure conditional on k, A's problem now is to

$$\min_{k} \{ E(k, \mathbf{v}, W_B) : k \in \{ (n+1)/2, \dots, n \} \}.$$
 (4)

I assume that if there are multiple solutions to this problem, A selects the smallest solution. Modulo this adjustment, let $k^*(\mathbf{v}, W_B)$ denote the solution to expression 4. This solution implicitly generates a solution to expression 1 through expression 2 and the induced bribe schedule described above.

Finally, recall that, by A2, $k^*(\mathbf{v}, W_B)$ must be at least (n+1)/2, so that by A1 the only individuals who vote for x, A's preferred alternative, are those who are bribed by A; Groseclose and Snyder (1996) refer to this as a flooded coalition.² The number of individuals voting for x is equal to $k^*(\mathbf{v}, W_B)$, the number bribed by A, and so results on $k^*(\mathbf{v}, W_B)$ are equivalently results on the size of the coalition voting for A's preferred alternative. In particular, a supermajority votes for A's preferred alternative if and only if a supermajority is bribed by A. Also, note that $k^*(\mathbf{v}, W_B)$ identifies not only the size of A's optimal coalition but also a voter, namely, the voter who receives the largest bribe from A.

RESULTS

I begin with a characterization of $k^*(\mathbf{v}, W_B)$; for notational ease, in some of what follows I will suppress the dependence of E and k^* on \mathbf{v} and W_B . Because the number of possible values for k is finite, I cannot employ calculus techniques to identify k^* , but a discrete version of these techniques can be used. For all $k \in \{(n+1)/2, \ldots, n-1\}$, define $\Delta(k) = E(k+1) - E(k)$; that is, $\Delta(k)$ is the difference in expenditures from adding the $(k+1)^{\text{st}}$ voter to the coalition $\{1, \ldots, k\}$. If $\Delta(k) < 0$, then (since A is attempting to minimize expenditures) A has an incentive to add the $(k+1)^{\text{st}}$ voter to the coalition. Conversely, if $\Delta(k) \geq 0$, then A does not want to add the $(k+1)^{\text{st}}$ voter (recall our tie-breaking rule in favor of smaller coalitions). This gives a sense of the local or "first-order" effects of changing the coalition size.

Next, suppose $\Delta(k)$ is increasing in k, which is simply the discrete version of the second-order condition that E(k) be convex in k. The following algorithm then can be used to identify k^* : If $\Delta((n+1)/2) \geq 0$, then we know from $\Delta(k)$ increasing that $\Delta(k) > 0$ for all larger values of k, and hence the optimal value of k is $k^* = (n+1)/2$. If $\Delta((n+1)/2) < 0$, then we know that k^* must be greater than (n+1)/2, so we next solve for $\Delta((n+3)/2)$. If this term is nonnegative, then, again by $\Delta(k)$ increasing, we have that $k^* = (n+3)/2$; if the term is negative, then we next check (n+5)/2; and so on. When $\Delta(k)$ is increasing, we have the following implicit characterization of the optimal coalition size:

$$k^* = \begin{cases} (n+1)/2 & \text{if } \Delta((n+1)/2) \ge 0\\ \max\{k : \Delta(k-1) < 0\} & \text{otherwise} \end{cases}$$
 (5)

Finally, I show that $\Delta(k)$ is indeed increasing in k. From equations 2 and 3,

$$\Delta(k) = \left[\frac{(k+1)W_B}{k+1 - (n-1)/2} - \sum_{t \le k+1} v_t \right]$$

$$- \left[\frac{kW_B}{k - (n-1)/2} - \sum_{t \le k} v_t \right]$$

$$= W_B \left[\frac{(k+1)}{k+1 - (n-1)/2} - \frac{k}{k - (n-1)/2} \right] - v_{k+1}$$

$$= \frac{-W_B(n-1)}{2(k+1 - (n-1)/2)(k - (n-1)/2)} - v_{k+1}$$

$$= T(k, W_B) - v_{k+1}.$$
 (6)

Treating k for the moment as a continuous variable, it is easily seen by differentiation that $T(k, W_B)$ is increasing in k. Furthermore, since $v_1 \ge v_2 \ge \cdots \ge v_n$, the second term, $-v_{k+1}$, is nondecreasing in k. Hence, the discrete second-order condition holds, which implies the above local analysis is also global: Equation 5 defines the optimal coalition size.

Although generating an explicit characterization of k^* via equations 5 and 6 admittedly would be somewhat messy, the parameter values that give rise to the "corner" solutions, that is, k^* equal to either (n+1)/2 or n, are straightforward to identify. We have that $k^* = (n+1)/2$ if and only if $\Delta((n+1)/2) \geq 0$, and $k^* = n$ if and only if $\Delta(n-1) < 0$; inserting the relevant values into $T(k, W_B)$, we obtain the following.

Proposition 1. (a) $k^*(v, W_B) = (n + 1)/2$ if and only if $v_{(n+3)/2} \le -W_B(n - 1)/4$; (b) $k^*(v, W_B) = n$ if and only if $v_n > -2W_B/(n + 1)$.

Therefore, to determine whether a minimal majority coalition is optimal, the only relevant part of the preference profile \mathbf{v} is the $((n+3)/2)^{st}$ term, and the only relevant part for a universalistic coalition is the last term. Of course, if neither inequality in proposition 1 holds, $k^*(\mathbf{v}, W_B)$ lies strictly between (n+1)/2 and n; that is, the optimal coalition is a less-than-universalistic supermajority. Note also that proposition 1(b) identifies a lower-bound constraint on \mathbf{v} symmetric to the upper-bound constraint imposed above. Whereas A2 requires no voter to prefer x over y by more than $2W_B/(n+1)$, proposition 1(b) says that if, in addition, no voter prefers y over x by more than $2W_B/(n+1)$, then the optimal choice by A is to bribe all the voters.

Proposition 1(a) gives as an immediate consequence separate necessary and sufficient conditions for a min-

² The implication of adding A2 to what Groseclose and Snyder (1996) already assume is that nonflooded coalitions, which are at times optimal in their environment, are never optimal here.

imal majority to be optimal, based only on the preference intensities of the "extreme" voters:

COROLLARY 1.
$$k^*(v, W_B) = (n + 1)/2$$
 if $v_1 \le -W_B(n - 1)/4$, and only if $v_n \le -W_B(n - 1)/4$.

In words, a sufficient condition for A to find it optimal to bribe a minimal majority of voters is that all voters find y significantly more attractive than x, whereas a necessary condition is that at least one voter finds this to be so.³

Like proposition 1(b), proposition 3.3 in Groseclose and Snyder (1996) provides a necessary and sufficient condition for a universalistic coalition to be optimal in the continuum model, but only under a linear restriction on preference intensities (see below). Such an assumption is also made for their proposition 4 as to when minimal majorities are optimal; yet, since the concept of a minimal majority is not well defined with a continuum of voters, their result assumes a finite legislature and so is directly comparable to results here. Suppose voter preference intensities can be written

$$v_i = \alpha - \beta [i - (n+1)/2],$$

with $\alpha \le 0$ and $\beta \ge 0$. Proposition 4 in Groseclose and Snyder (1996) asserts that if $k^*(\mathbf{v}, W_B) = (n + 1)/2$, then it must be that $W_B < (2.1)\beta$. That is, as long as B is willing to spend more than twice the difference in preference intensity between "adjacent" voters, A must bribe a supermajority. But consider this example: n =7, $W_B = 3$, and $v_i = -5$ for all $i \in N$, which implies $\alpha = -5$ and $\beta = 0$ in the above linear format. According to Groseclose and Snyder, $k^*(v, W_B)$ should be strictly greater than four, but this is not true. Because $v_i < 0$ for all $i \in N$, B gets all voters not bribed by A for free. If A bribes four voters, she must pay each 3 + 5 = 8 (so that B cannot afford to attract any one voter), giving a total payment of 32. If A bribes five voters, the required bribe is 1.5 + 5 = 6.5 (so that B cannot attract any two), for a total payment of 32.5; similarly, the bribes to six and seven voters total 36 and 40.25, respectively. Therefore, A's optimal strategy is to bribe precisely a minimal majority of four voters, which contradicts proposition 4 of Groseclose and Snyder. Furthermore, we know from proposition 1(a) that this example is robust to (small) changes in the values of α and β ; all that is needed is v_1 less than .75 (so A2 holds) and $v_{(n+3)/2}$ less than -4.5 for the optimal coalition size to be a minimal majority.

I next identify how the optimal coalition size varies with voter preference intensity. Given an arbitrary amount W_B and preference profile \mathbf{v}' , let $k' = k^*(\mathbf{v}', W_B)$. If k' = (n+1)/2, then we know that $k^*(\mathbf{v}, W_B) \geq k'$ for all \mathbf{v} , so suppose k' > (n+1)/2. From equation 5 we infer $\Delta(k'-1, \mathbf{v}', W_B) < 0$, which from equations 6 and 7 is equivalent to $v'_{k'} > T(k'-1, W_B)$. Now suppose the preference intensities change from \mathbf{v}' to \mathbf{v} , and $v_{k'}$ is such that $v_{k'} \geq v'_{k'}$. Then, $v_{k'} > T(k'-1, W_B)$, and hence $\Delta(k'-1, \mathbf{v}, W_B) < 0$. But

from equation 5 it must be that $k^*(\mathbf{v}, W_B) \ge k'$. Therefore, I have proven the following.

PROPOSITION 2. For all W_B , if v and v' are such that $v_{k'} \ge v'_{k'}$, where $k' = k^*(v', W_B)$, then $k^*(v, W_B) \ge k^*(v', W_B)$.

In words, if the preference intensity of the "marginal" bribed voter weakly increases, then the optimal coalition size will weakly increase as well, regardless of any changes in the other voters' intensities. An equivalent statement in terms of bribes is this: The number of voters bribed by A weakly increases as the voter who receives the largest bribe finds A's preferred alternative, x, more attractive.

The above argument can be turned around to generate a sufficient condition for the optimal coalition size to decrease weakly. As before, let $k' = k^*(\mathbf{v}', W_B)$; if k' = n, then clearly $k^*(\mathbf{v}, W_B) \leq k'$ for all \mathbf{v} , so let k' < n. By equation 5 I induction $(k', \mathbf{v}', W_B) \geq 0$, or from equations 6 and $(k', \mathbf{v}', W_B) \geq 0$. Suppose $(k', \mathbf{v}', W_B) \leq 0$, which implies by equation 6 that $(k', \mathbf{v}, W_B) \geq 0$, and hence by equation 5 that $(k', \mathbf{v}, W_B) \leq 0$, and hence by equation 5 that $(k', \mathbf{v}, W_B) \leq 0$, and hence by equation 5 that $(k', \mathbf{v}, W_B) \leq 0$. Therefore, we have the following.

Proposition 3. For all W_B , if \mathbf{v} and \mathbf{v}' are such that $\mathbf{v}_{\mathbf{k}'+1} \leq \mathbf{v}'_{\mathbf{k}'+1}$, where $\mathbf{k}' = \mathbf{k}^*(\mathbf{v}', W_B)$, then $\mathbf{k}^*(\mathbf{v}, W_B) \leq \mathbf{k}^*(\mathbf{v}', W_B)$.

In words, if the preference intensity of the marginal nonbribed voter weakly decreases, then the optimal coalition size will weakly decrease as well, regardless of any changes in other voters' intensities. (Unlike proposition 2, however, this voter cannot be identified from the bribes offered by A.) Combining propositions 2 and 3, we see that if the preference intensities of the marginal bribed and nonbribed voters do not change, then the optimal coalition size also does not change.

The logic of propositions 2 and 3 stems from the "convexity" of E and the subsequent ability to adopt a first-order approach in characterizing the optimal coalition size. As with the traditional calculus technique, in the presence of such convexity, only "local" information is relevant for generating comparative statics about how changes in \mathbf{v} affect changes in E. Here, this local information is summarized by the preference intensities of the marginal bribed and nonbribed voters.

Of course, in order to identify these voters (and so verify the conditions in either proposition) one needs to solve for the optimal coalition size, which as mentioned above might prove somewhat messy. Yet, both propositions give rise to a weaker, more global comparative statics result that does not require such a computation. Given two preference profiles \mathbf{v} and \mathbf{v}' , write $\mathbf{v} \geq \mathbf{v}'$ if $\mathbf{v}_i \geq \mathbf{v}_i'$ for all $i \in N$.

COROLLARY 2. For all W_B , if $v \ge v'$ then $k^*(v, W_B) \ge k^*(v', W_B)$.

Thus, the number of voters bribed by A, and hence the size of A's optimal coalition, weakly increases as voters

³ I thank an anonymous referee for suggesting that corollary 1 (which was in a previous draft) could be generalized to proposition 1(a) (which was not).

find A's preferred alternative, x, more attractive. This result certainly has a counterintuitive feel, as one might expect just the opposite, namely, that as x becomes more attractive relative to y, fewer voters will need to be bribed. The latter may well be true in a one-party model, but in this case A's optimal behavior is driven by the predicted competitive response of B. Thus, although A's total expenditure will surely decrease as x becomes more attractive, the optimal way to allocate this lower amount is to spread it more widely among the voters.

The logic of corollary 2, independent of its status as an implication of propositions 2 and 3, comes directly from the ability to restrict attention to leveling, monotonic bribe schedules. From the former (leveling) we can write A's expenditure, conditional on bribing k voters, as an additively separable function of the transfer necessary to fight off B and of voter preference intensities. Although this expenditure obviously depends on the transfer, the *change* in the expenditure due to a change in voter preferences does not. And from the latter (monotonic) we know that as x becomes more attractive to the voters, the change in expenditure will be greater, the larger is k. That is, from equation 3,

$$E(k, \mathbf{v}', W_B) - E(k, \mathbf{v}, W_B) = k \cdot t(k, W_B) - \sum_{i \leq k} v_i'$$

$$-\left[k\cdot t(k, W_B) - \sum_{i \leq k} v_i\right] = \sum_{i \leq k} (v_i - v_i).$$

This sum weakly increases in k when $\mathbf{v} \geq \mathbf{v}'$, since each of the terms in the sum is nonnegative. Let k' be optimal at \mathbf{v}' and k < k', which implies that k is necessarily suboptimal. Then, in moving from \mathbf{v}' to \mathbf{v} , the expenditure on k' decreases by a greater amount than does the expenditure on k, so k remains suboptimal. This does not imply that k' is optimal at \mathbf{v} , merely that if it is not optimal, then the new optimal size must be greater than k'.

Two additional features of corollary 2 are worthy of comment. First, Groseclose and Snyder (1996) identify a similar comparative statics result in their continuum model; voters are indexed by a uniform distribution on [-1/2, 1/2], and preference intensities are described by a nonincreasing and differentiable function $v : [-1/2, 1/2] \rightarrow \Re$. As in their proposition 4, however, they require v to be linear: $v(z) = \alpha - \beta z$, with $\beta \ge 0$ and $\alpha \le 0$. They then show that k^* is nondecreasing in α , which is analogous to my corollary 2.5 Second, the result as stated requires each voter's preference for x over y, as measured by v_i , to increase weakly. Yet, as mentioned above, voters are indistinguishable to A and B beyond these v_i s. Even if some voters have v_i

decrease, as long as the new distribution of preference intensities is everywhere above that of the old, corollary 2 will remain true. That is, as long as the highest value in \mathbf{v} is greater than or equal to the highest value in \mathbf{v}' , the second-highest value in \mathbf{v} is greater than or equal to the second-highest value in \mathbf{v}' , and so on, it will be the case that $k^*(\mathbf{v}, W_B)$ will be greater than or equal to $k^*(\mathbf{v}', W_B)$.

Finally, I show that A's optimal coalition size k^* is monotonic in W_B as well. Because A2 depends on W_B , I need to add the assumption that $v_i \leq 0$ for all i.

Proposition 4. For all \mathbf{v} , if $\mathbf{W}_{\mathrm{B}} > \mathbf{W}_{\mathrm{B}}'$, then $\mathbf{k}^*(\mathbf{v}, \mathbf{W}_{\mathrm{B}}) \geq \mathbf{k}^*(\mathbf{v}, \mathbf{W}_{\mathrm{B}}')$.

Therefore, as B's willingness to pay increases, A tends to bribe a greater number of voters. The proof is similar to that for proposition 2. Let $k' = k^*(\mathbf{v}, W_B')$. From equation 5 we know that $\Delta(k'-1, \mathbf{v}, W_B') < 0$, so from equations 6 and 7, $v_{k'} > T(k'-1, W_B')$. Since T is clearly decreasing in its second argument, $W_B > W_B'$ implies $v_{k'} > T(k'-1, W_B)$, and $\Delta(k'-1, \mathbf{v}, W_B') < 0$, which by equation 5 implies $k^*(\mathbf{v}, W_B) \ge k'$.

DISCUSSION

The propositions and corollaries presented here give a fairly complete theoretical picture of supermajority bribery under certain assumptions, in particular, the preference restrictions embodied in A1 and A2. These assumptions allow us to focus, without loss of generality, on a relatively simple class of bribe schedules (monotonic and leveling) and translate A's optimization problem into one with an attractive mathematical property (convexity). An open question is the extent to which my results survive the weakening of these assumptions. For instance, without A2 there can exist voters who prefer x to y by such a great amount that A finds it optimal not to bribe them at all (see Figure 4 in Groseclose and Snyder 1996), thereby adopting a nonmonotonic, although still leveling, schedule. A would essentially be working on two margins in moving through $\{1, \ldots, n\}$, namely, when to start bribing and when to stop, in contrast to the one-margin analysis (when to stop) associated with monotonic schedules. This suggests that the corresponding analysis of A's optimal behavior will be considerably more intricate than that found here.

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⁴ The only requirement is that $\nu_i \ge \nu_i'$ for all $i \in \{(n+3)/2, \ldots, n\}$, as the first (n+1)/2 voters receive bribes regardless of preference intensities.

³ Groseclose and Snyder (1996) also show that k^* is decreasing in β . Yet, although they state that "as β ruses, the initial level of support in the legislature for x declines" (p. 310), which suggests a similar effect to that from α , this statement only holds for $x \in (0, 1/2]$; for $x \in [-1/2, 0)$, a rise in β leads to an increase in support for x.

⁶ Groseclose and Snyder (1996) obtain a similar result in their continuum model, without the linear restriction on preferences.

Vote Buying, Supermajorities, and Flooded Coalitions

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In a recent paper, Banks (2000), adopting the framework of our model (Groseclose and Snyder 1996), derives several new and noteworthy results. In addition, he provides a counterexample to our proposition 4. Here we explain the error in our proposition but note that we can correct it easily if we invoke an additional assumption: In equilibrium the winning vote buyer constructs a nonflooded coalition, that is, she does not bribe every member of her coalition. We conclude with a brief discussion of the substantive implications of Banks's proposition 1; we note that it provides additional support for our general claim that minimal winning coalitions should be rare in a vote-buying game.

HOW PROPOSITION 4 CAN BE RESTATED CORRECTLY

ur mistake in proposition 4 was to assume that the solution to a step in the proof occurred at an interior point when, in fact, the solution might occur at a boundary point.

In our proof we define m as the surplus of A's coalition. That is, the size of A's coalition is m + (n + 1)/2, although only A needs (n + 1)/2 legislators (a bare majority) to pass the bill. Vote buyer B is willing to pay W_B to defeat A's bill. Since he must buy m + 1 legislators to invade A's coalition successfully, A can prevent B from invading if each of her legislators costs at least $W_B/(m + 1)$ for B to buy. We show that A will adopt a leveling strategy. That is, each member of her paid coalition will cost exactly $W_B/(m + 1)$ for B to buy.

Legislators are labeled $i=1, 2, \ldots, n$, and we assume that the legislators have linear preferences. That is, the utility that i receives for voting for A's bill is $v(i) = \alpha - \beta[i - (n+1)/2]$. Define i_0 as the left-most legislator whose initial preferences are less than $W_B/(m+1)$. This legislator will be the left-most member of A's bribed coalition. Let s be the (hypothetical, possibly noninteger-valued) legislator whose preferences for A's bill equals $W_B/(m+1)$. That is, $W_B/(m+1) = v(s) = \alpha - \beta[s - (n+1)/2]$, or $s = [\alpha - W_B/(m+1)]/\beta + (n+1)/2$. Define $\delta(m)$ as the distance between i_0 and s.

If A has not bribed all the members of her coalition, we call this a nonflooded coalition. In this case there exists a legislator to the left of i_0 , labeled $i_0 - 1$. Furthermore, s must lie between these two legislators. Therefore, $\delta(m) \in [0, 1]$.

If A has constructed a flooded coalition, however, that is, she has bribed all members of her coalition, then there is no legislator to the left of i_0 , and it is possible for $\delta(m)$ to be greater than one.

Our mistake is on line 20 of the proof: "Since $\delta(m) \in [0, 1], \ldots$ " Of course, this is not necessarily true when A has constructed a flooded coalition. If we

assume, however, that A constructs a nonflooded coalition, then the proof follows. Thus, the following is a corrected restatement of proposition 4: Suppose $v(i) = \alpha - \beta[i - (n+1)/2]$, with $\beta \ge 0$ and $\alpha \le 0$. If $a^*()$ and $b^*()$ constitute an equilibrium in which A constructs a nonflooded coalition and x wins, then $m^* = 0$ only if $W_B \le [1/3 + (28/9)^{1/2}]\beta < (2.1)\beta$. (The original version does not contain the phrase in italics, but it is otherwise identical.)

While the nonflooded coalition assumption is crucial for our result, the opposite assumption is crucial for Banks's results. As he shows, his A2 assumption implies that the winning vote buyer bribes each member of her coalition in equilibrium, that is, she constructs a flooded coalition.

Substantively, this condition is fairly restrictive. It requires that all members of a coalition, even the most ardent supporters, receive bribes in equilibrium. In contexts in which legislators themselves are the vote buyers, this is impossible. For a genuine flooded coalition to occur, the legislators must receive side payments, not pay them. An example of legislators who are vote buyers is Groseclose's (1996) case study of the Byrd Amendment to the Clean Air Act. Byrd was one vote buyer, and the opposing vote buyer was the team of President George Bush and Majority Leader George Mitchell.²

¹ We should also note that line two of the proof contains a typo. The minus sign in "b(i) > a(i) - v(i)" should be a plus sign.

² We should note, however, that a minor change in A2 does allow Banks's results to apply to contexts where legislators are vote buyers. For instance, suppose legislator 1 is the vote buyer, and she has such intense preferences for x that A2 is violated. As long as the other legislators' preferences satisfy A2, Banks's results do not change qualitatively. Nevertheless, if one restricts the size of the legislature so that it is not too small, and if one restricts preferences of the vote-buying legislators so that they do not differ too drastically from the preferences of the other legislators, then the results do change qualitatively. For instance, suppose (i) preferences take a linear form (as in our proposition 4); (ii) $n \ge 5$; (iii) legislator 1 is vote buyer A (thus, $v_1 = W_A$); and (iv) the preferences of the other legislators satisfy A2. Then there does not exist an equilibrium in which legislator 1 buys votes (Basically, A2 places a minimum bound on W_B , which makes it too expensive for 1 to buy votes successfully. A proof is available from the authors.) That is, if conditions i-iv are satisfied, then Banks's proposition 1 does not hold in the context where legislators are vote buyers, even with the change in A2.

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SUBSTANTIVE IMPLICATIONS OF BANKS'S PROPOSITION 1

We conclude with two brief thoughts on Banks's proposition 1. First, it is a very interesting and impressive result. It provides a necessary and sufficient condition for a minimal winning coalition to occur (unlike our proposition 4, which only provides a necessary condition). Furthermore, unlike our proposition 4, the result does not need to assume that preferences are linear. It does require, however, that preferences satisfy assumption A2,

Second, the proposition shows that our original conclusion—that minimal winning coalitions should be rare—is even more general than our original statement.³ The condition in the proposition requires that $\nu_{(n+3)/2} \leq -W_B(n-1)/4$. That is, for a minimal winning coalition to occur, the legislator just to the

right of the median voter must strongly favor the right-wing vote buyer's alternative, relative to the size of the legislature (n) and the right-wing vote buyer's intensity of preference (W_B) . When the size of the legislature is at least five members, the condition requires that the legislator favor the alternative more strongly than the right-wing vote buyer (even though the legislator is fairly "centrist"). In our view, this should occur very rarely in practice; thus it supports our claim that minimal winning coalitions should be rare.

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³ Many of the ideas in this paragraph developed from conversations with Ken Shotts, to whom we are very grateful.

Analytic Narratives by Bates, Greif, Levi, Rosenthal, and Weingast: A Review and Response

Analytic Narratives. By Robert H. Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry Weingast. Princeton, NJ: Princeton University Press, 1998. 296p. \$65.00 cloth, \$22.95 paper.

he rational choice revolution in political science began in American politics in the 1970s, first influenced international relations in the 1980s, and made its way to comparative politics during the 1990s. As it moved beyond its base in American politics, rational choice theory confronted comparative and historical questions of regime transition, social conflict, democratic stability, economic development, and international governance. The application of rational choice theory to these macro concerns creates analytic problems that were less relevant when the

approach was applied primarily to microquestions. Robert Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry Weingast have written a much discussed book, Analytic Narratives, that develops these novel applications. Jon Elster, a seminal rational choice theorist who also has written extensively on comparative and historical problems, believes that Analytical Narratives expresses hopes for rational choice theories of comparative and historical processes that exceed what the approach is capable of producing. Bates and his colleagues reject Elster's critique and defend their goal of a genuine social science capable of addressing large-scale phenomena. Is rational choice history a case of excessive ambition, or is it the logical next step in the rational choice revolution in political science?

Rational Choice History: A Case of Excessive Ambition

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n Pathologies of Rational Choice, Donald Green and Ian Shapiro (1994) claim that rational choice theory has poor empirical support. Analytic Narratives (AN) is among other things an attempt to rebut this claim (p. 231).1 In my opinion, Bates et al. do not succeed in their attempts to explain complex historical phenomena in terms of rational choice. Although my comments may seem harsh, certain statements in the Introduction and the Conclusion invite the application of strict criteria. The authors claim that "the chapters in this volume ... engage the concerns of many disciplines and should therefore command a broad audience" (p. 3). Similarly, "because the chapters engage such fundamental issues, they are of general significance" (p. 230); moreover, "they furnish deep insights in particular cases" (p. 232).

I proceed in three steps. First, I discuss each chapter to argue that the authors make serious errors of commission that undermine the credibility of the analyses. Second, I raise some general questions concerning the application of rational choice models to large-scale historical phenomena. I argue that the

contributors to AN make important errors of omission, such as failure to provide microfoundations or to offer evidence about the beliefs and intentions of the actors. Third, I try to draw some conclusions about the feasibility of the AN project.

INDIVIDUAL CHAPTERS

Greif's Chapter

The study by Avner Greif of the political economy of late-medieval Genoa offers a periodization of Genoese politics between 1099 and (roughly) 1350. From 1099 to 1154, an economically suboptimal but peaceful equilibrium obtained. Each clan invested in military strength to deter the other from becoming a controlling clan, with the result that fewer resources were available for economic cooperation abroad. (Following Greif, I assume throughout that there were two clans only.) They might still cooperate in economic ventures, but at a less than optimal level. Between 1154 and 1164, an external threat did away with the need for investing in military strength, which made more resources available for economic cooperation. From 1164 to 1194, there was semipermanent interclan warfare. After 1194, the presence of an external administrator of the city, the podestà, brought about peace and "spectacular economic performance" (p. 58). I focus on the second period (1154-64) and the final period (after 1194). As the chapter is opaquely written and marred by severe misprints (e.g., in the statement of Condition III on p. 51), I had to work hard to figure out what is being said. If I got it wrong, Greif must share the blame.

Greif's argument about the optimal nature of the

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¹ Unless otherwise stated, all page references are to this volume. In assessing the chapters by Bates and Levi, I also draw on some of their other writings. Although Greif refers to a forthcoming book-length version of his chapter in AN and Weingast to an unpublished book-length manuscript, I did not have access to these. Some of the issues I raise in the text may be addressed by these more extensive discussions.

second period is based on the premise that the external power will attack if and only if there is interclan warfare. Consider first the "if" part of the premise. Because the clans know that if they fight each other the future will look bleak, they need not invest in military strength to deter each other. Without an external threat, they may have to invest to raise the cost to the other of attacking. With an external threat, the benefits of attack are so small that no deterrence is needed. Consider next the "only if" part of the premise. If there is no need to fear an external attack as long as they abstain from fighting each other, the clans can devote all their resources to economic cooperation. This seems implausible. A strong external power might attack, with some positive probability, even if the clans manage to keep peace. Greif acknowledges as much on page 32. To deter attack and/or to increase their chances of winning a war against the external enemy, the clans would presumably invest some resources in military strength. Greif acknowledges as much: "Rival clans within various Italian city-states often cooperated in confronting external threats" (p. 42). This military cooperation must have come at the expense of economic cooperation. Greif seems to ignore, as far as I can see, that both investment to deter or fight an external enemy and investment to deter a rival clan must come at the expenses of resources available for economic cooperation. Although the presence of the external enemy does away with the need to invest for the latter purpose, it creates a need to invest for the former. In the absence of more information, the net effect of an external enemy on military investment is indeterminate.

I also was not persuaded by Greif's claim that the podestà restored peace and efficiency in the final period. Consider first the argument that the presence of a third party—the external podestà—prevented the endemic interclan warfare that characterized the third period. Greif captures the strategic interaction between the two clans and the podestà by means of two games. In the analysis of the "collusion" game between one clan and the podestà against the other clan, he asserts that "after collusion has occurred, the amount by which the clan would reward a podestà would depend on the podestà's ability to confront that clan militarily" (p. 49), because if he could not gain by confronting it, then the clan could not credibly promise to reward him.

The collusion game is embedded in a larger "podesteria game," in which the first clan has the choice between challenging and not challenging the second; the second, if challenged, has a choice between fighting and not fighting; and the podestà, if the second clan is challenged, has three choices—collude with the first clan, prevent the first clan from taking control (this means the podestà tries to take control for himself), and do nothing. For specific parameter values this game has a subgame-perfect equilibrium in which clan 1 does not challenge; clan 2 fights if challenged; and the podestà prevents if clan 1 challenges and clan 2 fights, and, if clan 2 does not fight, either colludes or does nothing. The podestà has to be strong enough to be

pivotal with a sufficiently high probability but not so strong that the first clan could credibly promise to reward him in a collusion against the second. (I simplify.)

One empirical question is whether the strength of the podestà was within the relevant interval. Greif tells us he had twenty soldiers (p. 53). This is a small number, but if the clans were of nearly equal strength, he might still be pivotal. Greif notes this fact, but fails to provide evidence about the relative strength of the clans. A more serious problem is that Greif does not explain how the podesta generated not only peace but also prosperity. In the second period, prosperity supposedly occurred because the clans could afford to disarm—they had nothing to gain by fighting each other. No mechanism is provided, however, for disarmament in the fourth period. The military strength of the clans is taken as given throughout the analysis, and Greif offers no argument that would exclude higher levels of military investment—and fewer resources available for cooperation—than in the first period. (We might also ask whether the high wage paid to the podestà cut heavily into these resources, but this is probably a minor factor.)

Rosenthal's Chapter

The second chapter is a study by Jean-Laurent Rosenthal of the political economy of European absolutism. His explanandum is nothing less than the divergent courses of French and English absolutism in the seventeenth and eighteenth centuries. To focus my comments, I limit myself to one part of that explanandum: "why per capita taxation was significantly lower in France or Spain than in England or the Low Countries" (p. 73). (Actually, I follow Rosenthal in limiting myself to the comparison between France and England.) The explanation relies on a model that links rates of taxation with gains or losses from wars financed by taxation. In brief, Rosenthal argues that tax rates were higher in countries with a unified tax system—in which either "the elite" or the king had full control over taxation—than in countries where fiscal authority was divided between them. The reason is that taxes are raised mainly for the purpose of conducting wars, the spoils from which are shared between the elite and the crown. In countries where fiscal authority is divided, neither actor can fully internalize the benefits from the taxes it raises; hence, we would expect tax rates to be lower than in a unified country.

Rosenthal assumes that "fiscal resources were used primarily to prepare for and prosecute war" (p. 69). Although he does not spell out what "primarily" means, an estimate is that "war expenditures often represented 80% of public expenditures" in early modern Europe (Bérenger 1996, 623). Whether one is justified in using 100% as a proxy for 80%, as Rosenthal does (p. 102), is not obvious to me. Rosenthal then proceeds to specify the objectives and constraints of the two actors. Both want to maximize the return from war. Both are constrained by the cost of raising resources for warfare. In a numerical exam-

ple, the cost of raising taxes is equivalent to 5% of the national product and equals the amount of taxes raised. The outcome of war affects the two actors differently, for two reasons. First, they "share the returns from winning and losing according to the extent of their fiscal control of the domestic economy" (p. 103). Second, if the war ends in a draw, which in one specification of the model happens in almost half the cases (p. 72), the elite bears all the costs of conducting war. The first mechanism is responsible for the free-rider problem mentioned above, which induces both parties to suboptimal funding of wars. The second implies that the elite might have reason to be even more reluctant than the king.

The idea of spoils divided in proportion to fiscal control is unsubstantiated, empirically and theoretically. Rosenthal does not cite any evidence that spoils were in fact divided in this way. He also does not consider alternative hypotheses, for example, that the spoils were divided in proportion to taxes raised for warfare. In one specification of the model (p. 72) the elite raises no taxes although it controls 10% of taxation and hence receives 10% of the spoils. Why would the king give away 10% of the fruits of victory to an elite that in no way contributed to it? In fact, why would the king ever give away anything? As the executive in charge of conducting the war, he was presumably in a position to retain all gains for himself. To say merely that, "after all, the king cannot hog the profits from war if he expects support from the elite" (p. 71) is to confuse ex ante and ex post decisions and to ignore the question of credible commitments. In any case, the argument fails if the elite does not raise any taxes for

The idea that the elite bears all the costs from a draw is partly defended as a substantive proposition: "The elite is . . . assumed to bear a cost from draws because it is responsible for staffing the army and the administration, both of which are made to work harder during hostilities" (p. 70). But that invites the question whether these costs would not also be incurred in case of victory or defeat. Partly (and inconsistently), however, the assumption is defended as a "convenient" (p. 70, n. 7) device to create "the key policy tension in the model: there are some wars that the king wishes to fight but that the elite does not." Rosenthal notes that there are "other ways of creating this difference," which do not, however "alter the substantive results of the analysis" (p. 70). But would it not be more appropriate to identify the reasons the king was more belligerent than the elite and model them directly?

The mathematical models in which these ideas are spelled out are described at three levels of abstraction. First, Rosenthal asserts that if one tries to work out the implications of the causal links described above, "it is possible to solve for equilibria in a general setting, but obtaining comparative static results requires additional assumptions" (p. 71, n. 9). Second, therefore, the mathematical appendix uses specific functional forms to model the relations between taxes and the cost of raising them, and between taxes raised and the probability of the war ending in victory, defeat, or draw.

Third, in the text Rosenthal draws on numerical illustrations of this model with specific parameter values, asserting that the "results stand up to specification changes" (p. 71). He makes no similar assertion, however, with respect to the choice of functional forms. Why should the reader have any confidence in the results?

Let us look more closely at one central result, concerning the partial explanandum described above. "In the model when either party controls the fiscal system, the tax rate is about 6.3 percent, whereas in the case in which the regime is evenly divided the tax rate falls below 5 percent, a drop in revenue of nearly one-fifth. This finding goes a long way toward explaining why per capita taxation was significantly lower in France...than in England" (p. 73). In a footnote Rosenthal adds: "The observed differences in rates between Great Britain and France are much larger than those found here, a finding that may well be driven by the fact that the elite was not a unified tax authority in France." I find this statement both uncomfortably ad hoc and hard to square with a later statement: "Given that the current model predicts the variation of tax rates properly, it may well be unnecessary to be concerned with elite politics" (p. 89).

In my opinion, the model does not go any way at all toward explaining the observed differences. The fit is poor (as Rosenthal admits), and the model is very artificial (as I have argued). In addition to the objections mentioned above, I cannot see any reason the same parameter values should obtain for all countries. One might expect, for instance, that the resources needed to raise a given amount of revenue (and for a given BNP) would be smaller in England than in the somewhat more sparsely populated France. Also, can one assume that all armies are equally good at converting money into probability of winning? This objection obtains both for purposes of comparative statics and for the purpose of determining the level of mobilization in the enemy.

Levi's Chapter

The third chapter, by Margaret Levi, is a study of nineteenth-century conscription in France, the United States, and, more briefly discussed, Prussia. The explanandum is "the disappearance of various ways of buying one's way out of military service if conscripted: commutation (a fee paid to government) and substitution and replacement (payment to someone else to take one's place)" (p. 109). The explanation relies on "the micromotives of four sets of key actors: government policy-makers, the army, legislators and constituents" (p. 111). As Levi notes later, a fifth actor plays a considerable role in explaining the disappearance of buying-out and its replacement by universal conscription, namely, the largely disenfranchised poor, who "rioted against commutation, making the practice sufficiently costly that government acceded to their demands" (p. 135). Although the explanation is presented as "derived from rational choice and relying on the logic of game theory" (p. 144), the chapter does not

draw on game theory at all and deviates from standard rational choice theory in one important respect.

Let me first address some empirical shortcomings of the section on France. I was surprised to read that after Thiers gave a speech in the 1848 constituent assembly advocating the maintenance of replacement, "the vote of the Assemblée Nationale was 663 against and 140 in favor of his position" (p. 129). In fact, the vote was 663 against and 140 in favor of a proposal by Deville to ban replacement. The book-length account by Levi (1997, 95) has the correct version, so the version in AN must be due to an accident of compression. Other mistakes or ambiguities, however, are common to the book and to the chapter in AN. They concern the timing of the introduction of universal (male) suffrage, the timing of the abolition of buying-out, and the causal link between the two.

Levi inconsistently dates the introduction of universal suffrage in France to 1875 (p. 111) and to 1884 (p. 130), but the dates of 1848 or $1\bar{8}71$ would be equally or more plausible. In the wake of the February revolution, the constituent assembly of 1848 was elected by universal suffrage. The constitution that it adopted also included a provision of universal suffrage. It was reintroduced in 1871, after defeat in the war against Prussia and the fall of Louis Napoleon. As for the changes in military service, Levi writes that "the military defeats of 1870 and the Commune of Paris were to sound the death of replacement" (p. 129). Hence, on her chronology, there could be no causal link between universal suffrage and universal conscription: "Since full male democracy was not achieved until 1884, direct political pressure by those who had come to believe themselves most harmed by the inequities of the system cannot explain the change in governmental norms about military service" (p. 130). Note, however, that she asserts the opposite in an earlier and more theoretical section: "The preferences of the government policymakers and the low-wealth population are the same, thus helping to account for the policies that emerge when the franchise is significantly extended" (p. 118). A similar passage (p. 115) is quoted below. As these statements are not explicitly made about France, I postpone discussing them until later.

Chronology and the facts suggest a close connection between the two reforms. With regard to 1871, the chronology is consistent with the idea that abolition of replacement and introduction of universal suffrage were part of the same ground swell—not that the latter was the cause of the former, but that the two sprang from the same popular sentiment. I know too little about this episode, though, to affirm that there was a link of this kind. I feel more confident in asserting a strong link during the first part of the 1848 revolution. Levi's treatment of this event is confusing. Although she correctly asserts (in her 1997 book) that the constituent assembly voted against a ban on replacement, she also says in AN that "the members of the Assemblée Constituante did overwhelmingly declare support of the abolition of replacement" (p. 128). The two statements can be reconciled by noting the drastic change of mind of the assembly after the June insurrection of the Paris workers. In the first draft by the constitutional commission, presented to the assembly on June 19, 1848 (four days before the insurrection), Art. 107 declared starkly: "Le remplacement est interdit." After the insurrection, the assembly canceled this measure and a number of other radical proposals, notably the right to work and progressive taxation. In this case, it seems clear that universal suffrage and universal subscription were part of the same ground swell, broken only when the bourgeoisie came to see the workers as a threat rather than an ally.

My main complaints with the chapter, however, are logical rather than empirical. I do not agree with Levi's construction of the preferences of the various social groups concerning military service. First, "the traditional elites and wealthy...would prefer a professional army and, second, a system in which they could buy their way out through commutation since, in principle, price is no object. Their third choice would be a system of substitutes or replacements, which would require them to incur some search costs as well as pay a fee" (p. 114). This seems illogical: Search costs and fee add up to a price, which may or may not be higher than in the second case. In the second case the fee is set by the government, in the third by the market, and there is no a priori reason market fee plus search costs should exceed the government-set fee. Also, if "price is no object," why mention search costs at all? Levi also claims that "the preference ordering of unemployed and low-skilled workers and landless peasants should be, first, a volunteer army, then substitution, then universal conscription, and, last commutation" (p. 114). I do not see why this group would prefer universal conscription over commutation. If commutation coexists with lottery, as it did throughout this period, then they would prefer this system (which gave them a chance of not being selected) to universal conscription.

Levi writes in summing up:

If [substitution and replacement tend to dominate universal conscription for everyone], why does so much of the population begin to object to directly purchased exemptions? It may be that, in fact, commutation, substitution, and replacement are not Pareto optimal. As government requires more men to serve, substitution and commutation may increase the probability of having to serve for those who can afford neither of those options. The more people who buy themselves out of the pool of eligibles, the deeper into the pool the army reaches. Thus it may not, after all, be an efficient system (pp. 114–5).

But this argument applies only to commutation. Also, even commutation is Pareto optimal, although, unlike replacement, it is not a Pareto improvement compared to lottery-based conscription.

These preferences are constructed on the assumption that people assessed the alternatives "on purely economic grounds" (p. 114). Levi argues, however, that fairness considerations also entered into the formation of preferences. The existing arrangement

may also begin to be perceived as an inequitable system. Those who cannot afford to purchase substitutes face the possibility of having to become soldiers when they would

rather not; they compare themselves unfavorably both with those who have purchased exemptions and with those who become substitutes instead of draftees and thus receive additional money. . . . If such groups begin to dominate the electorate numerically or resist with riots, then policy change will result (p. 115).

According to Levi, the fairness motive may generate "indirect costs," which salvage "the supply and demand model by expanding the notion of cost" (p. 115).

There are two senses in which considerations of fairness may be taken to generate costs, depending on whether those who harbor such feelings have the right to vote. If they do not, fairness-motivated riots by the disenfranchised may certainly generate costs for government. But if they do, should we say that the feeling of being unfairly treated represents a cost for the low-wealth enfranchised? To answer in the affirmative—as Levi seems to do—would be uncomfortably close to the Ptolemaic devices chastised by Green and Shapiro (1994), that is, people derive psychic benefits from doing their duty. If the feeling of unfairness is conceptualized as an emotion of resentment or envy, then it is inappropriate to view it as a mere cost (Elster 1998b, 1999). If it is conceptualized as a belief that a certain arrangement is wrong, then it need not go together with any subjective feelings of pain at all. Note that in the former case we would expect the resentment of poor conscripts to be directed toward poor replacements rather than toward the rich conscripts whom they replace, on the principle that envy presupposes the belief that "it could have been me" (Elster 1999, chap. 3, section 3).

Weingast's Chapter

The fourth chapter, by Barry Weingast, is an attempt to explain political stability in antebellum America. The central argument can be stated quite briefly. From the Missouri Compromise in 1820 to shortly before the Civil War, American political life was stabilized by a convention that new states would be admitted to the Union in pairs, a slave state and a free state at the same time, thus ensuring (given initial parity) that neither the South nor the North would be able to dominate.

If there were few sustained attacks on slavery—despite its constant presence below the surface as a source of discord—it was because institutions restrained those with the motives to launch such attacks. In light of the potential risk of changes in slavery rights, the South sought to institutionalize some form of credible commitment for the North not to change rights in slaves. The specific form of credible commitment was the convention of equal representation or regional balance in the Senate, giving both regions veto power and, in particular, the South a veto over any policy affecting slavery (pp. 165–6, emphasis in original).

The argument is spelled out in a game-theoretic model that, to the extent I understand it, does not add much to a verbal presentation. If I do not fully understand it, it is not because it is technically complex (it is simpler than other models in the book), but because it introduces a somewhat mysterious plethora of players. I may have missed something, or the exposition may be

the victim of excessive compression from an unpublished book-length manuscript. I shall focus on the simple verbal argument presented above.

What is the evidence for the existence of a convention? Weingast does not cite any relevant statements by contemporaries. Although he claims that "Jefferson clearly understood the principle and its implications" (p. 178), the only backing is a statement by a modern scholar to the effect that an equilibrium obtained. Weingast asserts that "contemporaries" under the second-party system clearly understood sectional balance" (p. 178), but no such contemporaries are cited. The claim that "John C. Calhoun . . . understood this principle" is supported only by a reference to Calhoun's proposal for two concurrent presidents, one elected in the North and one in the South, each with the power to veto national legislation. If Weingast is unable to produce explicit statements by contemporaries to the effect that there was a convention and that they relied on it, then why should we believe there was one?

An answer might be found in a compelling pattern of joint admission of states to the Union. There is, however, no such pattern. Weingast overstates his case: "As revealed by table 4.1, for the next thirty years [after the Missouri Compromise], states entered the Union as pairs" (p. 155). The table shows that the pairs after 1820 were (1) Arkansas and Michigan in 1836 and 1837 and (2) Florida-Texas and Iowa-Wisconsin in, respectively, 1845 and 1846–48. (As a matter of fact, the pairing was more regular before the Missouri Compromise.) This is not a compelling pattern.

If we look at some individual episodes, we may also doubt the existence of a convention. Following the Missouri Compromise, for instance, John Quincy Adams wrote in his diary: "I have favored the Missouri compromise, believing it to be all that could be effected under the present Constitution, and from extreme unwillingness to put the Union at hazard" (cited after Miller 1996, 190). This sounds like a simple one-shot bargain, not the creation of a durable convention. The Arkansas-Michigan admission may also have been little more than a piece of blackmail: "To a degree, as a congressman said, the admission of each state was held hostage to the admission of the other; but really, as another congressman observed, it was more one-sided than that: the admission of the free state of Michigan, about which there was no serious problem, was held hostage to the admission of Arkansas, with its provision of perpetual slavery" (pp. 210-1).

The one-sided nature of the relation between the two parts of the country is underlined by the fact that the North held a majority in the House of Representatives throughout the period. Although Weingast asserts that "the balance rule protected Northerners against the dominance of national policymaking by the South" (p. 151), this is inconsistent with his claim of the "veracity of an important prediction of the model, namely, that antislavery measures should be able to pass the House of Representatives but not the Senate" (p. 168). The North had no need for a Senate veto of antinorthern bills from the House, because its majority in the lower chamber ensured that no such bills would

pass. This feature of the situation is more consistent with one-sided blackmail than with a quid pro quo.

Because I am not a specialist on the period, I cannot offer a persuasive alternative to Weingast's account. Given the facts that he adduces, however, I cannot see that the convention-based explanation is superior to one based on a succession of bargains. The alleged convention is left dangling. What is the mechanism by virtue of which it enabled credible commitments? In a volume that generally tries to spell out the gametheoretic foundations of credibility, it is surprising to see that Weingast does not specify the sanctions that supported the convention. The obvious answer is that if the North had tried to impose its will on the South in matters of slavery, the South would have broken out of the Union. But that standing fact could equally have sustained a succession of deals.

Bates's Chapter

The final chapter, by Robert Bates, is an account of the operations of the International Coffee Organization (ICO) from 1962 to 1989. The main explanandum is the stability of the producer-consumer agreement against free riders on the consumer side. Why did not large American coffee roasters, such as Folgers, break the agreement by purchasing coffee from illicit sources at lower prices? An additional question raised in the chapter concerns the internal organization of the ICO. Here, the explanandum is the allocation of quotas to producer countries.

Bates begins by asking why there was any need for a cartel at all. Why did not the dominant producer, Brazil, try simply to deter other producers from entering the market? Although Selten's chain-store paradox shows that entry deterrence fails when there is full information and a finite number of successive entrants, deterrence can work when there is private information or an infinite number of periods. Bates does not address the strategic use of private information to develop a reputation for toughness to deter entrants (Fudenberg and Tirole 1991, 370). He asserts, wrongly, that reputation effects arise in infinite-horizon games (p. 204). Instead, he argues that the entry of African producers into the coffee market during World War II, whom Brazil could not exclude after the war, turned the market into a one-shot game. Once the African producers had incurred the high fixed costs of planting coffee trees, Brazil could not credibly threaten to drive them out of business by underselling them: "They would be reluctant to uproot their plantations, and instead would simply reduce their inputs of labor" (p. 205). The argument is obscurely presented and relies on an economic model that is unstated rather than explicit.

Next, Bates tells the story of U.S. involvement in ICO. The apparent paradox—why a consumer nation would enter into an agreement to raise prices—is explained by security reasons. After the rise of Castro, "the governments of Brazil and Colombia were able to link the defense of coffee prices to the defense of hemispheric security" (p. 205). Consumer concerns and

electoral preoccupations in the House of Representatives delayed U.S. adherence to the agreement but could not prevent it. Although Bates takes the delay as providing an argument against the realist school of international relations, one might equally well take the ultimate acceptance of the agreement as support for that school.

To explain the stability of the cartel, Bates focuses on the consumer side. In his book-length study of the subject, Bates (1997, 145–50) argues against the propositions that producers were kept in line by Brazilian hegemony, by U.S. enforcement (but see below!), or by redesign of the pricing system to deter free riding. Instead, he argues that stability among the consumers induced stability among the producers. The key element in the explanation is the discount on bulk purchases that Brazil and Colombia offered to large American roasters. The passage linking the discount to the stability of the agreement goes as follows:

[The] reasoning focuses attention on the relationship between those who demanded the agreement (the large coffee producers) and those who organized its supply (the coffee roasting firms). Examining their behavior in a collusive equilibrium, we find that when a large roaster, like General Foods, helped the U.S. government to police illicit shipments of coffee from the competitive fringe—say, Central America-it drove up the price of those coffees. The differential between the price of Colombian and other coffees then declined. Given the differentials in quality between Colombian and other mild coffees, buyers switched to Colombian coffee. As more buyers turned to Colombian coffee, Colombia's dependence on any one given buyer of its coffee declined. The strategies pursued by the parties in the collusive equilibrium thereby lowered the costs to producers of implementing threats to the roasting firms. The reduced costs to Colombia of canceling its contract with any given firm rendered Colombia's threats...more credible. The collusive agreement could therefore become self-enforcing (p. 215, emphasis added).

Once again, the economic model is verbal and implicit rather than explicit, which makes it hard to judge the validity of the explanation. It may be true, or it may not.

The phrase that I emphasized differs sharply from a passage in which Bates (1997, 147) denies the efficacy of U.S. enforcement:

From time to time; the United States followed a second tactic. It signaled to the "competitive fringe" that should they not adhere to the coffee agreement, they would not be eligible for benefits from the Alliance for Progress. While the United States used its aid program to offer inducements and sanctions, little evidence suggests that these measures affected the behavior of nations in the coffee market. Rather, the evidence suggests that such blandishments lacked credibility.

Like the chapter in AN, the book nevertheless asserts the role of the United States in policing illicit shipments (Bates 1997, 152).

One might try to eliminate the contradiction by a suitable interpretation of the claim that the roasters "helped" the United States police illicit shipments. Perhaps the roasters interfered directly in, say, the Central American countries where such shipments

might originate? In that case, the roasters would achieve what the United States could not do on its own by credible threats. This suggestion must be rejected, however, in light of the statement that, "from Colombia's point of view, the discount in the price of coffee to General Foods bought compensating advantages: the help of a large roaster in securing the United States' enforcement of the International Coffee agreement, thereby checking opportunistic behavior by the competitive fringe-and raising the average price of coffee" (p. 214). On a normal reading of this phrase, the roasters ensured U.S. enforcement rather than substituted for it. I have read and reread Bates's chapter in AN as well as his book, but I may have missed something. Even if I have, the need to go through tortuous exegesis to detect exactly what is being asserted does not support the claim that this is an "analytic" narrative. For me, to be analytic is above all to be obsessed with clarity and explicitness, to put oneself in the place of the reader and avoid ambiguity, vagueness, and hidden assumptions. As I have indicated, not many, if any, of the chapters in AN live up to this demand.

In a final section, Bates discusses the internal operations of the ICO. Even when supplemented by other writings (Bates 1997; Bates and Lien 1985; Lien and Bates 1987), the argument is opaque. Bates asserts that one can explain the 1982 adoption of quotas by the ICO in terms of the voting power, as measured by the Shapley value, of individual members. The mechanism for proposing quotas and the constraints on such proposals remain, however, mysterious. In one article (Lien and Bates 1987), there is a reference to a proposal that failed and to a proposal that succeeded, but there is no mention of who made them. By comparing equation 4 in AN (p. 225) and equation 10 in Lien and Bates (1987), we can infer that the successful proposal must have been made by Colombia. (I cannot understand, however, how equation 4 can relate "Colombia's Shapley value to its quota" [p. 225]. Presumably, it regresses the quotas of all countries on the Colombian proposal against their Shapley values.) It might seem (p. 222) that the failed proposal was made by Brazil, but this is inconsistent with the figures offered. On the one hand, under Brazil's proposal, it "would secure over 33 percent of the market" (p. 222); on the other hand, under the failed proposal, Brazil would obtain 30% of the market (Lien and Bates 1987, 631). Or perhaps these percentages are not comparable; I cannot tell.

Bates also asserts (p. 224) that producer countries could make a claim for a quota according to their average exports in one of two three-year periods in the recent past. Which of these two periods they chose was up to them; presumably all countries chose the period in which they had the larger share. If average coffee exports are measured in absolute numbers of tons, then the sum of the individual claims might in theory exceed the total to be allocated. If measured in percentage share of exports, then they must exceed the total when each country can choose the most favorable period. Bates never tells us, however, how much the claims

constrained the proposals. Presumably, they must have left some scope for bargaining—but how much?

Although the Shapley value is uncorrelated with the votes cast on the two successive proposals, and uncorrelated with the allocative quotas in the proposal that failed, it is correlated with the allocative quotas in the successful proposal.

The resolution of the paradox may lie in the following: quotas are established in stages. In the ICO, a proposal is first negotiated, then proposed, and then voted on. It is during the period of negotiations that each nation exercises its bargaining power to shape the proposed allocation. When the proposal is subsequently voted on, however, the nations then appear simply to determine whether they do better or worse under the proposed quota than under the status quo (Lien and Bates 1987, 635).

The claim is not that the Shapley value allows us to explain the outcome of prevote bargaining (and of the subsequent vote). It is the weaker claim that if the outcome of prevote bargaining is correlated with the Shapley value, then the proposal will gain the requisite majority. Is this analysis an application of "rational choice theory" (Bates and Lien 1985, 555, 559)? Apart from the indeterminacy just noted, the use of the Shapley value to explain the allocation of quotas is a deviation from the use of noncooperative game theory in the rest of the book and in the earlier parts of Bates's chapter. The microfoundations of cooperative game theory are by definition shaky.

I am also puzzled by the following statement:

Under the rules of the agreement, bigger producers were endowed with greater numbers of votes; intuitively they should therefore have possessed more power. Equation 5 provides the test we seek. As seen in that equation, the relationship between the voting strength of a nation and its Shapley value is indeed nonlinear; it is quadratic, such that as the number of votes controlled by a nation increases, its Shapley value rises more than proportionately (p. 225).

This seems to be confusing two claims. First, larger nations had more votes (see Bates 1997, 138, for details). Second, the relation between voting strength and power is quadratic, such that when the number of votes rises, the power rises more than proportionately. Even if the Shapley value has been a linear function of the number of votes, the larger producers would have had more power. Further puzzlement is provided by the fact that Bates uses statistical estimation to determine the Shapley value as a function of voting strength, whereas I would have thought that the relation obtains by definition.

It is quite possible that most of these puzzles are just (severe) problems of exposition, not of reasoning. It may seem petty to dwell on them, but I believe the programmatic nature of the volume justifies my doing so.

SOME GENERAL ISSUES

The volume is also beset by some more general and perhaps more serious problems. The objections cited above mainly concern second-decimal issues. Even if the authors had gotten them right, there would still be a number of first-decimal questions to be asked.

The Assumption of Rational Actors

The authors uniformly assume that the main actors acted rationally. With the exception of Levi and Rosenthal (who, albeit not in his formal mode, gives some place to the causal efficacy of religion), they also assume that the actors were motivated by their material interest. In general, of course, rational choice theory can easily accommodate nonmaterial or nonselfish interests. What matters is whether the actors pursue their goals in an instrumentally rational manner, not whether these goals are defined in terms of material self-interest. Yet, even when expanded to include broader goals, rational choice theory is often inadequate because people may not conform to the canons of instrumental rationality.

Let me distinguish between two issues. First, we may oppose rational choice theory as applied by the authors to the paradigm of bounded rationality (e.g., Nelson and Winter 1982) and to the research program variously labeled behavioral economics or quasi-rational choice (Thaler 1991). In the chapters by Greif and Rosenthal, actors (individual and collective) are endowed with the capacity to make very complicated strategic decisions. There is no reference to the fact that for most people the ability to engage in strategic reasoning is severely limited. Consider, for instance, French President Chirac's disastrous call for elections in June 1997. I conjecture that his coalition lost because voters understood that if he wanted early elections, it was because he knew something they did not. something which made him believe that he would lose if he waited. By calling early elections, he revealed what he knew, or at least that he knew something, and therefore gave them a reason to vote against him.

The point is not that this was a failure of rationality on Chirac's part, but that the failure is a very common one. Experiments on the Winner's Curse (Thaler 1992) show that people are very bad at anticipating how behavior may reveal beliefs. In Chirac's case, he did not anticipate that the voters would infer his beliefs from his behavior. In the Winner's Curse, subjects fail to anticipate the beliefs that they themselves will be able to infer from the behavior of others. In both cases, there seems to be a limit to how many inferential steps people can make ahead of time. It is not that it is particularly difficult; it just does not seem to come naturally.

Why, then, should I believe that Greif's Genoans were capable of reasoning through nested games of backward induction, complete with discounting over an infinite horizon? Also, why should I believe in Greif's assumption of exponential discounting when there is "compelling" evidence (Barro 1999, 1126) that time discounting is hyperbolic rather than exponential? It will not do to say that the hypothesis of exponential discounting is a useful first approximation, since it predicts behavior that is qualitatively different from what we would expect on the hypothesis of hyperbolic

discounting. By contrast, the hypothesis of quasi-hyperbolic discounting (Laibson 1997) is a useful first approximation.

Second, the departure from rationality may be much more radical if the actors are motivated by fairness concerns or by emotions. The Ultimatum Game (Roth 1995) is a standard example of a situation in which people are willing to take nothing rather than something, either because they react to being unfairly treated or because they are motivated by emotions of envy or anger. Weingast's chapter, for instance, ignores completely the emotional charge of the slavery issue. Commenting on the congressional debates over slavery before the Civil War, Miller (1996, 184, emphasis in original), observes that "the slavery interest has transmuted into the glorious Southern cause." As he documents, the thinking of the slave owners and their political representatives was seriously distorted by passion. (This may have made their blackmail more credible than it otherwise would have been.) Levi does not ignore these nonrational concerns, although she misdescribes them as rational. Although I know more about seventeenth-century French literature than about the politics of the period, even my scattered knowledge of the latter confirms the overwhelming impression conveyed by the former, namely, that the ruling elites were obsessed with glory and honor. To understand, for instance, the deep pleasure they took in humiliating their rivals, we must read Saint Simon's Mémoires rather than Games and Decisions.

The alternative to the rational actor is not, however, someone who is in the complete grip of passions and completely ignores concerns of security and self-interest. It would be as absurd to deny that the French absolutist kings were preoccupied with revenue as to assert that this was their only concern. They were, in a word, reward-sensitive. That is not to say, however, they were reward-maximizing. Nonrational concerns could often distort their thinking or shorten their time horizon so as to undermine the instrumental efficiency of their behavior. Yet, once their material interests were seriously threatened, they were capable of facing reality. The social sciences today, however, cannot offer a formal model of the interaction between rational and nonrational concerns that would allow us to deduce specific implications for behavior. As mentioned earlier, the idea of modeling emotions as psychic costs and benefits is jejune and superficial. The fact that emotion can cloud thinking to the detriment of an agent's interests is enough to refute this idea. Also, if (say) guilt were simply a psychic cost, then agents should be willing to pay for a pill that would eliminate the emotion-but in reality they would, I believe, feel guilty about doing so.

High Levels of Aggregation

Some of the explananda in the volume are vast historical entities: the development of absolutism, the stability of antebellum politics, the secular expansion of the Genoan economy. Some of the actors that enter into the explanations are huge collectives—clans, the elite,

North, South, and so on. When these are endowed with beliefs and goals and are assumed to engage in complex strategic calculations, credulity breaks down. Rational choice explanations divorced from methodological individualism have a dubious value. Although there may be collective actors who can for practical purposes be treated as individuals, the ones who appear in AN are not—or have not been shown to be—among them.

The most fundamental question is, of course, whether aggregates can coherently be said to have preferences at all. Although social choice theorists have argued that preference aggregation is a fragile business, there may be special cases in which it can be justified. We may be entitled, perhaps, to treat firms as individuals to the extent that they are subject to hard market constraints. Even larger aggregates such as nations may be forced by their own statements to behave consistently. Suppose there is a majority cycle of policies A, B, and C, and that procedural rules lead to the adoption of A, which is then stated publicly as the position of the country. If some of those who formerly preferred C over A change their preference because of the cost to the country of being perceived as inconsistent, even procedural rules that would originally have led to the adoption of C may now simply confirm A. This particular story may not apply to any of the cases in AN. To be taken seriously, however, any account that imputes goal-oriented behavior to aggregate entities has to explain why we should expect consistency in their behavior.

Little Concern for Intentions and Bellefs

One would expect rational choice theorists to take the idea of choice seriously, by looking for evidence that the agents whose behavior they want to explain did in fact have the goals and the beliefs they describe. To some extent, Bates and Levi provide direct evidence about mental states, drawing on interviews (Bates) and parliamentary proceedings (Levi). Weingast tries but fails. The other chapters neglect this aspect of socialscientific explanation. One might say, perhaps, that they are ruthlessly instrumentalist in the Milton Friedman tradition. But to explain observed behavior by intentions and beliefs that are imputed rather than documented is acceptable only if (1) the sources do not permit us to establish intentions and beliefs directly, (2) the observed empirical fit is very good, (3) other implications of the imputed intentions and beliefs are deduced and verified, and (4) plausible alternative explanations are given a good run for their money and then rejected. At the least, I do not feel confident that the chapters by Greif, Rosenthal, and Weingast satisfy these criteria.

Much of applied rational choice theory is a combination of just-so stories and functionalist explanation. One constructs a model in which the observed behavior of the agents maximizes their interests as suitably defined, and one assumes that the fit between behavior and interest explains the behavior. Suppose that higher education tends to make people pay more attention to the future, and paying more attention to the future

tends to make people better off. It is then a tempting step to conclude that people choose higher education in order to reduce their rate of time discounting (Becker and Mulligan 1998), or (in a more general version) that this particular benefit of education explains why it is chosen. It is, however, a temptation that should be firmly resisted, in either version (Elster 2000, 26-9). Unless one can demonstrate an intention (first version) or a causal feedback loop from the consequences of the behavior to the behavior (general version), the coincidence of behavior and interest may be only that—a coincidence.

No Concern for Uncertainty

Except for a minor feature of Weingast's chapter, all the models in AN assume full information. In the real world, of course, high-stake politics is permeated by uncertainty. No model of political behavior that ignores this fact can be successful in predicting outcomes. There are at least five kinds of uncertainty. The first is brute factual uncertainty (will there be a major earthquake in greater Los Angeles over the next decade?). The second is higher-order uncertainty about the cost of resolving first-order uncertainty (do I have time to ascertain the enemy's position before going into battle?). The third is strategic uncertainty due to multiple equilibria (do cartel members play tit-for-tat or sudden death?). The fourth is uncertainty due to asymmetric information (is my opponent irrational or only faking?). The fifth is uncertainty due to incomplete causal understanding (will tyrannical measures imposed by a dictator make the subjects more compliant or less?). The compound effect of these (and perhaps other) forms of uncertainty will, in most complex situations, tend to be overwhelming. At the same time, existing models of decision making under uncertainty, of equilibrium selection, and of games with asymmetric information tend to be very artificial. One might say, perhaps, that it is to the credit of the AN authors that they stay away from these models. Yet, I think the proper conclusion would have been to eschew formal modeling altogether.

CONCLUSION

It is not clear to me what an analytic narrative is. Although the authors deploy rational choice theory, they assert that analytic narratives also could derive from "the new institutionalism...or from analytic Marxism" (p. 3). In the absence of specific examples, I shall understand an analytic narrative as a deductive explanation of individual historical facts, with "fact" being taken in a broad sense that may include long-term developments. In practice, this means rational choice explanation, for the simple reason that rational choice theory is the only theory in the social sciences capable of yielding sharp deductions and predictions. The theory rests on maximization theory, the beauty of which is that it typically yields a unique maximum.

I have argued that the contributions in AN fail to sustain this research program. I have also hinted that,

over and above flaws of execution, the failure may be due to the intrinsic nature of the program. As in the case of sociobiology (Kitcher 1985), deductive rational choice modeling of large-scale historical phenomena may be a case of excessive ambition. But this analogy may be imperfect. In the case of sociobiology, we know from first principles that patterns of animal behavior must have an explanation in terms of biological fitness. The main objection to sociobiological just-so stories is that they are premature, not that some story of this kind will not eventually come to be found. In the seventeenth century Pascal ([1660] 1995, 22) made a similar observation about the Cartesian attempt to create a mechanistic biology: "In general terms one must say: 'That is the result of figure and motion.' because it is true, but to name them and assemble the machine is quite ridiculous—pointless, uncertain, and arduous."

Rational choice history is in a worse situation than that of either mechanistic biology in the seventeenth century or of sociobiology today. The analogy would be appropriate if it were mainly a question of refining the theories and of gathering more evidence. One could refine theory by incorporating bounded rationality and quasi-rational choice, so as to match more closely the way in which actual decisions are made. One could gather more evidence by paying careful attention to sources that illuminate the beliefs and goals of the actors. When we look at the public justifications given for the votes in the first French constituent assembly, for instance, it appears that all deputies had lofty public-spirited motivations. When we read the letters they wrote to their wives, it is apparent that some voted against bicameralism or against an absolute veto for the king because they feared for their life if they did not (Egret 1950). After the publication by Alain Peyrefitte (1994, 1997) of the notes of his conversations with de Gaulle, we can distinguish between what the latter said for public consumption and what really animated his behavior. There is no reason to think that de Gaulle was also being strategic in these conversations, whose substance has been confirmed by his son, Admiral Philippe de Gaulle. By proceeding on these two fronts, one would approach the ideal of explaining behavior by deductive models that rely on (1) realistic assumptions about the way agents make their decisions, given their mental states, and (2) assumptions about their mental states that are independently verified rather then stipulated.

Unfortunately, this will not take us very far. I do not want to insist on the issue of uncertainty. If we can identify the actual beliefs of the agents, we need not worry too much about indeterminacy in rational belief formation. I also do not want to insist on the problem of aggregation. We can proceed from the bottom up to explain the behavior of aggregates of agents, for example, by putting together individual-level explanations of the voting behavior of all members of an assembly to explain the overall vote. In many cases, the voting procedure, including the ways in which alternatives are help up against each other, can simply be taken as parametric. The main obstacle to analytic narratives,

understood as rational choice history, arises at the level of motivations.

As suggested by my earlier discussion, I want to make two claims. First, nonrational motivations are important and pervasive. Wars have been lost because soldiers were taught that it was dishonorable to take defensive measures (Dixon 1976, 54-5). Analyses of why some individuals harbored Jews in Germany or German-occupied countries during World War II whereas others did not suggest that a major factor was that the former were asked by someone to do so (Varese and Yaish 1999). On a rational choice account, this would be a matter of information: To harbor someone, you first have to know about their existence. On an alternative and perhaps more plausible account, it is a matter of the emotional difficulty of refusing a face-to-face request. In war trials after World War II. individuals accused immediately after liberation were sentenced much more severely than those tried for identical crimes two years later (Elster 1998a), which most plausibly can be explained by appeal to the dynamics of anger and hatred. These are scattered examples, which could be multiplied indefinitely.

Yet, if we embrace the most abstract characterization of analytic narrative as deductive history, rather than rational choice history, such facts are not necessarily fatal to the project. To the extent that emotions—their triggering and their dynamics—can be modeled in a way that yields definite predictions, they can be incorporated into an analytic narrative. My second claim, however, is that we do not know how to construct such models. We do not know how to predict the behavior that will occur when an individual is entirely in the grip of an emotion. Fear, for instance, can lead to fight, flight, or freezing, and we do not know which will be triggered in a given situation. We may not even be able to predict which emotion will be triggered. If A favors B at the expense of C, will C feel envy toward B or anger toward A? Also, we do not have good models of the trade-offs at work when emotion and rational pursuit of a goal coexist as motivations. (On all these points, see Elster 1999.)

If pressed, I might have to choose between two versions of the latter claim. Do I mean, à la Pascal, that deductive modeling of emotions is a premature project? Or do I want to state, more strongly, that such modeling will forever remain beyond our reach? Although I incline toward the second answer, my reactions are tempered by previous attempts to legislate a priori what science cannot do-often followed, decades or centuries later, by scientists who carry out the allegedly impossible task. Nevertheless, two reasons make me believe that deductive history will forever remain impossible. First, the micromechanisms, if and when we find them, are likely to be of very fine grain. Second, however assiduously we search the historical record, the evidence is unlikely ever to match the fineness of grain of the mechanisms.

All this is not to say that rational choice theory cannot illuminate historical analysis, as long as its claims are suitably modest. Collective action theory has changed forever the way social scientists and historians

think (or ought to think) about rebellion, revolution, and related phenomena. Hobbes, Tocqueville, and Marx may use language that reminds us of modern discussions of the free-rider problem, but formal analysis is needed to bring out its relation to the subtly different phenomena modeled in the game of Chicken or the Assurance Game. Montaigne and Descartes may have understood at a qualitative level that iterated interactions differ importantly from one-shot interactions, but they did not and could not anticipate gametheoretic results about the conditions under which behavioral differences are likely to arise. Modern analyses of credibility and precommitment have revolutionized our understanding of strategic behavior. The idea of burning one's bridges has always been known, but only after Schelling (1960) has the motivation for such behavior been fully understood. Again, examples could be multiplied indefinitely.

The need for modesty appears in two ways. First, as I have been at some pain to emphasize, one should avoid the postulate of hyperrationality. Collective action, iterated games, and credibility are simple ideas that can be and have been refined to yield rococo (or baroque?) constructions that no longer bear any relation to observable behavior. To be useful, they have to be constrained by what we know about the limitations of the human mind. Second, because formal analysis has nothing to say about the motivation of the agents, it cannot by itself yield robust predictions. Although it is extremely useful to know that the structure of material interests in a given case is that of a one-shot Prisoner's Dilemma, that fact does not by itself imply anything about what the agents will do. If they have nonmaterial or even nonrational motivations, they might behave very differently from the noncooperative behavior we would expect if they were exclusively swayed by material interests. If they are in fact observed to cooperate, then we will have to search for nonmaterial or nonrational motivations. Rational choice theory tells us what to look for, not what we will

In closing, to focus the discussion I might put six questions to the contributors to AN. (1) Do they agree with my characterization of analytic narrative as deductive history and with my statement that in practice this tends to mean rational choice history? (2) Do they agree that a plausible analytic narrative requires independent evidence for intentions and beliefs? (3) Do they agree that a plausible analytic narrative must be at the level of individual actors or, failing that, that specific reasons must be provided in a given case to explain why aggregates can be treated as if they were individuals? (4) Do they agree that standard rational choice theory needs to be modified to take account of the findings of bounded rationality theory and of behavioral economics? (5) Do they agree that it also

needs to be modified to take into account nonrational motivations? (6) Do they agree that at present there is no way to model nonrational motivations and how they interact with rational motivations, at least not in a way that yields determinate deductions?

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The Analytic Narrative Project

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n Analytic Narratives, we attempt to address several issues. First, many of us are engaged in in-depth case studies, but we also seek to contribute to, and to make use of, theory. How might we best proceed? Second, the historian, the anthropologist, and the area specialist possess knowledge of a place and time. They have an understanding of the particular. How might they best employ such data to create and test theories that may apply more generally? Third, what is the contribution of formal theory? What benefits are, or can be, secured by formalizing verbal accounts? In recent years, King, Keohane, and Verba (1994) and Green and Shapiro (1994) have provoked debate over these and related issues. In Analytic Narratives, we join in the methodological discussions spawned by their contributions.

In one sense, the aim of our book is quite modest: We hope to clarify the commonalities in approach used by a number of scholars, including us. We do not claim to be developing a brand new method. Rather, we are systematizing and making explicit—and labeling—what others also attempt.¹ In another sense, the aim of the book is ambitious; by trying to systematize we begin to force ourselves and others to lay out the rules for doing analytic narratives and to clarify how such an approach advances knowledge. We realize that our book is only a first step and concur with another of our reviewers: "As a method, analytic narrative is clearly still in its infancy, but it has promise" (Goldstone 1999, 533).

We have an additional aim: to transcend some of the current and unproductive "tribal warfare," especially between the new economic versus historical institutionalists and between advocates of unbounded and

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Many others write analytic narratives that combine detailed data collection with rational choice or game theory analyses. Among them are Gary Cox, Jean Ensminger, Kathryn Firmin-Sellers, Barbara Geddes, Anthony Gill, Miriam Golden, Stathis Kalyvas, Edgar Kiser, David Laitin, John Nye, Sunita Parikh, Roger Petersen, and Susan Whiting.

bounded rationality. We believe that each of these perspectives brings something of value, and to different degrees the essays in our book represent an integration of perspectives. By explicitly outlining an approach that relies on rational choice and mathematical models, we do not mean to imply that other approaches lack rigor but, rather, to reveal how to apply our tools in a useful and explanatory way. We wish to join the debate, not claim an end to it.

Part of that debate is among the five of us. Achieving a minimal degree of consensus was no easy task. By including a set of individually authored essays that reveal both our commonalities and differences, we indicate the range of possible approaches while also attempting to create boundaries. The five studies all draw from the same general rational choice approach. Even Levi's norms of fairness are modeled in terms of rational choice (in fact, this is part of Elster's objection). We have accepted rational choice theory and are among those attempting to extend it in historical and comparative research. We are imperialists if that means believing, as Gary Becker did when he applied neoclassical economics to the family and to discrimination, that the domain of rational choice can be usefully enlarged. We are not imperialists if that means believing that rational choice theory is the only possible approach to historical and comparative research.

The central problem we tackle in Analytic Narratives is how to develop systematic explanations based on case studies.2 King, Keohane, and Verba correctly urge us to move toward generalizing. They recommend that scholars first consider how to define the universe of cases of which their case is an element and then attempt to devise a way of drawing a sample from that universe. This is excellent advice. The problem is that, for many studies, their approach is not so easy to apply, at least in the initial stages of research. Many political scientists begin with an interest in a particular phenomenon, such as the American Civil War, the French Revolution, the cause of World War I, the fall of socialism, or the rise of the New Deal. At the beginning of research, before formulation of an account of the phenomenon, the universe of cases containing a case of this sort is not obvious. Indeed, only after acquiring a significant understanding of the phenomenon—that is, only after much if not all the research has been concluded—can a scholar have any prospect for defining the larger universe of events.

² We are hardly the first to attempt this. See, for example, Eckstein 1975, George and McKeown 1985, Lijphart 1971, and Przeworski and Teune 1970.

The five of us, representing two different disciplines, all use the analytic narrative approach to achieve insights into different puzzles in very different historical periods. A major problem we address is how to develop systematic explanations for, and extract valid inferences from, such cases. Each chapter identifies a puzzle unique to the place and period under study, offers an explanation by using the tools of analytic narrative, and lays out the more general questions raised by the specific study. Moreover, in each chapter, the author derives a model and confirms its implications with data.

Greif investigates the growth of Genoa in the twelfth century and accounts for the puzzle of how the podestà, a ruler with no military power, resolved harmful clan conflict and promoted economic prosperity. His case has implications generally for issues of factional conflict and political order. Rosenthal models both long-term and divergent institutional change among countries and offers new insights into the relationship between war and governmental regimes by investigating the differences in taxation in France and England in the seventeenth and eighteenth centuries. Levi accounts for the variation in nineteenth-century conscription laws in France, the United States, and Prussia and finds that changing norms of fairness, resulting from democratization, influence the timing and content of institutional change. By focusing on the balance rule and how it deflected civil war in the United States, Weingast advances the program of understanding the institutional foundations and effects of federalism. Bates analyzes the rise and fall of the International Coffee Agreement; he discovers and explains why during World War II and the Cold War the United States, a principal coffee consumer, cooperated with the cartel to stabilize prices. His major finding concerns the circumstances under which a political basis for organization will trump economic competition in an international market.

Our project represents a means of connecting the seemingly unique event with standard social science methods. First, we model a portion of the critical dynamics in a way that affords tests of parts of the idea. This in itself is worthwhile. Second, we go farther and attempt to use the single case to generate hypotheses applicable to a larger set of cases. It is only in developing the account or model that we are pushed toward seeing what components of the account are testable and generalizable.

Before turning to our response to Jon Elster, it is worth considering his approach to social science. Elster's review attacks all forms of social science explanation that attempt to provide systematic answers to political, economic, and historic questions. Analytic Narratives is simply his current foil. Elster has a very circumscribed conception of what constitutes useful theory. He claims (1999a) that social scientists can do little more than develop a repertoire of mechanisms. His observation of "both the failures to predict and the predictions that failed" has entrenched his skepticism about the possibility of law-like explanations, a skepticism "bordering on explanatory nihilism," redeemed only by "the recognition of the idea of a mechanism

that could provide a measure of explanatory power" (1999a, 2). We, in contrast, believe that although we may not be able to derive general laws, we nonetheless can develop, refine, and test theory-driven models and thus employ theory to gain deeper insights into the complex workings of the real world. Our book features both efforts to employ such models and discussions of the difficulties encountered when doing so.³

ELSTER'S QUESTIONS

Elster poses six questions and an additional set of concerns. The first is: "Do they agree with my characterization of analytic narrative as deductive history and with my statement that in practice this tends to mean rational choice history?" This raises two questions, one about the role of rational choice theory and one about deductive history. Yes, we use and have a preference for rational choice theory, but it is not a necessary condition for an analytic narrative. For example, one could use instead prospect theory or any systematic theory of individual choice, including nonrational theories of choice, to generate the predictions of individual behavior. Aside from prospect theory, however, the alternatives are not sufficiently developed to provide a consistent technique for generating behavior in games.

Turning to the issue of deductive history, we observe that analytic narratives are not deductive histories. In particular, we do not deduce the structure of the game from first principles. Of course, all rational choice models are deductive, but the deductive component, especially when applying game theory, assumes the existence of an appropriate game to analyze. Yet, there exists no finite list of games or any reason to believe that there is one. By the same reasoning, we should not expect to find a French Revolution game, the American Civil War game, or the Genoese game. It is at precisely this point that an analytic narrative relies on inductive methods. We take pains to explain that the process of deciding the appropriate individuals, their preferences, and the structure of the environment that is, the right game to use—is an inductive process much like that used in modern comparative politics, by historical institutionalists, and by historians. Once that induction is complete, we can use the deductive methods to study behavior within the context of the game.

Elster's second question is: "Do they agree that a plausible analytic narrative requires independent evidence for intentions and beliefs?" This can be read as concerning the relationship between rational choice methods and interpretive methods that tell us how individuals construct understandings of their world and give meaning to their life. Much of everyday life and of politics is about this, and rational choice theorists have for too long ignored these concerns. Luckily, there is a critical trend among a subset of rational choice theorists who have been trying to integrate interpretive and

³ A related discussion in sociology was begun by Stinchcombe (1968, 1978) and takes its current form in a debate sparked by Kiser and Hechter (1991, 1998; also see the responses by Quadagno and Knapp 1992; Skoopol 1994; and Somers 1998).

rationalist accounts. An article by John Ferejohn (1991) exemplifies the effort, as does a recent book by David Laitin (1998). Some of our own work (Bates, de Figueiredo, and Weingast 1998; Levi 1997; Rakove, Rutten, and Weingast 1999) is self-consciously part of this enterprise.

To varying degrees the chapters in Analytic Narratives contribute to this program. Each of us has labored for years on the subject on which we write. Where necessary, we learned new languages; where feasible, we worked in the field. Each of us spent long periods in the archives, both public and private, and immersed ourselves in the secondary literature on our subject. Indeed, immersion of this sort constitutes a core part of the method we advocate. We seek not only to bring theory to bear upon data but also to bring data to bear upon theory. We strongly endorse the use of qualitative materials, fieldwork, and the painstaking reconstruction of events as anticipated, observed, and interpreted by political actors. Such intimacy with detail, we argue, must inform the selection and specification of the model to be tested and should give us a grasp of the intentions and beliefs of the actors.

There is also a narrower issue that Elster raises. He argues that without independent knowledge of the intentions and beliefs of the actors, the assumption of rationality adds little, resulting in explanations that are tautological. We disagree with Elster's equation of the words of actors with their intentions.⁴ For example, there are two ways to read James Madison. The typical modern political theory approach takes the text and studies its ideas apart from the historical context, assessing Madison's discussion of an ideal world. In contrast, a historical approach embeds Madison in his context and suggests that he was constructing a political document designed to persuade a certain group of citizens to support the proposed revisions to the U.S. Constitution. It is clear from Madison's own writing that he did not believe in this as the ideal.

The point is that an assessment of Madison's intentions is an enormously difficult and necessarily speculative task. Because it is so difficult to judge intentions, rational choice theorists tend to rely instead on revealed preferences and behavior. Indeed, even in instances in which Elster claims we considered intentions directly, we did not do so. When Levi discusses legislative debates, she understands the legislators' public arguments as behavior; their rhetoric is often calculated and strategic, meant to attract certain constituents or change the votes of other legislators.

Thus, to the specific question of the requirement of independent evidence for intentions and belief, we give a negative answer.

Elster's third question is: "Do they agree that a plausible analytic narrative must be at the level of individual actors or, failing that, that specific reasons must be provided in a given case to explain why aggregates can be treated as if they were individuals?" Highlighting the disjuncture between individual and collective rationality, Elster takes us to task for relying on the use of aggregates: "the elite," "the North," and so forth. "Rational choice explanations divorced from methodological individualism," he maintains, "have dubious value" (Elster 2000, 693).

Whether aggregation is justified depends on the extent to which the problem of decision making within the aggregated unit can be examined separately from the interactions among such units. The question is empirical, not theoretical. Aggregation is widely used in economics and political science because it is often an appropriate assumption that leads to tractable analysis highlighting processes at the aggregate level. Firms and families, rather than rational individuals, long stood on the supply and demand side of the economist's market, and political parties—along with rational candidates—contest Downsian elections. In each case, the aggregation assumption makes the analysis tractable.

In the case of the Genoese clans, for example, there is evidence (apart from the implications of the assumption that such separation is appropriate) that directly supports decision making by the clan. The constraint of "one person, one vote" will lead us away from understanding the politics of Genoa as a republic. The same holds true with respect to other chapters. Should Bates go to the level of the individual American and his/her interest in the Cold War?

At least two of us went farther to explore the institutional mechanisms that produce (in the case of Bates) or that fail to produce (in the case of Weingast) a well-defined preference ordering. Contrary to Elster's claim, then, we do not ignore the paradox of collective irrationality but, rather, employ it as a wedge with which to open the analysis of the influence of institutions.

Collectively, however, we urge caution about taking collectivities as actors, capable of formulating and pursuing coherent and sophisticated strategies. Each of us attempted to move to the highest level of disaggregation that was appropriate; thus, Weingast's unpacking of the Senate and Greif's treatment of free riding in the politics of Genoa.

Elster's last three questions are: "Do they agree that standard rational choice theory needs to be modified to take account of the findings of bounded rationality theory and of behavioral economics? Do they agree that it also needs to be modified to take into account nonrational motivations? Do they agree that at present there is no way to model nonrational motivations and how they interact with rational motivations, at least not in a way that yields determinate deductions?

Long-a student of rationality, Elster advances important arguments regarding our use of rational choice theory. As do others, Elster recognizes that people cannot possibly perform the calculations necessary for backward induction. Instead, he urges us to assume bounded rather than full rationality. He also criticizes the assumption that actors can pursue objectives in an instrumentally rational manner, arguing that emotions and other nonrational motivations play a stronger role

⁴ Despite Elster's attempt to say otherwise, how else can one read his statement that Bates through interviews and Levi through interpretation of parliamentary debates "provide direct evidence about mental states" (Elster 2000, 693)?

than we accord them. Finally, he suggests that recognizing the role of nonrational motivations questions the very possibility of modeling in the way we do.

As a team, we are divided over the first of Elster's arguments; some of us agree and find it increasingly difficult to defend the assumption of full rationality as a "necessary convenience." But even those of us discomfited by this assumption reject Elster's implication. Elster implies that inferences drawn from the use of models that incorporate such assumptions must be flawed, whereas we find some of these models highly insightful. They help us understand just how and why the balance rule worked to preserve political order in the United States before the Civil War; just how and why the rules of the International Coffee Organization influenced the allocation of the "coffee dollar" in international markets; and just how and why an official (the podestà) with just twenty soldiers brought peace and prosperity to the most powerful city-state in the twelfth century Mediterranean world. The burden falls on Elster, then, to show how the assumptions we made led us to err. Just how were we misled by them? How would the outcomes have differed had we assumed boundedly rational actors?

We have no difficultly conceding that emotional life is powerful and affects behavior. We also have no difficulty conceding that a rational choice account grounded only in material interests may be both unrealistic and analytically limited. For Elster's critique to carry more force, however, it is necessary that he show how the inclusion of nonrational motivations would improve the power of our particular analyses. We employed models to explain, not to describe; our models therefore need not capture every feature of human life. The ratio between the variance they confront and the variance they explain provides a measure of their success. Until Elster shows how explicit reference to emotional life and other nonrational motivations would enhance that ratio in the accounts we offer,5 his criticism points to an omission but not to an error arising from that omission. He runs the risk of weakening the power of his explanation. At the least, his argument fails to point out errors of commission, as he suggests.

Elster has been a preeminent advocate of rational choice theory and then of considering nonrational motivations. In social science today, the attempt to develop better models of choice and action represents an extraordinarily important and exciting program, one to which we all subscribe to varying degrees. *Analytic Narratives* is, after all, dedicated to Douglass North, whose recent work focuses on developing better cognitive models (1991; also see Denzau and North 1994), and, among us, Levi in particular is identified with the project of understanding the limits of rationality and rational choice (Alt, Levi, and Ostrom 1999; Cook and Levi 1990; Braithwaite and Levi 1998).

Yet, Elster's program fails what Kenneth Shepsle

once called "the first law of wing-walking," which holds that you do not let go of something until you have a grip on something else. We do not throw out models based on rationality just because we agree that there are nonrational aspects of choice. We are likely to learn quite a bit in the next ten years about how to extend choice-theoretic methods to include emotional and nonrational elements. The approach will be much stronger for this. But we do not believe that this now requires abandoning our current tools. Nonrational approaches to choice are not yet far enough along to provide an analytic approach that challenges or extends the traditional choice framework, at least as applied to empirical research outside the laboratory (see, however, Jones 1999). There are, nonetheless, scholars who are beginning to integrate expressive and rational bases of action. For example, Elster's own work on emotions and social norms (1991a, 1999a, 1999b) explores a range of useful phenomena and mechanisms that will prove useful. Sunita Parikh (in process) explores riots as events whose explanation requires understanding a mix of motivations and behavior. Thus, in contrast to Elster's implied thesis (in question six) that "there is no way of modeling nonrational motivations and how they interact with rational motivations, at least not in a way that yields determinate deductions," we believe that there are good first models of how noninterest-based and even nonrational motivations might interact with rational motivations (e.g., Bates, de Figueiredo, and Weingast 1998; Ensminger and Knight 1997; Levi 1997; Lupia and McCubbins 1998). As these new choicetheoretic approaches become more defined and complete, we believe they can be easily integrated into the analytic narrative framework.

UNCERTAINTY

Having attempted to respond to Elster's questions, we now turn to another major criticism Elster makes in the text, our purported failure to deal adequately with uncertainty. He invites us to impale ourselves upon one of two horns, both chosen by him. On the one hand, he criticizes us for failing to employ models that take into account the effect of uncertainty; on the other, he brands such models as "artificial."

Elster lumps together the separate issues of uncertainty and incomplete information. We use the concept of uncertainty in our chapters more than he recognizes. Consider, for example, Greif's analysis, which takes into account uncertainty about who will win the war, whether the emperor will come, whether the podesta and one clan will ally, and so on.

We are more cautious in applying models of incomplete information, however. Because of their complexity, explicit reliance on Bayesian updating (which we doubt people use in reality), and artificiality, we felt that we should apply them only when failing to treat uncertainty would imply ignoring a central feature of the puzzle under investigation. In most instances, we found we could avoid the use of such models. We, like Elster, believe that political actors occupy a terrain clouded by uncertainty, but we also believe it is char-

⁵ Elster attempts to do this in his 1999 books, but he does not demonstrate there or in his review how his claims produce different analyses than the ones we offer.

acterized by sharp institutional features and powerful political forces. Events that take place in such settings do possess a high degree of contingency. When we give insufficient weight to the role of chance, we deprive ourselves of the opportunity to assess the level of confidence with which we can advocate our explanations. If we succeed in apprehending the major forces at play, then the systematic component of our explanations should prevail over that which is random.

"JUST-SO" STORIES

Elster suggests we are guilty of the very sin we try assiduously to avoid, namely, writing "just-so stories." We admit that it is difficult to elude this problem. In the analysis of a single case in biology—or social science—a just-so story can establish the plausibility of the theoretical perspective, but we resisted the temptation to stop at that point. Instead, we laid out the basis for our analytic decisions, indicated the deviations from the predictions of our initial models, and applied the criterion of falsifiability to our hypotheses.

"We identify the actors, the decision points they faced, the choices they made, the paths taken and shunned, and the manner in which their choices generated events and outcomes" (Bates et al. 1998, 13–4), and we do this in each of the single-authored chapters. We attempt to make clear the preferences and to model the outcome of choices. By this means, we aim to offer both a recognizable historical representation and an explanation of significant institutional arrangements and changes. The analytic narrative approach gains credibility when the equilibria of our models "imply the outcome we describe" (p. 12).

Moreover, "when history contradicts the model, so much the worse for the model" (Goldstone 1998, 533). For example, when a model based only on self-interest fails to account for the change in citizen reactions to substitution and commutation, Levi employs an alternative that permits her to explore the implications of the more narrow with the broader rational choice model. In the Bates chapter, the failure of the chain store paradox model highlighted the significance of high fixed costs in the production function of coffee. The Stigler/Pelzman model also foundered, but its internal inconsistency drove Bates to recognize the significance of security interests and the political role of large corporations.

Contrary to what Elster argues, we do not employ formal theory to construct just-so stories. The theories are tested against the stories, and they can and do fail. From their failure, we then learn about the case. We use deductive theories for inductive purposes. As our introduction and conclusion describe, each of us goes back and forth between the model and the data, testing our ideas against reality.

Elster (1989b, 3–10; 1999a, 1–47) himself makes "a plea for mechanisms," for the necessity to explanation of identifying the causal mechanisms. Elster argues that dozens of potential mechanisms potentially underpin human behavior, which he takes to mean that there is no way to predict ex ante which mechanism will come

into play or, if many are at work, which will predominate. If this correctly characterizes Elster's position, then only after the fact can one suggest which mechanisms work. As a consequence, his method is far more susceptible to "curve fitting" than is our own.

We believe that rational choice offers a superior approach because it generates propositions that are refutable. Being subject to standard methods of evaluation—such as the out-of-sample testing of predictions and the systematic pursuit of falsification—the models we employ are not mere just-so stories.

THE CASE STUDIES

Elster finds much to which he objects in our book. By his own admission, however, much of his specific critique of the chapters focuses on secondary issues. What underlies this kind of criticism, however, is his belief that "to be analytic is above all to be obsessed with clarity and explicitness, to put oneself in the place of the reader and avoid ambiguity, vagueness, and hidden assumptions" (Elster 2000, 691). He argues that "not many, if any, of the chapters in AN live up to his demand," (p. 691). We beg to differ. We feel confident that most readers will find that each author produced an analytic narrative which both advances knowledge and produces generalizable and falsifiable implications. And that formal reasoning was key to these contributions.

Rather than go through each chapter and debate each of Elster's specific second-order charges, we addressed many of his more general and theoretical critiques in our answers to his six questions. There remain a few points to which we need to respond.

Greif's Chapter

Elster argues that the analysis does not support the assertion that external threat or internal innovation released resources for use in cooperative endeavors. He is absolutely correct; the analysis does not support his assertion. But it never attempted to do so. The focus is on a different issue, namely, the *motivation* to cooperate, not the *ability* to do so.

Rosenthal's Chapter

At no point does Elster contest Rosenthal's claim that the conclusions follow from his model or the fit between the implications of the model and the historical records. Changing the division of spoils, the "war bias" of the crown, or the incentives of the elite to free ride would indeed change the predictions that issue from Rosenthal's model and lead to convergence between the policies of England and France, just as Elster claims. But then the modified model would be contradicted by fact and thus proven wrong!

⁶ We all draw on related research available in published and forth-coming material. See, for example, Bates 1997, Levi 1997, Greif n.d., Hoffman and Rosenthal 1997, and Weingast n.d.

Levi's Chapter

Elster's condemnation of Levi's logic rests more on misunderstanding and honest disagreement than on pinpointing logical flaws in her argumentation. Elster maintains that "abolition of replacement and introduction of universal suffrage were part of the same ground swell—not that the latter was the cause of the former" (Elster 2000, 688); this is Levi's explanation, too. He critiques her for holding a different view than his about the relationship between fairness and rationality and a distinct interpretation of the actors' preferences. Elster correctly chastises her for inconsistent wording; price is indeed an object, even in the considerations of the wealthy. But he does not seem to disagree on what is easier or less costly.

Weingast's Chapter

Elster questions Weingast's principal argument that Americans constructed antebellum political stability in part on the balance rule. His main criticism is that pairing occurred because southerners blackmailed northerners by holding the admission of northern states hostage to the admission of southern ones. This assertion supports rather than contradicts Weingast's thesis. The South held up admission of free states precisely to maintain a balance, and it used its sectional veto to do this. Elster's comments therefore add to Weingast's argument by showing how the balance rule was self-enforcing during this period.

Bates's Chapter

Elster criticizes Bates for not showing how the consumer side of the market enforced the agreement. Elster appears to be looking for a unilateral form of intervention; indeed, he cites one attempt (by the U.S. government) and conjectures regarding the possibility of another (by U.S. corporations, acting in Central America). But the very form of the explanations advanced by Bates rest on game-theoretic reasoning; rather than seek forms of unilateral imposition, he seeks behaviors that would prevail in equilibrium.

In short, our responses underscore a larger problem with the review: Elster's repeated failure to engage with the main points made by the essays or his misreading of them.

CHARACTERISTICS OF ARGUMENTS

Elster's criticism of our work is, and was intended to be, powerful. It is a "scorched earth" review, and we have tried to resist the temptation to respond in kind. However, we have, on occasion, felt compelled to answer some of his criticisms with a tone that matches his, especially since much of the power of Elster's critique is due to the substitution of his premises for our own and then showing that our results do not follow. He advances his premises so diffidently that it may not appear reasonable to object, but there is little diffidence in the manner in which he claims to clinch

the subsequent argument, even though the argument that fails is his, not our own.

Elster often substitutes his opinions for our judgments. Admitting that he is not an expert on French politics of the period, he nonetheless feels free to reconstruct the preferences of social groupings, as depicted by Levi. If he cannot see that it would be reasonable to suppose that the groups prefer one form of recruitment to another, then perhaps it is because, unlike Levi, he has not immersed himself in the necessary evidence. Although he admits he is "not a specialist" (Elster 2000, 690) on nineteenth-century American politics, Elster nonetheless feels free to "doubt the existence" (p. 689) of a convention regarding the admission of states to the Union. He is entitled to challenge our logic, and we cannot simply resort to our detailed knowledge of the cases as our defense. But given that we attempted to combine reasonable claims with in-depth research, it is incumbent upon Elster to demonstrate how his alternative and abstractly derived construction matters empirically. At issue is more than a question of taste. At issue is a better or worse explanation of actual events.

Elster repeatedly seeks to ensnare his prey in double binds. On the one hand, for example, he calls for the clarity that only, he claims, a formal model can provide; on the other hand, he then attacks a model—"to the extent I understand it" (Elster 2000, 689)—for failing to add "to the verbal presentation" (p. 689). On the one hand, he criticizes a chapter for deviating from "standard rational choice theory" (p. 695); on the other hand, he condemns the authors for depicting actors as being fully rational. He endorses collective action theory but less than a page later brands collective action theory for being "rococo (or is it baroque?)" (p. 695). The authors are thus damned if they do and damned if they do not.

Elster cites distinguished figures, long dead, to add authority to his arguments, but he often does an injustice to their actual words and meaning. For example, Elster enlists Pascal, applying to our enterprise the phrases applied by Pascal to the mechanistic biology of Descartes: "ridiculous—pointless, uncertain, and arduous." When he wrote these words, Pascal had abandoned philosophy for faith. For our part, we are engaged in secular pursuits, and we take inspiration from history's vindication of Descartes; "mechanistic biology" has proven fertile, yielding major advances in medicine and medical engineering.

Rhetoric helps Elster forcefully communicate his low opinion of our work. The tone is Olympian and harsh, and it diverts attention from his failure to engage with the substance of our cases and our method.

CONCLUSION

Elster criticizes us at three levels. First, he claims that we fail to execute the program we propose; that is, we fail to apply skillfully or persuasively formal theories to elucidate complex cases. We believe him wrong and encourage readers to explore the case studies and decide for themselves.

Second, Elster argues that our framework is itself flawed or, at best, premature; he chides us for "excessive ambition" in attempting "deductive rational choice modeling of large-scale historical phenomena." His real opponent is rational choice theory. But as a critic of the analytic narrative approach, Elster fails to engage with the main purposes of our book: How can we build and test systematic explanations based on case studies? How can we move from the world of a problem to be studied to a test to be administered? Moreover, the analytic narrative method easily affords substituting more general modes of "choice" for the explicitly—and limited—rational choice game theory. Put simply, we believe that the debate over choice versus rational choice is orthogonal to the issues we raise.

The burden is on Elster to explicate a better methodological alternative that can actually inform empirical research. This means demonstrating how the weaknesses of rational choice adversely affect the use we make of it. Unless Elster can show how the assumptions of rationality lead us to conclusions that are wrong, he is merely restating obvious truths rather than uncovering material errors. Unless he can offer us a better set of tools, we shall proceed with those we have.

Finally, Elster criticizes Analytic Narratives because we espouse an ambition to a genuine social science. We believe that generalizations are possible and that many have emerged in the social sciences. Our project represents an attempt to bring some analytical tools to the task of studying unique case studies, a question long of interest to political scientists.

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BOOK REVIEWS

Political Theory

Civil Society: The Critical History of an Idea. By John Ehrenberg. New York: New York University Press, 1999. 288p. \$55.00 cloth, \$18.50 paper.

Civil Society, Democracy, and Civic Renewal. Edited by Robert K. Fullinwider. Lanham, MD: Roman & Littlefield, 1999. 408p. \$69.00 cloth, \$26.95 paper.

Alan Wolfe, Boston College

We have passed the ten-year anniversary of civil society, and the end is nowhere in sight. The idea that there is a realm of social life different from either the market or the state received its most visible expression in Eastern Europe, when intellectuals and political activists rallied behind the concept of civil society as they overthrew communist regimes in 1989. But in Western Europe and the United States, the same concept took hold on both the Left and the Right as a way to avoid either the bureaucratic inefficiencies of the social democratic state or the cold indifference of the capitalist market. The contemporary fascination with the idea of civil society has by now lasted long enough to warrant a thorough reexamination, which is what both books under review attempt to do.

John Ehrenberg's Civil Society: The History of an Idea is really two books. One is an intellectual history of civil society that begins with Plato and Aristotle, continues through Cicero and St. Augustine, and then veers off into continental Europe (Kant, Hegel, Marx, Rousseau, Montesquieu) and Great Britain (Hume, Smith, Burke, Ferguson). The second book within a book offers a critique of the idea as it has been applied to contemporary politics in both Eastern Europe and the United States.

Ehrenberg's intellectual history of civil society does not offer an exhaustive scholarly treatment of the thinkers brought up for discussion. Other writers, including Jean Cohen and Andrew Arato and Adam Seligman, have covered much of this ground, and they have a familiarity with European languages that Ehrenberg lacks. But unlike them, Ehrenberg's treatment, concise and well-organized, rarely loses focus. Indeed, considering the range of thinkers discussed, Ehrenberg's treatment of the way ideas about civil society have been discussed in Western political theory is exemplary.

When he turns to the role of civil society in contemporary life, Ehrenberg is, if anything, too focused. For him, discussions of civil society are flawed if they lead to the domination of society by market principles. Looking at Eastern Europe, he is convinced that "it is not 'civil society' that has been restored in Eastern Europe—it is capitalism" (p. 198). Indeed it is, but in the Eastern European situation, capitalism is more hospitable to pluralism than state socialism. Surely it is better to have Adam Michnik editing a newspaper that contributes to open political debate than it is to have Adam Michnik in prison.

Ehrenberg is equally critical of those in the United States who, in the spirit of Tocqueville, urge that we pay more attention to social capital. They fail to realize, he argues, that Tocqueville was writing for a small-scale society far at odds with the global capitalism of today. "In the end," he concludes, "reviving civil society requires the breadth of thought and action that only politics and an orientation to the state can provide" (p. 249). Ehrenberg is certainly entitled to that point of view, but if one takes it literally, there is no reason to discuss civil society at all, since the whole point of the

discussion was to encourage alternatives to reliance on the

Robert Fullinwider's Civil Society, Democracy, and Civic Renewal is a collection of essays and hence lacks the focus of Ehrenberg's book. Most of the essays were prepared in conjunction with the National Commission on Civic Renewal, chaired by former Senator Sam Nunn (D-GA) and William Bennett and directed by political scientist William Galston. Fullinwider's book is distinguished by its wide range. Unlike many volumes that deal with civil society, it includes chapters that examine the international context. It adds historically oriented essays by Kathryn Kish Sklar and Kathleen McCarthy. In other chapters, philosophers examine such issues as the role of public journalism (Judith Lichtenberg) and virtue and vice (Loren Lomansky). Unfortunately, the editor does not offer an essay-length introduction that provides an overview of all the diverse approaches, and conclusions, represented in the book. The result is a series of very uneven contributions, some quite outstanding and some not.

One key question that has emerged from the academic discussion of civil society is whether the term should be reserved for groups, organizations, and movements that serve particular political objectives. Two of the essays argue that it should be. For Benjamin Barber, the whole point of civil society is to encourage strong democracy, which he defines as "an understanding of citizenship conducive to civic revitalization and democratic engagement" (p. 24). Barber has in mind groups that deliberate over policy and activate their membership, such as the Industrial Areas Foundation or the Oregon Health Parliament. The more specific Barber becomes, the clearer it is that, for him, civil society supports one particular side in political controversies and not another. For example, Barber would like to see more efforts at campaign finance reform, environmental protection, and limits on advertising, which suggests that groups who lobby against those goals are somehow diminishing civil society. The problem is that we rarely know in advance which policies will enable civil society to flourish and which will not. Campaign finance reform could indeed revitalize elections, but it could also put a damper on political information.

Barber is at least aware that conservatives as well as liberals have a stake in seeing civil society flourish. Jean Cohen is not. Her essay is dedicated to the proposition that all those who worry about declining social capital "will play into the hands of social conservatives" since their view of the problem is "theoretically impoverished and politically suspect." For Cohen, an organization must be democratic inside if it is going to strengthen democracy: "Hierarchical, authoritarian associations such as the Mafia can easily generate skill in strategic action; the Catholic Church can generate loyalty. But I suspect that only associations with internal publics structured by the relevant norms of discourse can develop the communicative competence and interactive abilities important to democracy" (p. 63).

For Nancy Rosenblum, Cohen's comments on the Mafia and the Catholic Church would constitute a violation of the principle of indeterminacy. As she argues in her chapter in the Fullinwider book, and more extensively in *Membership and Morals* (1998), "The moral valence of membership is neither simple nor predictable" (p. 265). If Rosenblum is right than even membership in hate groups offers moral benefits to members, then surely Cohen is way off base to question whether Catholics can make good democratic citizens. The whole point of civil society is to create spaces in which people decide for themselves the hows and whys of politics. It is a sad commentary on the state of the Left that writers such as Cohen and Ehrenberg, who so distrust

political expressions with which they disagree, leave little room for genuine politics from below. Interestingly enough, the same cannot be said of one of the conservatives represented in the Fullinwider book. William A. Schambra of the Bradley Foundation writes in support of localism and finds common cause with localists of all political colorations, including those on the Left.

One other essay in the Fullinwider book that deserves serious attention is Fredrick C. Harris's chapter on African-American civic life. Responding to Cornel West's assertions that a strain of nihilism runs throughout African-American social behavior, Harris demonstrates the vibrancy of civil society among black Americans, particularly those who have achieved middle-class status. His essay effectively proves the importance of class in matters dealing with race, yet it also oddly concludes with a backhand endorsement of West's argument. Harris's data show "that crvic life has virtually disappeared in poor black communities" (p. 334).

In one form or another, civil society is here to stay. There must be a reason so much attention has been paid to Robert Putnam's "Bowling Alone" (Journal of Democracy 6 [January 1995]: 65–79) or why so many political theorists, such as William Galston and Michael Sandel, have touched a public nerve. To be sure, critics of the nostalgic attitude taken by some of civil society's advocates are correct to raise questions about whether civic life has really declined and whether new, and vibrant, forms of civic participation have emerged. Still, as the historical sections of Ehrenberg's book show, the great political philosophers of the past sharpened their ideas by applying them to the political transformations through which they lived, and that is very much what students of civil society are doing today.

Enemy in the Mirror: Islamic Fundamentalism and the Limits of Modern Rationalism. By Roxanne L. Euben. Princeton, NJ: Princeton University Press, 1999. 239p. \$49.50 cloth, \$16.95 paper.

Cyrus Masroori, University of California, Riverside

For more than twenty years the resurgence of Islam, and especially its "fundamentalist" manifestations, has instigated both fascination and fear in the West. Yet, despite a significant body of inquiries and examinations by policymakers and students of politics, some important questions about political events in places most affected by this Islamic revival, from Afghanistan to Algeria and from Iran to Sudan, remain without satisfying answers. In particular, we still have little to say about how Islam has inspired and mesmerized tens of millions of people, many of whom would readily give their life for the sake of ideas and values that to most average citizens of the West may appear archaic and irrational.

It is against this background that Roxanne Euben initiates an ambitious project and takes a creative approach to reevaluating Islamic fundamentalism as a set of ideas without preconceiving them as inherently archaic or idiosyncratic. In an engaging journey, Euben attempts to answer some of our urgent questions about the meaning and appeal of fundamentalist ideas. After pointing to some problems common to many Western studies of Islamic fundamentalism, Euben offers an account of the method she finds adequate in studying the topic. She uses this method to examine important political beliefs of a leading Islamic fundamentalist, Sayyed Qutb (1906–66). Euben then locates Qutb's thought in a context that includes another well-known fundamentalist (Ayatollah Khomeini), two Islamic modernists (Sayyed Jamal al-Din al-Afghani and Muhammad 'Abduh), and several

Western critics of modernity (including Hannah Arendt and Charles Taylor).

It is particularly refreshing that the book includes a rather extensive methodological discussion, which is frequently neglected by studies of Islamic fundamentalism. Euben begins with a critique of post-Enlightenment rationalism and its social scientific models, such as modernization theory, class analysis, and rational choice. These models generally assume that the process of urbanization, expanding education, and industrialization in Islamic society leads to alienation, which makes Islam, and particularly its fundamentalist form, appealing.

Such models actually distort Islamic fundamentalist ideas by irrationalizing them. One dimension of this distortion is to separate an "essentially" secular West from an "essentially" antisecular Islam. An extension of this distortion is to make fundamentalist ideas subject to categories outside the model's worldview, which leads to marginalization of what defines the very significance of their movement to the fundamentalists.

The alternative, according to Euben, is a dialogic model that recognizes meaning and function are "mutually determinative." Without a grasp of what fundamentalism means to the adherents, we cannot adequately explain what they do. The prerequisite for such understanding is to let Islamic fundamentalist thought express itself. Only then we can seek commonalities, allow the fundamentalists to engage in a dialogue in behalf of their own ideas, and hope for a better understanding of the intrinsic appeal of those ideas.

Guided by the dialogic model, Euben examines some writings of Sayyed Qutb, one of the most widely acknowledged "ideologues" of Islamic fundamentalism. Euben intends to (1) provide evidence for the failure of rationalist models to explain fundamentalist ideas and practices; (2) represent an example of Islamic political theory that is not created by imposing "Western constructs to interpret non-Western cultures" (p. 52); and (3) explore how Qutb's thought was "constrain[ed] and frame[d]" by the "experiences of his life" (p. 56).

Euben begins with Qutb's Ma'alim fil-Tariq [Signposts along the road], which revolves around the concept of Jahlliyya (jahl = ignorance). This refers to the period right before the revelation of Islam, not because human beings were deprived of rationality or unaware of its application to understanding the physical world, but because they were ignorant of the true God (Allah) and His laws (Shari'a), which represent the only right way of living. Modern Muslims have fallen back into jahulyya because they ignore the sovereignty of Allah and His laws.

Modern ignorance, Qutb suggests, is a product of adopting post-Enlightenment rationalism and the political theories and regimes it has spawned. Although he finds no fault with applying reason to examine the physical world, Qutb rejects rational inquiry into "'whys' of the universe," particularly those questions concerned with the essence and purpose of human existence. The latter inquiry has proclaimed a false completeness and competency for human knowledge, portraying religion and revelation as irrelevant and obsolete. Modern jahiliyya is not limited to the West, since many Muslims have accepted and adopted the philosophical conclusions of Western rationalism and are subject to the tyranny of political regimes based on that philosophical ground.

Euben gives a brief review of Qutb's treatment of such topics as the Islamic state, freedom, equality, gender, and race, and she makes frequent reference to his other writings, such as Social Justice in Islam, but the center of attention

remains on Signpost and the concept of jahiliyya. It is mostly because of this concept that Qutb stands separated from and even in opposition to the Islamic modernists of the nineteenth and early twentieth century.

Typically, Islamic modernists such as Sayyed Jamal al-Din al-Afghani explain the decline of the Muslim community as a result of "neglect of science and philosophy" (p. 97), so it is not surprising that Qutb rejects Islamic modernism as an "apologia for Western dominance" (p. 93). Yet, despite such differences, Euben is on target when she reminds us that both Qutb and Islamic modernists are engaged with "Western frameworks and ideas," whether they redefine or criticize them (p. 116). Meanwhile, the basic assumptions of jahiliyya bring Qutb into ideological proximity with Khomeini. Both are hostile toward "rationally acquired" knowledge about essential questions of life, since such knowledge can only be sought in revealed truth.

Although Qutb refuses to describe the institutional arrangements of an Islamic state on the ground that it degrades Islam to the level of man-made political theories, he provides a general prescription for overcoming jahili society. Such emancipation requires jihad, holy war, which aims at liberating human beings from tyranny by establishing a true Muslim society. In such a society, everyone lives and participates according to the laws of God and consequently is free from the tyranny of other men. Euben's suggestion that jihad "had, over time, fallen into disuse [and]... Mawdudi was the first to stress the imperatives of jihad for contemporary Muslims" (p. 74), requires qualification, since in fact the concept was revived in Iranian political discourse in the early nineteenth

Although Qutb's political thought may at first appear alien and impenetrable to a Western reader, Euben persuasively argues that such a reader can successfully interpret his writings. This is possible in part because Qutb himself was influenced by Western political ideas. Euben points to a number of arguments by Qutb that are reminiscent of such Western philosophers as Plato, Rousseau, and Marx. She correctly points out that in the postcolonial world, it is not surprising to find Qutb in part influenced by and responding to debates that are basically "Western." This is important in allowing Western students of Islamic fundamentalism to participate in a dialogue with Qutb. They will find Qutb's arguments less alien if they review the writings of some of the Western critics of modernity.

Euben presents brief sketches of how six contemporary Western thinkers find a crisis of modernity related to Enlightenment rationalism. The review covers Hannah Arendt's examination of the decline of authority, discussions by Alsdair McIntyre, Charles Taylor, and Richard Neuhaus about the modern crisis of morality; and arguments by Robert Bellah and Daniel Bell regarding the decline of community. Euben compares the views of these authors to one another and to Qutb's critique of the modern West, in each case pointing to the differences and similarities among them. The valuable lesson here is that an important requirement for expanding our understanding of Islamic fundamentalism is to situate it "within an extensive and ongoing critique of modernity and the processes that define it" (p. 151).

Because of the book's concern with method and its original questions and insights, *Enemy in the Mirror* is a valuable and innovative project in comparative political theory. Some of the important points Euben intends to make receive only brief attention. For example, one of her goals is to explain the attraction of Islamic fundamentalism based on the intrinsic power of its ideas, but one is left with the impression that this power is almost entirely based on its critique of Western

modern rationalism and the way that it constructs and explains political structures and phenomena.

Another important suggestion made by Euben is that "the experiences of [Qutb's] life and the moment in which he worked constrain and frame the dynamics of his political thought" (p. 56). Yet, Qutb's life, including the intellectual experiences of his youth, receives very little attention, and the social, political, and intellectual environment in which he acted are largely unexamined. Nevertheless, Euben provides valuable points of departure for future research.

The Politics of Judgment: Aesthetics, Identity, and Political Theory. By Kennan Ferguson. Lanham, MD: Lexington Books, 1999. 153p. \$40.00.

John Seery, Pomona College

Is there anyone out there in Political Science Land who still believes in rational choice, not just as an occasional or even frequently useful research tool, but as a true description of normal human behavior? Perhaps, but such people probably are not avid readers of novels. The thick descriptions found in great literature, as with the best in historical writing, usually attest to the unique, complex, unfixed, and irreducible character of human events and their subjects. Distinguishing between idiographic and normothetic forms of knowledge, Max Weber wanted modern social science to attend to the unprecedented and idiosyncratic, even illogical, particulars of human behavior—as opposed to behavior that readily admits to ordered and rule-governed classification. It is fair to say that contemporary political science, however, has largely not followed Weber's lead; accordingly, literary approaches to the study of politics have been shamefully relegated to the outermost margins of the discipline.

In The Politics of Judgment, Kennan Ferguson does not explicitly confront the prevailing predilections of rational choice analysis, and he is not recommending a novelistic method in its stead. But like a well-read Weberian, he turns to aesthetics to recover a complicated, contestatory understanding of politics. The study of politics is fascinating at least because political actors often seem to be guided by manifold motivations that defy summary explanation. For Ferguson, the modern philosophical field of aesthetics, which recognizes the underlying complexity involved in how people go about making judgments, can offer modern political science some valuable and sorely needed lessons. Focusing on judgments allows us, suggests Ferguson, to escape the rationalirrational dichotomy. Judgments, as Kant substantiated in Critique of Judgment, are often made on the basis of taste, not interest, and such tastes involve processes and decisions that are contingent but are nevertheless not merely arbitrary or subjective. Judgments based on taste seem to reflect community or cultural standards that are somehow insinuated within and presupposed by individual preferences. Aesthetics as a field attempts to track the subtle negotiations and interconnections between individual and community tastes, and thus it offers to political theory more sophisticated insights into the gray zones of human alliances. "Judgments are the basis of political identities" (p. 1), Ferguson declares, and aesthetics is the scholarly field best prepared to explore those shifting tides.

Ferguson well knows the dangers commonly associated with attempts to link aesthetics with politics. Aestheticized conceptions of politics can be overly precious or prettified, and wondrous attempts to lend artistic grandeur to the business of governance all too often prove utterly disastrous. History is sadly replete with examples of the visionary

seduction whereby cruelties are executed in the name of art. But Ferguson dismisses such concerns with easy dispatch. Because his political interest in aesthetics is not concerned with questions of the form or content of the artwork, or with issues of the proper meaning and uses of such productions, he avoids all the customary misconceptions and their attendant traps. Rather, his use of the term aesthetics derives from Kant's famous investigation into the public nature of judgment. Kant was the first philosopher to insist on the shared and contested nature of judgment. For Kant, judgments of taste appeal not to standards of truth but by a similar token are usually not reducible to mere subjectivism. They often seem to presuppose various communities of common albeit internegotiated sensibilities. These economies of judgment are not legislated but, for Ferguson, are nevertheless "political" insofar "as they involve the contestation between individuals and groups over ways the world is understood" (p. viii). Political identities are thus based more on judgments than on contracts, and we should first plumb the politics of our constitutive agreements and differences in order to understand our outward allegiances.

Ferguson is clearly on to something big. He is quite right to reacquaint us with a neglected Kantian tradition, suggesting that we should look for political instruction more in Critique of Judgment than in Critique of Practical Reason. He has gathered together an impressive array of political thinkers— Nietzsche, Wittgenstein, Arendt-whose political theories seem to have been greatly influenced by Kantian aesthetics. In respective chapters, Ferguson deftly points to Kantian and Nietzschean combinations in Arendt's mature thought; Kantian and Wittgensteinian combinations in Pierre Bourdieu's tome on taste; and Wittgensteinian and Nietzschean aesthetic influences on later Foucault. All these seminal thinkers reject claims of truth as the appropriate standard for politics, yet all renegotiate, according to Ferguson, shared aesthetic judgments, although in different ways. Ferguson explicates and intervenes in each account (and one can take issue with particulars in each account). In the end he refrains from trying to resolve the differences into a unified aesthetic theory of politics.

The Politics of Judgment is hugely ambitious, and a great deal of territory is covered in a very short book. Middle chapters examine the importance of aesthetics in, respectively, the racialized politics of multiculturalism, foreign policy and nationalism, and the identity politics of gender/sex distinctions. Such topics frequently lend themselves to abstruse expositions, but Ferguson's prose is a welcome model of concision and clarity. Yet, because he attempts far too much, he must take liberties and shortcuts. Often he argues by relying on the testimonies of other authors, and such casual citationality seems pitched not to potential skeptics but to the already converted. In one short stretch (pp. 80-2), he marches through the works of Carol Gilligan, Laura Mulvay, Jacques Lacan, Judith Mayne, Teresa de Lauretis, Kaja Silverman, and Catherine MacKinnon. The result seems squeezed and schematic, the argument asserted but never demonstrated. The book does not enact the agonistics it wants to showcase, and thus its own aesthetics seem strangely at odds with its subject matter.

The finale urges us to view aesthetic judgment as a form of political action, and therewith Ferguson invites us to consider "dissident" aesthetics as politically efficacious. He proposes that cultural disputes over fashion, music, and language need not be liberatory in a Bahktinian sense in order to be transformative and "world-creating." In short, the aesthetics of interpretation can be political, a conclusion that may strike some as a bit vanguardish. Ferguson responds by insisting

that political aesthetics need not be confined to a highbrow elite, although surely his own book betrays that point. *The Politics of Judgment* brims with many admirable traits, but one suspects it will remain an academic work in search of a wider audience.

Living with Nature: Environmental Politics as Cultural Discourse. Edited by Frank Fischer and Maarten A. Hajer. New York: Oxford University Press, 1999. 269p. \$27.50 paper.

Tim Hayward, University of Edinburgh

Environmental concern has become firmly established on the mainstream agenda of international politics. Central to global strategies for dealing with environmental problems is the goal of sustainable development. International agreement about the desirability of this goal has coalesced because it appears to hold out the promise of a win-win scenario, whereby economic development can continue while its environmentally harmful consequences are avoided. Yet, when it comes to devising and implementing appropriate policies, that promise remains largely unfulfilled.

One aspect of the problem is that economic and environmental interests do continue to conflict in significant ways. Moreover, the relative importance of particular environmental goals is seen differently in different parts of the world, with striking contrasts in perspective between the richer and the poorer countries.

According to Fischer and Hajer, however, the problem is not only an opposition between development for the poor and conservation for the rich; if current strategies of sustainable development do not live up to their promise, this also has partly to do with how the goal itself is construed. The dominant "discourse coalition" of sustainable development is essentially a "managerial" one and has a global sphere of reference; in both these respects, they believe, it is vulnerable to criticism. On the managerial approach, environmental problems are supposed to be identified by objective scientific means, and technical solutions then are proposed to deal with them. Existing institutions are adapted to this task of implementation, but an argument of this book is that "an institutional approach that purports to deal with the challenges of environmental degradation, while leaving aside basic cultural and political questions about the institutions implicated in producing the crisis in the first place, has become very vulnerable to radical critique" (p. 5). As for the global turn in environmental politics, they claim, this involves a dramatic reduction in the complexity of the problem and thus does not provide an adequate basis for arriving at solutions. Moreover, this new global environmental discourse "has suggested much more unity and shared understanding than can be legitimately assumed" (p. 9).

A better approach to environmental policy, the authors believe, would pay more attention to cultural politics, that is, to "the particularity of the way societies relate to the natural environment" (p. 6). The dominant managerial discourse of environmentalism is grounded, they believe, in a set of specific cultural assumptions that are not universally shared and that do not merit unqualified endorsement. Thus, a stated aim of the book is to examine critically the various practices through which particular ways of relating to nature are reproduced so that the cultural underpinnings of present strategies may be politicized.

The book is divided into three parts. The first consists of chapters offering general considerations about the social construction of understandings of the natural environment. A

central message is that, although one should heed the fundamental ecological insight that humans are, after all, part of nature and not something apart from it, one should not forget that the ways humans actually understand and relate to the rest of nature are deeply permeated by specific cultural interpretations and values. Thus, Yrjo Haila says the challenge for environmental politics is to retain an awareness of the earth as a whole without losing our sensitivity to the cultural particularity of entities, processes, and situations.

The second part considers specific cases of how contrasting understandings of the natural environment are constructed. The contributors seek to show, for instance, how science is used to impose a particular cultural view of nature upon local communities. Peter Taylor's case studies from Mexico and Africa illustrate some ways in which scientific techniques simplify reality in an effort to control the multiple manifestations of complex phenomena. To uncover the processes through which such simplifications occur, he suggests, we need to investigate the cultural politics involved in constructing scientific knowledge. From the chapters of this part, a strong suggestion emerges—in keeping with much current environmental political theory—that a more discursive democratic politics is an important part of the answer to an equitable approach to environmental protection.

Part 3 has contributions on environmental politics in relation to cultural difference. David Harvey provides a particularly useful overview of the principal environmentalist discourses to be found today. He offers an exposition and critique not only of "standard" managerialist discourse but also of positions of more radical green groupings. His main interest, however, lies in theorizing the prospects and problems of the environmental justice movement, both as manifest within U.S. politics and as linked to a more global "environmentalism of the poor." He indicates various ways in which socioeconomic disadvantage are reproduced and reinforced through discursive framings of environmental issues, which at times are tantamount to "cultural imperialism." Nevertheless, on Harvey's account, contrasting cultural construals of environmental problems are firmly grounded in fundamental issues of national and international political economy. A particularistic politics of place is not in itself the answer unless or until harnessed to a more universalistic movement that comprehends and seeks to take on the systematic obstacles constituted by the global capitalist order: "The fundamental problem is that of unrelenting capital accumulation and the extraordinary asymmetries of money and political power that are embedded in that process" (p. 184).

This is a useful reminder that much of what is relevant about cultural differentiation and particularity is relevant precisely because of underlying relations of oppression and exploitation. This point is not always so evident in discussions of environmental politics as cultural discourse.

The book is intended to counteract aspects of abstraction in environmental discourses by attending to concrete particularities of diverse cultures. Yet, any general theory of the importance of particularity has to be alert to potential paradoxes about its own status. To my mind, not all contributors show sufficient alertness. Recommendations about what a sociologically nonspecific "we" need to do or learn crop up more often than one might expect in a book of this kind; some contributors effectively advocate their own preferred view of, or alternative to, sustainable development; others advocate a restoration of "spiritual traditions" or establishment of an ethic of "partnership with nature" whereby "Nature, along with women and minorities, will speak in a different voice" (p. 223). The editors themselves

evince a nostalgia for the earlier (affluent Western) green discourse, which emerged as a countercultural "critique of industrial progress" and of "endless material growth and consumption." Yet, if the most relevant cultural differences apply to those who have hardly even started to enjoy growth and may in fact have all but stopped consuming, it may be somewhat premature or overly sophisticated to speak, as they do, of transcending "the old, simplistic opposition between development for the poor and conservation for the rich."

Meanwhile, whatever the shortcomings of the dominant discourse of sustainable development, it has ushered environmental concern onto the mainstream agenda; it does at least embody some recognition in principle of the issue of equity; and its institutions do engage to some extent with realities of economics and political power. Although the discourse of sustainable development may indeed be vulnerable to radical critique, the question remains as to where, concretely, that critique may lead. Essays extolling in a general way the virtues of attending to particularity I found less rewarding than those that focus on specific cases of contrasting understandings. Yet, when it comes to cases, the striking differences alluded to in general arguments appear rather more nuanced; and where striking differences do indeed appear, they often can be-and perhaps most usefully are-comprehended as questions primarily of injustice. This is not to deny that specific cultural values and assumptions can be both a source and a medium of oppression; and the most valuable parts of the book reveal how this is the case. But going too far down the cultural theory route risks obscuring the woods for the trees: Some issues of environmental justice may centrally involve contrasting cultural interpretations, but many of the most serious environmental problems afflicting the world, especially its majority poor population, seem evident enough without extensive cultural analysis. Still, if that analysis proves helpful in devising specific solutions, it is of course to be welcomed. I would thus commend this book for its central aims, which are well conceived, and for those contributions to it that most faithfully pursue them.

The Theater of Politics: Hannah Arendt, Political Science, and Higher Education. By Eric B. Gorham. Lanham, MD: Lexington Books, 2000. 235p. \$65.00 cloth, \$24.95 paper.

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Although the title suggests this work will examine the philosophy of Arendt, Gorham instead focuses on applying Arendt's distinctive conception of politics to various topics, especially the college classroom. He is deeply concerned about the loss of citizenship in the United States (p. 22). He raises important issues for the development of civic-minded citizens, advocating that political science faculty make the classroom more a place of student practice of politics and the activity of political judgment, that is, give students the opportunity to "appear in public" (p. 195). While good citizenship requires active involvement, most citizens understand politics only as spectators (p. 1); Gorham believes Arendt's theory frames action "in particularly useful ways" (p. 3). He offers good suggestions on how to engage students in class and how to structure some out-of-class experiences.

This book is organized into an introduction, eight chapters, and an appendix. It analyzes the idea of politics as theater; examines "reporting" and the relation of "facts" to politics (including the Eichmann trial); applies Arendtian ideas of citizenship to the university; applies Arendt's council-democracy to the European Community; examines the idea of space of appearance; discusses the practice of political action in

class; and suggests ideas about out-of-class community service. Gorham hopes "to bring out meaning in [Arendt's] work" (xvi, n. 1) by viewing the university as an opportune place for citizens to develop the capability of judging political action. Although much of the analysis depends on applying an interpretation of Arendt, Gorham only tangentially analyzes Arendt and her interpreters and critics. He believes the political science faculty ought to stimulate students to "enlarge their mentality" and "make informed and reasoned judgments" (pp. 94, 96).

A good application and critique of Arendtian theory is provided in the chapter that applies the Arendtian notions of political action and council-democracy to the development of the European Community. Gorham differentiates "movement" politics from political parties, noting that European parties are more successful in answering the "social question." Parties represent interests, not "opinion or action," and are less "political" in Arendt's sense than the movements (p. 119). Gorham observes that the practice of discussion can contribute to the demise of free public spaces because certain forms of discussion "transform free spaces into bureaucratic ones" (p. 119). He finds that Arendt's theories apply well to the early postwar European "space of appearance," but her theory fails to "account for the difficulty... of creating stable, free, democratic spaces of appearance" (p. 116). One is left to wonder about the consequence for our understanding of Arendt's theory. Does Arendt not put enough emphasis on institutions? If Europe's democratic deficit is related to a flaw in Arendt's theory, is the missing element a lack of analysis of character?

If it is a defect of Arendt's theory that foundational and revolutionary spaces can ultimately destroy themselves, why does Gorham proceed to apply Arendt's theory to the classroom? He recognizes that Arendt did not believe that classrooms were spaces of appearance (p. 156). Assuming one could choose to extend her theory of politics as theater to the classroom, there may be valuable insight from recognition of theatrical aspects of classroom teaching. When faculty present differing, "often opposing interpretations," they are not merely imparting "truth" but exploring the meaning, the significance of political action (p. 162). For Gorham, a teacher who questions a text and performs a dialogue "presents the author as an actor" (p. 163). This technique is an attempt to get the student to develop "a taste for thinking." Unfortunately, this is not Arendt's view of "action," which would require the living, talking author to "appear" before the students. Gorham does not suggest we do away with lectures, but that faculty should offer in-class opportunities for role playing and political action (pp. 166, 167). In my view, the more valuable part of this exercise for the student may be to see the teacher as thinker, questioning the meaning of the text, not as actor.

Gorham believes faculty must "politicize" the classroom, which means to heighten students' awareness that they "are engaged in some artificial practice" (p. 167). This would include encouraging student participation in the university's academic policy process (p. 172) and recognizing the economics of the student's relationship to society (pp. 167-8). Yet, as Gorham points out, Arendt did not want the economic, social, and "administrative" questions to impinge on what she conceived as purely "political action" (pp. 54, 118, 145). Community-service learning can be valuable, says Gorham, because it teaches "how to act, in the Arendtian sense," although he notes there is no guarantee of political learning or of civic virtue (pp. 183, 188-9). The author is aware that a possible outcome is mindless activism, and he wants to build in "incentives" that would help students stop

and think about what they are doing (p. 198). I wish he could have been less vague about how to accomplish this thoughtfulness.

Several issues can be raised about the book. First, although Gorham correctly describes Arendt's distinctive view of 'politics," he does not adequately examine her intentional disparagement of economics and administration. Does this detract from the adequacy of her theory to conceptualize politics? Are there unintended normative consequences? Second, there is a serious problem with Gorham's call to politicize the classroom (for Arendt this does not simply mean partisanship): To what degree is student "practice" of politics going to inculcate good judgment? Yes, Gorham wants students to experience the "vita contemplativa" (p. 160), but what more is this than the time-honored attempt to develop critical thinking skills? Do we really want to politicize our classes and our research (pp. 106, 157, 184)? Third, Gorham wants students to develop judgment and is aware that they need practice. But what does it mean to say citizen decision making should be based on "taste and enlarged mentality of the person judging" (p. 107)? Finally, politics to Gorham is an expressive art, an action that takes place in a "space of appearance" (pp. 29-35). He does not address the adequacy or relevance of other notions of politics, such as the process by which we make the rules by which we live in common; or determine who gets what, when, and how; or allocate values.

Several rewarding treatments of the political thought of Arendt, among the abundant works, are: George Kateb, Hannah Arendt: Politics, Conscience, Evil, 1983; Margaret Canovan, Hannah Arendt: A Reinterpretation of Her Political Thought, 1992; and Michael Gottsegen, The Political Thought of Hannah Arendt, 1994.

Adam Smith and the Virtues of Enlightenment. By Charles L. Griswold, Jr. Cambridge: Cambridge University Press, 1999. 412p. \$59.95 cloth, \$21.95 paper.

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Charles Griswold poses a series of alternatively fruitful and frustrating questions. He begins with an excellent query for consideration by moral and political theorists that he then chooses not to address: "Is there a sense in which [others'] misery is necessarily the price of our happiness" (p. 3)? Instead, it quickly becomes apparent that the motivating question of this study is to be a rather more provocative and self-absorbed one: "Might the apparent devolution of liberty into spontaneity, or pluralism into relativism, of knowledge into technology and thence into the self-vitiating mastery of nature, or science into a 'worldview' produced by a given historical milieu, of culture into vulgarity, of reason into imagination and then into fantasy—in short, the devolution of the Enlightenment into what is widely termed 'postmodernism'-itself be a natural consequence of the very premises of the Enlightenment" (p. 3)?

Griswold contends that our response to this largely unanswerable (if not wholly nebulous) question depends on how we analyze "the virtues and vices of the Enlightenment" (p. 3). In this analysis, he claims to follow Leo Strauss and Alasdair MacInytre in viewing the Enlightenment as a "fundamentally mistaken rejection of ancient and medieval philosophy" (pp. 3-4). He then asks: "How can the older, virtue-centered tradition be made to mesh with modern political, jurisprudential, and economic practice" (p. 4)? This series of interrogatives brings us to one last question, which reveals that the true aim of the book is to resolve all this anget

about the twin Enlightenment/postmodernist threats to ancient virtue. So, Griswold asks, before rejecting the Enlightenment for its inability to return us to the virtue of the ancients (or some simulacrum thereof), might we not first look to see whether there are "overlooked or misunderstood resources for self-criticism and justification in the Enlightenment itself, especially ones that also provide for the preservation of desirable aspects of ancient thought" (p. 7). It is in this search that both the political economy and moral theory of Adam Smith become one such resource—a kind of moral tertium quid.

Griswold begins by treating the Enlightenment as something of a "black box" until he arrives at Smith and recognizes that it is a complex and multiple idea. "Critics of the movement were part and parcel of it from the start, and Smith himself may in some respects be counted among them" (p. 9). At this point, some might say that the motivating question for the study falls away, since out of a "movement" realistically so complex, few, if any, natural or necessary consequences could be shown to follow. Even so, we are left with a number of thought-provoking insights on Smithian "sympathy" and moral imagination. The book is at its best when Griswold turns his considerable powers of textual and philosophical analysis to some of the issues dominating more recent considerations of Smith's work, such as the theory and practice of rhetoric, Smith's use of examples, and particularly the "theatrical" character of the moral and commercial world (p. 66). Here Griswold certainly adds to our understanding of Smith's larger moral as well as economic and political concerns. Notably, he enlarges upon and corrects the view of Smith's "impartial spectator" and its function in his theory of literature and moral education, presented most recently by Martha Nussbaum (Poetic Justice: The Literary Imagination and Public Life, 1995) (pp. 9, 42, 68).

At the same time, one might question the overly optimistic rendering Griswold offers of Smith's own understanding of the implications of an advancing and future commercial society, and the function of the market mechanism. "Smith's moral, political and economic doctrines are geared toward explaining how individuals as well as nations can live together harmoniously in spite of the ever present potential for conflict" (p. 10). "Spontaneous order" was not a concept Smith himself employed, irrespective of his later Hayekian interpreters. A contemporary economist might tend to characterize Smith's inquiry as a harmony theory, but it is equally one in which competition is the central, driving force of development. Smith's caustic references to landlords, who "love to reap where they never sowed" (Wealth of Nations, I.vi), to the need for police to ensure the property owner a decent night's sleep (V.iv), or to the justified demands of laborers for "combinations" in order to resist manufacturers' greed for profits (I.viii), indicate that conflict within his vision of an evolving commercial society was something more than merely potential. Rather, conflict seems woven into the very warp and woof of economic life, however much of the component parts of price, and with it the great orders of society, might be argued to produce an integral whole. Indeed, it was in large part this recognition that led Marx to consider Smith a classical rather than a vulgar political economist.

Certainly, Griswold is right that the combined effort of Smith's moral, political, and market vision "seeks to free us from war and faction" (p. 10), but Smith does not claim—as his modern economistic interpreters would have us believe—that the market can perform this feat for us single-handedly. And Smith does not engage us in dichotomizing the ancient and modern worlds along the axes of consensus and conflict,

as the author seems to suggest (p. 10). Surely, Smith's "view of the world" and our study of his "careful attention to moral sensibility" (p. 10) are lent no more credence by reference to "the catastrophic collapse of moral sensibility and the corresponding butchery in our century" (p. 10) than by reference to the accounts of butchery offered by Thucydides or the moral collapse of the Crusades.

At the same time, it seems strange to argue that the political economist who made famous the term "invisible hand" believed "our destiny is in our entirely visible hands, waiting to be crafted by us" (p. 15). Such a confident reliance on the efficacy of human agency seems to fit poorly with Griswold's own recognition of the place of "moral luck" (p. 16) and the Stoic-inspired "propensity for irony" (p. 16) present in Smith's account of the four stages of history. Yet, if Griswold demonstrates some of the difficulties of attempting to render Smith utterly relevant to our contemporary concerns, he offers for consideration one generous and synthetic account of the relationships among Smith's aesthetic, moral, political, and economic thought.

Messianic Revolution: Radical Religious Politics to the End of the Second Millennium. By David S. Katz and Richard H. Popkin. New York: Hill and Wang, 1998. 303p. \$26.00. Apocalypses: Prophecies, Cults, and Millennial Beliefs through the Ages. By Eugen Weber. Cambridge, MA: Harvard University Press, 1999. 288p. \$24.95.

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The turn of the millennium has invited a good deal of attention to the topic of millennialism. It is a welcome opportunity for scholarly reflection on an abiding feature of historical experience, despite its curious absence from our own millennial celebrations. The publication of the volumes by Katz and Popkin and Weber have all the earmarks of efforts to take advantage of the contemporary interest in apocalypses and messianic expectations. The price of the cloth editions attests sufficiently to the publishers' faith in the project. In other words, neither book is intended for specialists in the field of apocalyptic studies, but they are none the worse for their general appeal. Indeed, they are in different ways commendable attempts by historians of ideas to address the present moment.

The principal value of both volumes is that they illuminate the deeper sources of millennial speculations that perpetually hover around the margins of mainstream public discourse. As such, they provide invaluable insights into notorious but little understood fringe groups. Much of the focus of Messianic Revolution is on demonstrating the shortcomings of law enforcement efforts to deal with the Branch Davidians at Waco, the Freemen of Montana, and the various militia and survivalist movements that remain a persistent irritant of our otherwise placid prosperity. Katz and Popkin, although they provide a survey that connects the American variants of millennialism to biblical and early modern sources, devote most of their attention to the Protestant world and the United States in particular. Their account is an invaluable survey of the creative streams in which apocalyptic inspiration flowed following the Reformation and which formed such a central ingredient in what Harold Bloom calls the "American religions."

In many ways Katz and Popkin have the broader net within which to capture radical religious movements. In their category of "messianic revolution" one does not expect to encounter the scion of the scientific revolution, Isaac Newton, but there he is, displayed in all his speculative glory and

demonstrating the continuity between science and the calculation of the apocalypse. Equally valuable is the retracing of the complex relationships between the foundation of a Jewish state and the various strands of millennial expectation. An extensive discussion of the theoretical contribution of John Darby (1800–82) to millennial discourse underlines the rarity of development in what is essentially a static expectation. Ironically, the anticipation of the greatest transfiguration in history can only be sustained by an unvarying detachment from actual history. Darby introduced the notion of the "rapture" as a distinct phase in the expectation, one that allows the possibility of individual transfiguration in advance of the final cataclysm.

Weber deals with many of the same characters, but he ranges more broadly over the whole history of apocalyptic expectations. As a result, he provides a context beyond the strictly religious forms of radicalism. Weber is more willing to explore the points of convergence between the secular and religious apocalypses, especially through their conjunction in postmillennial traditions. The latter emphasize the degree to which historical progress is engaged in preparing a state of perfection, after which the advent of the millennial reign of Christ on earth can take place. In contrast, the premillennial enthusiasts emphasize the unregenerate condition of the world, which can only be regarded as incapable of transformation and therefore ripe for a cleansing apocalypse before the reign of Christ becomes possible. Not surprisingly, premillennialists have formed the dominant strand among chiliastic movements. Revulsion at the degenerate state of the present is always a more effective motivation than optimistic hope for incremental improvement. But the postmillennialists are more in tune with the progressivist outlook of modern civilizational history as a whole. Weber certainly draws attention in passing to the connection, but both he and Katz and Popkin fail to take the full measure of the relationship.

Despite the informative, accessible, and well-documented treatments provided by both volumes, they suffer from a theoretical limitation in their conception of apocalypse. In large measure because they take their own starting point in the agnostic perspective of contemporary historiography, they are inevitably held captive to conventional categories of analysis. Apocalyptic speculations are on this conception the preserve of extremist religious movements that operate at the margins of the historical mainstream. The possibility that the same mood might for extensive periods infect the mainstream itself is rarely confronted in its full significance. As a result the forest of apocalyptic longing is missed in fascinating studies of the trees of a particularly luxuriant variety. Politically, this is a serious defect, since it is precisely the apocalyptic appeal that has provided the impetus behind the most powerful movements of the late modern era.

We enjoy an almost unprecedented moment of global peace precisely because of the disappearance of the last political movement of apocalyptic intensity. Communism endured for almost a century because it could draw on the counterfactual faith of all apocalypticists. No amount of evidence to the contrary can dislodge the expectation of a transfiguration that bases its claim entirely on the future and not at all on the present. The other most destructive political movement of our era was equally rooted in the representation of a self-styled apocalypse. Nazism took over the classic Joachimite claim of a third age, a third Rome, a third Reich. A widespread style of extremist politics received definitive expression in the purest revolutionary faith of all, that of anarchism. Since a transforming apocalypse was the necessary phase by which the end of history would be reached, it was pointless to speculate on the character or content of that

new age from the unregenerate present. All that could be said was that it would be radically other than the condition we know, and it could be hastened only through the work of universal destruction. As a tactic this inspiration became internalized in the phenomenon of terrorism. The whiff of apocalyptic rhetoric even finds its way into the musings of liberal statesmen, as in FDR's famous "Four Freedoms" speech.

This is surely a significant material oversight. It is not as if the connections between the totalitarian, the revolutionary, and the religious forms of apocalypse have gone unnoticed. Weber even refers to the connection between nineteenth-century socialists and their Christian counterparts, but he does not contemplate the apocalyptic appeal of the revolutionary movements as such. There is no mention of the classic study by James Billington, Fire in the Minds of Men: Origins of the Revolutionary Fath (1980), or of the extensive literature on political messianism throughout the modern era. So long as one sticks with the assumption that apocalyptic longings are the preserve of marginal sectarian cults on the religious Right or Left, one is bound to overlook the extent to which the same appeal is operative in virtually all forms of radical and revolutionary politics.

The defect is essentially methodological, although it leads to empirical narrowness. Perhaps the source of limitation lies in the all too confident assumption that we could never be taken in by such eschatological fantasies. Despite the evident open-mindedness of the authors, they display a distinct lack of interest in probing the sources of the apocalyptic experience itself. Why would anyone place reliance in such visions of an all-transfiguring end of the world? Where do they come from, and what finally explains their growth and decline? The kind of depth analysis of the experiences and symbols of the transfiguration available in the work of Eric Voegelin, for example, is entirely neglected. Without reaching some clarity on the inner structure of apocalypse as a mode of existence, it is inevitable that scholarship will remain captive to the prevailing prejudices. Even given this serious limitation, however, both volumes provide a compelling and informative account of the rich diversity of millenarian and messianic expectations that swirl around the mainstream of religious and secular consciousness.

Principles of Social Justice. By David Miller. Cambridge, MA: Harvard University Press, 1999. 337p. \$45.00.

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Miller's aim in this clear and well-written book is to defend a plural theory of justice that consists of three component principles: desert, need, and equality. Rather than try to combine our beliefs about justice with highly abstract principles, Miller thinks we should "begin by trying to discover the practical principles that guide those beliefs" (p. 24). Michael Walzer (Spheres of Justice, 1983), with whom Miller has much in common, claims to find such principles in the meanings of social goods, but Miller prefers a different strategy. Rather than unpack the meanings of goods, he looks to the various ways in which human beings interact, to what he calls "modes of human relationship" (p. 25). In brief, the author argues that a principle of justice as need is associated with the mode of "solidaristic community," desert is similarly relevant to "instrumental association," and equality to "citizenship" (p. 26).

Miller's claim is that these modes of relationship "call forth" their associated principles of justice (p. 32). More precisely, a particular principle of justice will require a

particular relationship to be feasible. Moreover, the mode of relationship makes some associated principle appropriate; they fit together. As the defense of the fit between certain types of relationship and principles of justice cannot be asserted as a matter of logic or conceptual clarity, Miller invokes evidence of meaning and usage as well as a style of justification associated with ordinary language philosophy. Thus, he looks to social science to tell us how people think about justice and how they use the term.

The conclusions are presented in chapter 4. First, our ordinary use of justice is pluralistic and complex. Second, there is some support for the connection made above between certain types of interaction and certain governing principles of distribution. Third, desert in some primitive, preinstitutional sense plays an important role in people's thinking about justice. Fourth, need is also important, but people give short shrift to what they consider mere wants and preferences as against genuine claims of need. Fifth, what people think about equality is hard to determine, but it is clear that they think the current inequalities in income and wealth in contemporary liberal democracies (notably the United Kingdom and the United States) are far too great.

Much of Miller's book is a working out and defense of these positions, most notably the third: that a theory of justice must give a prominent place to a principle of preinstitutional desert. Miller's contextualized concepts of need and equality may raise a few eyebrows, but it is the centrality of desert that makes his theory distinctive. Having considered and dismissed MacIntyre's account of desert, Miller proceeds to his own. A desert judgment is a claim that it is fitting or appropriate for an agent to have some benefit on the basis of some voluntary and intended performance. Of course, such an account can be understood in an institutional (or entitlement) sense. Given the way races are run, the person who first crosses the line deserves—is entitled to—the gold medal. But Miller wishes to resuscitate a more basic preinstitutional sense of desert, one that can be used critically to assess institutions. Good institutions are those in which, ceteris paribus, those who deserve something get it. Many philosophers find this idea quite mysterious, but Miller insists that those less troubled by abstract thought have no such qualms.

Yet, restoring the central place of desert—at least in instrumental associations—requires that a number of questions be considered. Even ordinary language, and people, discriminate between what is done voluntarily and intentionally and what is owing to bad luck or is otherwise beyond the agent's responsibility. As noted above, people distinguish between genuine needs and mere preferences. Here, the technique of looking to ordinary usage is not much help. These are difficult questions, and people's beliefs (including, of course, the beliefs of many philosophers) are inconsistent. Once these problems have been overcome, the primitive notion of desert needs to be unpacked. What is the measure of "meritoriousness" to be? What makes some response fitting or appropriate, and how do we know it to be so?

The major drawback of Miller's book lies in the failure to address adequately the first set of questions. The theory requires an account of responsibility and a much more compelling discussion of luck. What makes this lacuna extraordinary is that so much contemporary literature (in the "equality of what?" debate and beyond) revolves around these questions.

The same problem does not arise for the second set of questions, which get full consideration in chapter 9 (a revised version of Miller's "Two Cheers for Meritocracy," Journal of Political Philosophy 4 [1996]: 277-301). The relevant measure of merit, given that what is at stake is the principle of social

justice associated with instrumental associations, is productive contribution as an indicator of "social value," and it is to be measured by the value that it would attract in a wellregulated market. Miller's defense of market valuations is careful and sophisticated, but it is (as he admits) a defense of the market as a measure of "economic contribution" (p. 185, emphasis in original). Even if this is accepted, what is not clear is why economic contribution is the relevant measure of merit, even in that part of society best characterized as an "instrumental association." Miller relies on there being discrete modes of association with their own "mode[s] of recompense" (p. 200), but he has no argument in defense of this (any such argument would have attached to it the danger of slipping back into a Walzer-like focus on goods). In short, the problem is revealed in Miller's use, seemingly interchangeably, of "merit," "social contribution," "social value," and "economic contribution." Not only is the market a poor measure of most of these, but also we need to know, and have defended, what is relevant to the principle of desert and what response is fitting to each.

In focusing on desert, I have taken what I think to be most distinctive about Miller's book. The working out of the principles of need and of equality is done well and advances the cause of those who think principles of justice plural and their content deeply contextual. It is in the nature of such approaches that more questions are raised than resolved concerning the relationship of the principles in real situations in which modes of association are not clearly distinguishable.

Is Multiculturalism Bad for Women? Edited by Susan Moller Okin. Princeton, NJ: Princeton University Press, 1999. 146p. \$29.95 cloth, \$12.95 paper.

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Many students in required cultural diversity courses enter (or leave) with what Uma Narayan (Dislocating Cultures, 1997) has called "the anthropological perspective"—the idea that those in the Western world should be interested in and knowledgeable about other cultures but avoid any moral criticism of them. This refusal to judge is a healthy corrective to racist or imperialist assumptions about better and worse cultural traditions, but it is morally problematic. In this book, Susan Moller Okin insists that we judge, and that we judge according to the effect of a cultural practice on women.

This slim volume with the provocative title is composed of a short essay by Okin, no less than 15 responses to it, and Okin's concluding reply to her interlocutors. The book addresses a specific manifestation of "multiculturalism": the claims made for special group rights of minority cultures in liberal societies. Okin's primary point is that, in the vast majority of cases (language rights are the obvious exception), protecting the minority culture's right to certain practices means refusing to protect women in that culture. The list of practices includes clitoridectomy, child marriage, coerced marriage, polygamy, asymmetrical divorce laws and property laws, responses to rape, veiling, and murder. In particular, Okin cites recent legal cases in which minority males relied on "the cultural defense" to explain criminal violence against "their" women—such as "kidnap and rape by Hmong men who claim that their actions are part of their cultural practice of zij poj naim, or marriage by capture, and wife-murder by immigrants from Asian and Middle Eastern countries whose wives have either committed adultery or treated their husbands in a servile way" (p. 18).

The liberal defense of minority group rights (most cogently articulated by Will Kymlicka, a respondent in this volume) is

that these cultures should be protected in liberal societies because a culture is necessary to provide individuals with both a sense of self and the capacity to live a chosen life. Okin contends that if these theorists would consciously attend to the private sphere, to familial and sexual arrangements, they would recognize that many practices of minority cultures undermine this possibility for women; in fact, "most cultures have as one of their principal aims the *control* of women by men" (p. 13, emphasis added).

Okin charges that liberal proponents of group rights for minority cultures ignore a crucial point: These are not monolithic or homogeneous groups. They are, rather, characterized by internal difference, and a primary axis of difference is gender. Because most cultures are not sexually egalitarian, this gender difference is in fact gender hierarchy. This in turn affects what we "know" about a culture's practices and beliefs—"the more powerful, male members are those who are generally in a position to determine and articulate the group's beliefs, practices, and interests" (p. 12). Yael Tamir makes a strong supporting argument that, as a consequence, "group rights strengthen dominant subgroups within each culture and privilege conservative interpretations of culture over reformative and innovative ones" (p. 47).

Okin is certainly vulnerable to the charge that she makes very broad claims and blurs together a variety of diverse practices. Some respondents (Nussbaum, al-Hibri) react to her claims about the patriarchal nature of the world's major religions precisely by noting the existence of "reformative" feminist interpretations of these religions. Others question Okin's implication that there is a simple opposition between men's and women's perspectives by pointing out empowering interpretations by women of such practices as veiling and polygamy (Honig, Parekh, Bhabha). Taken together, these responses provide a more subtle specification of the question at hand. If we take seriously Tamir's and Okin's point that the interpretation of "dominant subgroups" should not be the privileged account, what interpretation should be? In other words, whose representation of a culture and its practices is to be taken as authoritative? Both supporters and critics of Okin imply that it is women's perspectives that must be taken seriously. Yet, these writers also make clear that the contestation within a culture about its gendered "traditions" and practices includes contestation among and between women. How to deal with this—how it should matter for the judgment of minority group rights in liberal societies—is a crucial subtext of this volume that is not fully confronted. Does it undermine Okin's case that many women in the relevant cultures support and in fact perform clitoridectomy? Does it undermine her interlocutors' case that many women do not?

It makes sense that one would look to "what women say" in particular cultures about judging gendered cultural practices. It makes sense because learning how a practice functions or what it signifies means looking at what practitioners say about what it means to them. Okin's respondents have aided in showing how much hard work is necessary in order to know something about a culture, and how much that work requires conscientious dialogue within and between cultures. As trite as it may sound, this may be at least a process of judging that is compatible with multicultural respect. But "what women say" can never be a final easy ground for judgment, because women say different things. We cannot dismiss this difference by attributing false consciousness to women who support what seem to us oppressive practices. But neither are we bound to treat all perspectives as beyond criticism as Okin says in her reply, "one need not rely on the Marxist theory of false consciousness to recognize that persons subjected to unjust conditions often adapt their

preferences so as to conceal the injustice of their situations from themselves" (p. 126).

In such a situation, what should guide judgment? Are there principles by which these judgments should be made, and are such principles flexible enough to attend to context and variation? Sunstein and Nussbaum begin to address this issue in terms of judicial principles; Okin relies on liberal principles of human dignity and equally freely chosen lives. But, as various interlocutors point out, Western liberal societies themselves are not models of gender equality, and their practices deserve critical attention, too, a point with which Okin is in fervent agreement. Such critical attention can serve to remind us (as Parekh does) that there are conflicting interpretations of Western liberal principles.

My complaint about this volume is the format (undoubtedly a residual effect of original publication in the Boston Review). Because of the number and length of responses, each author makes only a series of abbreviated points, without fleshing out the arguments or going deeply into particular examples. (To be frank, some of the responses do not add up to much.) This is one concern I have about using the book as a teaching text; it is not a very good model of argument or of what it means to "know" something about other cultures. Despite that concern, I can imagine the volume being useful in courses on democratic theory, liberalism, feminist theory, or ethnic politics. I would suggest using selections from this book in conjunction with an essay that more closely examines a particular example of "the politics of traditional formation" (e.g., a chapter from the Narayan book cited above).

As Katha Pollitt asks in her contribution (p. 28): "What is a culture, and how do you know?" This book keeps that question before our eyes, and thus makes it impossible for us to avoid critical moral engagement with issues of multiculturalism.

The American Language of Rights. By Richard A. Primus. Cambridge and New York: Cambridge University Press, 1999. 262p. \$54.95.

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The postphilosophical, pragmatist point of departure for this wonderful first book from Richard A. Primus is that we should talk less about the abstract or ontological properties of rights (which, frankly speaking, do not exist) and more about the political dynamics of rights discourse as a distinctive social practice.

Primus begins with a critique of scholars who are preoccupied with elaborating formal definitions of rights and with policing the boundaries of the category "rights." Instead, scholars should think of rights as a kind of Wittgensteinian "language-game," in which the meaning (and reality) of the concept is induced from usage rather than deduced from proffered analytic categories. Such an approach makes room for the ways in which conceptions of rights have changed over time and for the fact that different users adopt diverse (and potentially contradictory) understandings of the concept. It also means that scholars interested in examining rights as they exist in practice "should seek to encompass as many instances of seeming rights discussion as can be accommodated before coherence breaks down" (p. 33), rather than limit inquiry to the technical boundaries set out in many formal treatments. He does not share (for example) Hohfeld's concern with distinguishing assertions of privileges from assertions of rights, since Primus properly notes that these philosophical preoccupations do not do justice to the

various ways in which the concept is used by actual historical agents.

What does fall within the language of rights? Primus argues that in the American case "people use rights claims (1) to claim general authority for specific propositions, (2) to attempt to entrench politically precarious practices, and (3) to declare particular practices or propositions to be of special importance" (p. 39). In a separate chapter on the development of rights in American history, Primus suggests that three elements have shaped transformations or developments in the social practice of rights discourse. First, a condition of "adversity" persuades people that prevailing political arrangements are unacceptable. Second, a "reaction" takes place in which people draw from a previously existing culture of rights to articulate a new rights agenda that is designed to correct the adverse conditions. Third, a "synthesis" occurs that attempts to reconcile the most recent layer of rights claims with previous traditions. Primus's effort to generalize about stages of development brings to mind the work of historically minded constitutional theorists, such as Akhil Amar and Bruce Ackerman. Primus distinguishes his contribution, however, by criticizing Amar for ignoring twentieth-century developments in rights discourse and Ackerman for paying too little attention to the American reaction to Nazism and totalitarianism after World War II.

Primus fleshes out his claims about adversity, reaction, and synthesis in three empirical chapters. The first focuses on the founding period and emphasizes how the use of rights discourses violated many of the tenets of modern analytic rights theory. For example, that generation "believed individuals, institutions, and communities all to have the capacity for possessing rights," and it "felt that rights came from many different sources, human, natural, and divine" (pp. 88-9). What tied together these diverse arguments was a shared interest in mitigating the specific adversities suffered as a result of colonial practices (relating mostly to armies, trials, religion, and trade) and entrenching new policies designed to prevent reoccurrence. Similarly, in the chapter on Reconstruction, Primus interprets postwar developments not as an effort to cleanse a set of underlying founding principles but as a reaction by northern whites to specific threats associated with the antebellum politics of "slave power." Primus also shows that as Reconstruction progressed, whites created and manipulated distinctions, such as "civil v. political v. social rights," in order to privilege the policies they cared most about while simultaneously rejecting broader claims premised on a more thoroughgoing egalitarian vision.

In the third empirical chapter, Primus emphasizes the ways in which the American experience with Nazism and totalitarianism facilitated another reconfiguration. Unlike the social welfare agenda articulated in FDR's "second bill of rights" (which never gained a foothold in constitutional law), the leading themes of the antitotalitarian experience "included personal privacy, police procedures, free expression, and protection against discrimination" (p. 181). It also included a new language of "universal human rights," which was both substantively different and more inclusive than the founders' notion of natural rights. These themes became central (albeit contested) features of the Supreme Court's jurisprudence in the decades after World War II and laid the groundwork for the kind of nonpositivist privacy jurisprudence that emerged in the 1960s. They were also constitutive of most modern academic theorizing about rights, most notably Ronald Dworkin's conception, which Primus respects more as a stylized articulation of a historically contingent political agenda than as an abstract philosophical statement.

The scope of this work makes it inevitable that Primus will

stake out some positions that will be considered insufficiently justified. He claims that his conception of rights is not simple nominalism because an assertion of a right is only properly considered a right if it "obeys the rules of the governing practice" (p. 238). Yet, he also knows that on his account the 'practice" is not "governed" by "rules" as such, and when he attempts to referee the practice with reference to his "entitlements-liberties-powers-immunities rule" and his "important-and-should-be-protected rule" (p. 238) he hovers a bit too close to the analytic tradition he is quick to criticize. It might have been more consistent to keep the focus on the prevailing assumptions within the relevant interpretive communities. Moreover, Primus's reasonable decision to take rights claims at face value (more or less) results in a discussion that runs together assertions that appear purely cynical and manipulative (such as the "shell game" of Reconstruction rights for blacks) and those that appear to be relatively autonomous from conventional partisan agendas (such as the enhanced commitment to privacy in the shadow of totalitarianism). If rights discourse is to be respected as a distinctive and important social practice, it may be necessary to distinguish its purely instrumental deployment from more sincere or principled uses, even though such an effort would seriously complicate the hermeneutical methods at the heart of this project.

Finally, some may complain that Primus's periodization inevitably leads to a review of only a narrow array of rights discourses—namely, elite claims on issues of great importance during political crises—and (in Ackermanian fashion) to focus too much on large-scale change rather than everyday, uneven evolution in different corners of the language game. But Primus understands these limits, and he is correct to respond that "showing a new way to read well-known sources might do more to make us rethink our understanding of rights discourse than a reading of less-familiar sources would" (p. 74). For those interested in the theory and practice of rights in the American tradition—whether in political philosophy, constitutional law, elite politics, or social movements-Primus successfully opens some important new theoretical debates as well as some promising new lines of empirical inquiry. The result is a thoughtful, well-researched, and provocative contribution to our understanding of liberalism, constitutional theory, and American political development, and it is highly recommended.

Carl Schmitt: The End of Law. By William E. Scheuerman. Lanham, MD: Roman & Littlefield, 1999. 360p. \$70.00 cloth, \$22.95 paper.

Ellen Kennedy, University of Pennsylvania

When Carl Schmitt died in April 1985, only The Concept of the Political was available in English, and few in America or Britain knew his work. Since then, a significant body of scholarship on Schmitt has appeared in this country, and numerous texts by him are available in English. But why we should read Schmitt and how we should read him remain in dispute. William E. Scheuerman tries to answer both questions. The introduction provides a brief overview of Schmitt's life and work and wastes no time in getting to the reason he is and will remain controversial. "Too many scholars continue to miss the central place of Schmitt's ideas about law in his intellectual agenda" (p. 2), and it was those ideas about law that led Schmitt into collaboration with Hitler. Since Schmitt's critique of liberal jurisprudence is not limited to Weimar, demonstrating what is so dangerous about it is more than an archaeological project.

Carl Schmitt: The End of Law is two books in one. Part 1 is provocatively titled "The Jurisprudence of Lawlessness." Schmitt's ideas are unwittingly being continued by political scientists and critics of liberal legal theory. Together they represent "the subterranean influence on postwar American political thought" of this most un-American of all thinkers, whose ghost, like the Marxian specter of 1848, "haunts political and legal debates not just in Europe, but also in the contemporary United States" (p. 1). "Mainstream" modern legal thought is liberal. It requires that legal norms be (1) general in character, (2) relatively clear, (3) public, (4) proscriptive, and (5) stable. This theory of the rule of law, Scheuerman argues, "renders state action predictable and makes an indispensable contribution toward individual freedom." It is the foundation of constitutional government that binds officials to the law and minimizes "the potentially undesirable exercise of discretion." A look at regimes where these are not established, he contends, shows "the strength of the liberal argument" (pp. 4-5).

Yet, Scheuerman acknowledges, there are troubling signs that contemporary practices-"necessary forms of state intervention in the capitalist economy" and "the proliferation of powerful constitutional courts" (p. 5)—are undermining the rule of law. The first creates a powerful state active in areas regarded by liberal political theory as outside its sphere. The second may not appear inconsistent with the rule of law, but it "probably requires that constitutional courts engage in relatively limited forms of judicial review," which "for better or worse" they have not always done. These developments signal the very problem liberal theories were intended to prevent: the problem of legal indeterminacy. Scheuerman's account of this is necessarily brief, and it is not really his target. Rather, this book is about the responses to legal indeterminacy, all of which he seems to think are more or less inconsistent with the idea of the rule of law. H.L.A. Hart represents "limiting indeterminacy," Richard Posner and the Realists stand for "indeterminacy," and CLS (Critical Legal Studies) lawyers such as Duncan Kennedy are "racial indeterminants." Although forceful about the dangers of such positions, Scheuerman offers an inconsistent account of how Schmitt fits into these debates.

The strongest claim would be that Schmitt's work influenced American legal scholars, political scientists, and intellectuals (such as Stanley Fish, whom Scheuerman calls a "nihilist," p. 37). Part 2, "Carl Schmitt in America," might be described as "outing" some very high-profile figures in political science and economics as closet "Schmittians." Unlike the legal theorists discussed in Part 1, the men discussed in Part 2 knew Schmitt personally and were influenced by his work. These chapters have otherwise little interconnection. They try to do for Joseph Schumpeter, Friedrich Hayek, and Hans Morganthau what Heinrich Meier did for Leo Strauss (Carl Schmitt and Leo Strauss: The Hidden Dialogue, 1995). Meier's analysis of the intellectual relationship between Strauss and Schmitt was a tour de force of interpretative history. Scheuerman's discussion in Part 2 is something less.

Will empirical political scientists working on democracy and electoral politics be surprised to find themselves aligned with Schmitt via Schumpeter? Perhaps, but Carole Pateman made the point about the elitist bias of American political science in *Participation and Democratic Theory* (1970). Unlike her, Scheuerman seems to regard this as an object less to be explained than merely condemned. "In what surely belongs among the great intellectual paradoxes of our time," he writes, "many American political scientists, in the immediate aftermath of the victory over National Socialism in 1945, embraced a tradition of political thought that was complicit

in the antidemocratic sins of twentieth century European political theory and practice" (p. 207). "The unholy alliance of Carl Schmitt and Friedrich Hayek" (chap. 8) links "authoritarianism and capitalism" through Schmitt's position on private property in a minor text entitled Equality before the Law (1926) and Hayek's The Road to Serfdom (1944). This explains "the elective affinity between free-market economics and authoritarian politics that has become so common in the contemporary political universe" (p. 224). Most of what the author says about Hans Morgenthau's relationship to Schmitt comes from Morganthau's An Intellectual Autobiography (1978), but Scheuerman does offer an interesting account of how assumptions about "the autonomy of the political," or "pure politics," work within the arguments of the two men. None of these chapters pursues the questions of intellectual traditions or influence effectively and probably should have been published elsewhere, either as separate articles or another book with a broader theme. Their final effect is defamatory and suggests more than it proves. Schumpeter's theory, Scheuerman writes, "is hardly fascistic. Yet Schumpeter may be only a few steps away from Schmitt's path" (p.

Similar rhetorical turns characterize the first part of Carl Schmitt: The End of Law, which distract the reader from some of the most interesting aspects of Scheuerman's analysis. He is right to see legal indeterminacy as central to Schmitt's political thought, and he traces it through Schmitt's major works on political institutions. The argument is persuasive and important that Schmitt was the first to recognize the importance of legal indeterminacy, not just for the logic of law but for a political science of the state. In two early works (Law and Judgement: An Examination of the Problem of Legal Practice, 1912, and The Value of the State and the Significance of the Individual, 1914), Scheuerman writes, Schmitt "struggles to overcome the antinomy of law and power" (p. 25). Unlike the young Hans Kelsen, who separated these domains conceptually, Schmitt focused on the links between them. In these works, as in those of the Weimar period and later, the originality of Schmitt's approach often reshaped the stock questions of continental jurisprudence. The topic of what is "right law," the subject of so many jurisprudential tomes, was conceived in purely formal terms by the nineteenth-century lawyers. Schmitt restates that as "how do we know when a judge's decision is right?" and modernizes it by applying Weber's notion of the ideal-type. We know that a judge's decision is right when another judge would have made the same decision, if, as Schmitt explains, "another judge is the empirical type of the expertly trained jurist" (Law and Judgement, quoted on p. 23). At stake is the traditional liberal assumption that, as for the rule of law not men, there must be a single determinate answer to any legal question. Otherwise, the rule of law would be replaced by its opposite, will and discretion.

The chapters that follow link the major themes of Schmitt's work to recent debates in political science. In chapter 2, "The Decay of Parliamentarism," for example, Scheuerman makes the connection between the current discussion of civil society and Schmitt's powerful analysis in *The Crisis of Parliamentary Democracy* (1923) and *Constitutional Theory* (1928) of how special interests working through political parties transform the theory and practice of representative government. "The Critique of Liberal Constitutionalism" (chap. 3) reads Schmitt in the context of problems fashionable among post-modern political theorists, such as Jacques Derrida, and legal scholars like Robert Clover: How should "normative" constitutional theories be understood in light of the "violent willfulness" that usually generates them? "The original sin of

foundational violence means that the constitutionalist dream of 'government of law, not men' always suffers from a fundamental hypocrisy, it obscures the arbitrariness that haunts even 'normal' political experience" (p. 82). Like Schmitt, Derrida et al. are drawn to "limit situations," an approach Scheuerman seems to think must lead to fascism.

approach Scheuerman seems to think must lead to fascism. In the "governability" debate of the mid-1970s (Michael Crozier, Samuel P. Huntington, and Joji Watanuki, The Crisis of Democracy: Report on the Governability of Democracies to the Trilateral Commission, 1975), Scheuerman hears the echo of Schmitt's analysis of the destructive effects of pluralism and polyarchy during Weimar's final presidentialist phase, from 1930 to Hitler's appointment in January 1933. The Defender of the Constitution (1931) was the Schmitt work most admired by Neumann and Kirchheimer because of its brilliant analysis of "adversarial pluralism" and the erosion of parliamentary politics through the dependence of political parties on what we call "soft money" today.

But readers unfamiliar with those German debates will be misled by Scheuerman's less than careful distinction between what Schmitt (and others in the early 1930s) meant by "the total state," as distinct from "the totalitarian state" under National Socialism. The total state referred to a system we all more or less accept today, one in which governments take responsibility for steering the economy toward prosperity and provide a security net when that fails. In the process, governments intrude into sectors that classical liberalism regarded as "private." Whether this is desirable or not, it is not a liberal theory of government, and Schmitt consistently stressed the consequences of such developments on law and the state.

To demonstrate the centrality of Schmitt's political theory to the questions of government in the twentieth century, as Scheuerman does, is a significant achievement. Yet, this is an oddly unsure book. Schmitt's work is important and should be read—but its conclusions are "shocking" (p. 84). Schmitt "tackles the hard questions often ignored by contemporary radical jurists" (p. 134), and he "would have agreed with the basic outlines of (Dworkin's) diagnosis" of legal indeterminacy, but his answers led to Nazi terror (p. 139). When Scheuerman asserts that "Schmitt speaks to important questions within liberal theory" but "lacks the conceptual instru-ments for analyzing these questions adequately" (p. 74), the confusion is complete. Its source lies in a fault Scheuerman rightly discovers in Schmitt's critique of Hans Kelsen: the backward reading of a text to reveal a telos (p. 78). Just as Schmitt sees Kelsen's liberalism as implicit in all previous versions, so, too, Scheuerman reads the worst of Schmitt's arguments (in such notorious texts as "The Führer Defends the Law," Deutsche Juristenzeitung 39 [1934]: 945-50) as implicit throughout. This also explains the frequently maudlin outbursts in this book, in which the impulse to warn seems to overtake the work at hand. If fascism and its horrors were always implied, then the reading of Schmitt must be taken as a kind of vaccine against repeating the errors and horrors of the century just gone.

Why should we read Carl Schmitt? How should we read him? This book answers neither question.

Modesty and Arrogance in Judgment: Hannah Arendt's Eichmann in Jerusalem. By Barry Sharpe. Westport, CT: Praeger, 1999. 192p. \$55.00.

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The unique qualities of the human faculty for judgment must be recognized in any explanation of political phenomena that seeks to stay true to the phenomenology of actual lived experience of political decision making and action. Judgment is, moreover, implicit in the various vocabularies by which we assign praise and opprobrium to political actions and agents. Morality and ethics—even the very notion of personal responsibility—verge toward meaninglessness without at least the tacit supposition of a widely shared ability to judge right from wrong. Attributions of beauty, quality, and greatness are merely emotive expressions without a presumed ability to judge that is both based on community standards and somehow able to transcend them.

Judgment is thus central to the ways we speak and act every day. Yet, judgment is one of those ineffable human capacities that routinely frustrate our attempts to grasp them through theoretical analysis or empirical observation. As a result, political judgment is not written about very much, at least not directly. It is much easier to avoid wooly arguments by reducing judgment to a matter of choosing the means most likely to satisfy some constellation of desires, preferences, or interests. These desires, preferences, and interests are then treated as prior to rational decision making and choice, as if they were not also the products of various decisions and choices. Judgment becomes simply a matter of rule-following, the correct application of a general principle (e.g., utility maximization) to a particular case. But judgment qua subsumption of particular case to general rule always begs the question of which rule to follow (e.g., is it really appropriate for me to be thinking of maximizing my utility in thus situation), especially in situations in which agents find themselves faced with decisions involving multiple competing interests and obligations and also when there simply does not seem to be a general rule applicable to the particular facts at hand. Such general rules turn out to be like Wittgenstein's famous signs: At best, they provide some idea of what can be expected in a certain direction; they are not able to tell us which way we ought to go. The latter is literally a matter of judgment.

The most serious theoretical considerations of the human capacity for judgment are found in Aristotle and in the German philosophical tradition. For contemporary political theorists, the leading inspiration has tended to be the controversial and unfinished work of Hannah Arendt. Two substantial books (Ronald Beiner, Political Judgment, 1983; Peter Steinberger, The Concept of Political Judgment, 1993) and a host of articles have sought to build on the foundations laid by her, but it seems fair to say that after Arendt our progress in understanding judgment has been mixed and sporadic. When a new theoretical analysis of political judgment comes out, however, hope seems to spring eternal. When the analysis is based on a shrewd and novel insight, as is Barry Sharpe's Modesty and Arrogance in Judgment, there is all the more reason to expect an advance in our understanding of this clusive, baffling, and utterly fundamental capacity.

Sharpe's strategy is to interrogate and explicate Arendt's apparent theory of political judgment by means of the judgments Arendt made herself in reporting on the trial of Adolf Eichmann, the SS officer who supervised the logistics of the Holocaust. Eichmann's insistence that he was not qualified to pass judgment on the orders and purposes of the Nazi leadership illustrates what might be called excessive modesty in judgment. Eichmann's "Who was I to judge?" is an implicit claim that he did not make a moral choice to participate in the Final Solution and bore no responsibility for the consequences of his actions. No doubt Eichmann did feel this modesty—although new light may be shed on this issue by the recent release of Eichmann's copious apologia pro vita sua, written while incarcerated in Israel-and most people can feel at least some empathy for the difficulty of his position. But this modesty, if allowed to stand as an excuse for not judging, leads directly to the complete breakdown of the notion of personal responsibility for one's actions and of the moral and legal systems based on this notion.

But if Eichmann must be held accountable for judgments he made (or neglected to make) in a difficult situation, consistency and fairness prompted Arendt (never overly modest herself) to require that others also be held accountable for their choices in situations even more difficult. For this reason she pointed an accusatory finger at the Judenrate, the Jewish leadership councils that cooperated with the Nazis by compiling lists of people, addresses, and property, by administering the ghettos, and even by helping decide who would die and when. Arendt's presumption to judge the Jewish leadership caused perhaps even more controversy than her description of the banality of evil exemplified by Eichmann. This presumption to be able to judge others acting in situations that one never directly experienced oneself typifies and crystallizes what Sharpe calls the inherent "arrogance" of judgment.

These concepts of modesty and arrogance in judgment provide the structure for what Sharpe says (pp. xiv-xv) will be an analysis of ways in which distance—in space, time, and imagination—serves to facilitate and obstruct individual judgment. But what could have been a very interesting and

enlightening analysis never quite comes to fruition. Earlier treatments of judgment are far more subtle, nuanced, and sustained, and one can learn a great deal more about judgment by reading Arendt directly than one can glean from most of Sharpe's commentary. Readers umfamiliar with Arendt will find in this commentary an admirable and useful introduction to the powerful argument she makes in Eichmann in Jerusalem and to the controversy still surrounding this argument. Sharpe also demonstrates some of the linkages between the Eichmann arguments and other important aspects of Arendt's work, including the analyses of totalitarianism and political judgment. He updates and clarifies some of Arendt's ideas in view of more recent scholarship on the Nazis and the Holocaust. He has some very interesting and important things to say about the role and responsibility of the Judenrate. Given the nature of the subject matter, Sharpe's writing is generally quite lucid, although a number of gaffes should have been caught by a careful editor. But there is little in the analysis of political judgment that adds to what Beiner, Steinberger, Benhabib, and a few others have already said. In short, this book does not quite measure up to the lofty goals it sets for itself but certainly may be useful for other purposes.

American Politics

Reconstructing Reconstruction: The Supreme Court and the Production of Historical Truth. By Pamela Brandwein. Durham and London: Duke University Press, 1999. 272p. \$49.95 cloth, \$17.95 paper.

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Because this book is an outgrowth of a doctoral dissertation, it should have extensive documentation in the form of endnotes, a bibliography that is inclusive and up-to-date, and a thesis that is pursued throughout, all of which it has. The thesis is that a particular perspective on Reconstruction was adopted by the Supreme Court, a perspective that then took on a life of its own as the prevailing view in constitutional law debates. The author combines an historical approach with theory, which is appropriate in that she is attempting to develop a sociology of constitutional law. Of course, for readers who prefer facts, the many pages devoted to theory can become tedious. In addition, Brandwein is a sociologist, not a political scientist, so much of her material is presented from a perspective not very familiar to many in the constitutional law field. Those who have read widely in sociological jurisprudence, however, will find most of what she writes quite readable, because thankfully the book is not loaded with jargon from sociology.

The introduction is very extensive, more so than in most books, but useful in setting up the theoretical framework. Brandwein devotes a good first portion of the book to contrasting the view of slavery of northern Democrats in the Reconstruction Congress with the Republican view. The Northern Democrats opted for popular sovereignty. Once slavery was abolished by the Thirteenth Amendment, they considered the rebellion finished, as that was what the Civil War was all about. Local majorities were then free to control individual rights, the only exception being that formal slave law could not be instituted. Brandwein asserts, however, that the northern Democrats had "a strong strain of white supremacy" (p. 41). Republicans, in contrast, felt that slavery

was still a problem even after the surrender, due to the southern Black Codes, which is why they pushed for the Fourteenth Amendment.

A main theme of the book is how the Supreme Court, in the Slaughter-House cases, adopted the northern Democratic view. Justice Samuel Miller, in a five-four decision, held that the bulk of our privileges come from our state citizenship, not our U.S. citizenship; since the Fourteenth Amendment protects the latter, it is inapplicable in the case (concerning a monopoly and the right to work). "Miller suppressed views of the dangers of slavery that were formed by Republicans in the decades before secession" (p. 65). Despite the dissenters' arguments, the majority version prevailed over the years, weighing "state jurisdiction more heavily than the democratic participation of black citizens" (p. 81). The Court's subsequent decisions made Miller's view of the Fourteenth Amendment "definitive" (p. 94).

The author examines in depth the debate between Charles Fairman and William Crosskey concerning the original understanding of the Fourteenth Amendment. The debate arose when Justice Hugo Black wrote a dissent in a 1947 case, Adamson v. California, arguing that the Fourteenth Amendment incorporated the Bill of Rights against the states. Fairman disputed that point, which Crosskey then defended. Fairman disputed Crosskey, and the battle was joined. Most critics supported Fairman, and "what lawyers and judges received in Fairman's account was . . . an affirmation of state control over citizenship rights and a stabilization of the doctrine of stare decisis regarding Fourteenth Amendment history" (p. 134). This version has become so institutionalized that any attempts to deviate from it run into trouble, even though most of the Bill of Rights has eventually been incorporated.

To illustrate the last point, the author looks at the Warren court debates over legislative apportionment, as exemplified in Baker, Wesberry, and Reynolds, and she concludes that the majority "justices attempted to develop nationalist solutions to citizenship dilemmas without challenging the institutional account of Reconstruction" (p. 184). Many in the legal

community were unpersuaded by those arguments. The author contends that "understanding how stores of symbolic power are accumulated, maintained, and lost is one of the basic objectives of a sociology of constitutional law" (p. 207).

In sum, Brandwein does a good job of showing that an understanding of how the ideas of majorities in passing legislation can be subordinated by the Supreme Court in favor of the ideas of political minorities "is critical to our understanding of American political structures and the nature of the challenges that face American democracy" (p. 214). The book definitely contributes to scholarship in this area; in fact, it breaks new ground and opens the way for future scholarship.

Race, Redistricting, and Representation: The Unintended Consequences of Black Majority Districts. By David T. Canon. Chicago: University of Chicago Press, 1999. 324p. \$50.00 cloth, \$18.00 paper.

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It is sometimes striking what small use the current Supreme Court has for that which we, as political scientists, regard as empirical evidence. Justice O'Connor, speaking for the Court, showed no inclination to seek out or examine such evidence when, in Shaw v. Reno (1993), she remarked that racially conscious redistricting lines "bear an uncomfortable resemblance to political apartheid" and asserted that officials elected from such majority-minority districts are more likely to represent exclusively members of that minority group. Canon's carefully researched and thoughtfully argued book provides a refreshingly empirical response to the Court's simple assertion.

In essence, Canon argues that black members of Congress can represent white constituents well. The electoral dynamics of majority-black districts often, in fact, causes black representatives to adopt the "politics of commonality" and to draw on a biracial voter base (p. 243). This most often occurs when several strong black candidates run against each other in the Democratic primary, splitting the black vote, and then must make successful appeals to the interests of white voters in order to win the nomination.

Canon begins his argument with an inquiry into the differences between the policy preferences of blacks and whites. Relying primarily on a comparison of data from the 1992 and 1994 National Election Survey, he finds that blacks "have the same issue preferences as whites on nonracial issues" (p. 30). This implies that previous studies that use standard voting scores to gauge the responsiveness of representatives to minority constituents need to be reexamined (e.g., David Lublin, *The Paradox of Representation*, 1997, and Charles Cameron, David Epstein, and Sharyn O'Halloran, "Do Majority-Minority Districts Maximize Substantive Black Representation in Congress?" *American Political Science Review* 90 [December 1996]: 794-812).

Canon's analysis is suggestive, but it leaves the reader uncertain of the importance of the problem. Can the same pattern be confirmed in other surveys and for other times? How large is the difference between, and confidence intervals for, the corrected and uncorrected scores?

Canon continues by examining (with Mathew M. Schousen and Patrick J. Sellers) the "process of candidate emergence" (p. 93) in black districts. He starts with a detailed study of the redistricting process in North Carolina, which he uses to develop a "supply-side" hypothesis. He uses the data to predict successfully most of the contests in seventeen majority-black districts in 1992. The main finding is that the

characteristics of each district determine the racial makeup of the candidate pool in the Democratic primary, which in turn determines the coalition of voters a candidate must attract in order to win. In particular, a large black majority increases the likelihood that several ambitious black candidates will compete. As long as no white candidate enters the race, the resulting split among black voters creates the opportunity for a "new style" of black candidate. This type of candidate practices a politics of commonality and attracts the additional white vote necessary to defeat the "traditional" black candidates, who practice the "politics of difference."

Canon relies on unique data, that is, the political bases of support for and the policy positions of candidates running in majority-minority districts. It is puzzling that he is not more clear about how the data were collected and coded. He states only that he "examined" the policy positions and racial coalitions of these candidates, using data from interviews, newspapers, and public records (pp. 137-8, 265). Much of this information and the data used later in the book were collected from interviews, personal meetings, and constituency mailings and are not reproducible. Canon has opened up new lines of inquiry, but he misses an opportunity to have his research more directly extended by failing to provide replication data. (See Gary King, "Replication, Replication," PS: Political Science & Politics 28 [September 1995]: 443-499, for a description of the replication standard). One hopes that the author will soon place these data in a public archive.

Furthermore, 1992 was not a typical year for majority-minority districts (see David Lublin, "Racial Redistricting and African-American Representation," American Political Science Review 93 [March 1999]: 183-6), and one wonders whether the dynamics that Canon observes apply to other years. Moreover, Canon's supply-side explanation of representational style leaves vital questions unanswered. Is the "politics of commonality" durable—did the new-style candidates remain successful in future elections, and did they continue to maintain their biracial political style and base? Why did so few white candidates compete? Are white Democrats less likely to run in these districts if other white Democratic districts exist? If so, what are the extended supply-side repercussions of making other Democratic seats less safe to create majority-black districts?

After testing the supply-side theory, Canon draws upon a variety of sources—including roll-call votes; an extensive collection and content analysis of congressional newsletters, flyers, and press releases; and an analysis of news coverage—to show that "commonality" representatives are more moderate on racial issues than "difference" representatives. This is, arguably, the book's most significant contribution to the discipline. Canon's analysis of the behaviors of "difference" and "commonality" representatives is, however, mildly marred by a lack of clarity in testing his hypothesis.

The most rigorous tests are six regressions (Tables 4.9, 4.10, 4.14, 4.17, 4.18, and 5.8), in which the independent variable is (typically) a measure of moderateness (e.g., a Leadership Conference on Civil Rights score), and the representational style of a member is represented by dummy variables. Canon discusses whether the individual coefficients were significant and whether members practicing the commonality style appear to be more moderate than those practicing the difference style. Canon does not, however, test explicitly the inference that these representatives behave differently. (For example, he might have computed the confidence interval for the difference between the coefficients for the two styles. Canon does refer to "significant" differences [pp. 190, 196, 236], but it is not clear to which differences he refers, how they are computed, and what is the

associated confidence level.) Without his data, one can only compute bounds on the possible confidence intervals. These bounds confirm that explicit tests are indeed necessary to determine whether the differences in behaviors were significant. In at least one test (Table 4.9) that otherwise would appear to support Canon's hypothesis, the difference clearly was not significant. (Canon does not note this.) Despite the need for clearer tests, however, the weight of the evidence seems to support Canon's theory.

In conclusion, the findings in Race, Representation and Redistricting are of immediate and lasting importance. The variety of data sources and research methods is impressive, as is the effort that the author invested in data collection and coding. Race, Redistricting and Representation should be required reading for anyone with an interest in the linkage between representatives and their constituencies.

Governing with the News: The News Media as a Political Institution. By Timothy E. Cook. Chicago: University of Chicago Press, 1998. 289p. \$48.00 cloth, 17.95 paper.

Steven Chaffee, University of California, Santa Barbara

There is a ritual quality to academic critiques of the press nowadays. One must see deep, virtually irremediable problems and yet offer solutions. One must know a lot about news processes and yet dismiss as naive journalism's working norm of "objectivity," along with "traditional" empirical social scientific inquiry. One must champion diversity and yet not appear to be offering simply another elite prescription. The task becomes doubly difficult when the object of one's hand-wringing is not only the press but also the government. Within these conventional strictures it is hard to write a useful book, but in this case the author has succeeded.

Cook's study originated in teaching a class on the press and American government, and it might serve as a partial text for such courses. It merits review also as original scholarship and as an extended argument. The argument is that the news media "are not merely part of politics; they are part of government" (p. 3) and have been almost from the beginning of the nation. Moreover, something ought to be done about that, although it is not wholly clear just who should do what.

As a textbook, the volume is well organized and readable for advanced undergraduates; it provides an organizing scheme around which more elaborate course assignments could center. Yet, it omits several important parts of an overall picture of political communication. There is no comparison to other systems, no coverage of the large literature on media effects on political opinions and behavior, and surprisingly little content on levels or branches of government outside the White House. The analysis instead emphasizes the "uniqueness of the president's position visavis the news media" (p. 162), although admittedly "all political actors in Washington" use publicity in their efforts to govern.

The substantive heart of the book is an excellent synthesis of historical research on the press-government relationship. It is a very cozy symbiosis, all told, despite journalists' self-professed independent role as watchdogs. Early in the nation's history, favored partisan newspapers got printing contracts and other forms of sponsorship from public officials who needed to communicate with constituents. The American Revolution was egged on by printers angry over the Stamp Act of 1765, a tax that applied to each newspaper published in the colonies. Subsidies such as cheap postage rates for newspapers, selection of supportive reporters to cover official business, and even an administration newspaper

marked the early nineteenth century. But the goal was sheer communication as much as it was a slanted press. Cook points out that "public policy from the outset of the American republic focused explicitly on getting the news to a wide readership" (p. 44).

The sponsored press evaporated after the Civil War, as paid advertising succeeded public printing as the principal source of newspapers' revenue. The government's continuing need to communicate widely led to a new set of institutions directed to that purpose, mainly the creation of press offices to service reporters and thus use "public funds to help generate the news" (p. 44). Public relations specialists became well know under such press-conscious presidents as William McKinley and Theodore Roosevelt. George Creel, hired to direct World War I propaganda for Woodrow Wilson, summed up the American way then as now when he stated: "Censorship policy must be based on publicity, not suppression" (p. 51).

With regulation of radio and television in the twentieth century came new ways for government to shape public communication in the service of public policy. The long tradition of independent editorial review of governmental action continues, but in most instances American journalism finds American government doing approximately the right thing. Typically, a newspaper or public affairs magazine presents the reasons for a policy much more fully than arguments against it.

The press is aided in this work by a remarkable subsidy: thousands of press officers throughout the corridors of power in Washington, each ready with handouts, briefings, and photo opportunities. Thus assisted in their work, journalists focus mostly on official action at the expense of coverage of the problems that give rise to it. Cook feels that most of the time this practice serves to legitimize what is being done officially. Institutional dependence of the media on the formal government is a far more serious problem for democracy, in his view, than partisan political biases one might discern in news personnel or coverage of a contested issue.

A related problem is limited access to the media for alternative voices. This well-documented fact, Cook asserts, results in a narrowing of news content to commentaries pro and con a current governmental initiative, as if there were no third perspective. In a search for "solutions" to recommend, however, he essentially comes up empty—neither journalism nor the government can be expected to do the job—so he leaves off by urging that whatever is done be geared toward widening people's access to news channels and diversifying content.

Some subsidies are inevitable and should continue, Cook concludes (p. 187), but they should be targeted toward economically marginal news organizations, not a few big media. As new communication technologies develop, they should be somehow "crafted to allow maximum access by as many citizens as possible" (p. 189). And the author hopes that public officials can find ways to communicate without the interference of processing by news professionals, an "unelected" class he does not trust to do their job right. He cites cable television's C-Span as a step in the right direction, although he admits that only a small audience awaits additional public affairs telecasts of extended actualities.

The book, although carefully crafted, is not without internal contradictions. Cook's achievement is to bring together an original analysis of political institutions with accumulated scholarship in the field of mass communication. The author employs particularly well the legal-historical school mentored by H.L. Nelson at the University of Wisconsin circa 1960–90. Cook's careful reading of this body of work, which is largely

quantitative and hypothesis-testing, contrasts with his own firm eschewal of parallel analyses in the behavioral school of political science. His approach is in fact essentially empirical, often building on quantitative evidence accumulated by these assiduous media historians.

In parallel spirit, Cook dismisses the journalist's traditional goal of objective reportage, which shares many normative assumptions with empirical social science. He advocates instead "marshaling evidence" in support of one's arguments, both in journalism and in scholarly analysis. It is not surprising, then, that he approves strongly of the movement among mid-sized newspapers toward "public journalism," which involves local media intimately in community problem-solving. At the same time, he singles out journalists as illegitimate political actors: "Who elected them?" (p. 167), as if all parties to public deliberation require prior public approval.

Cook also mocks the tradition of "linear, unidirectional" effects of news media (pp. 10, 12), but in advocating a widened scope of publicity beyond that of controlling elites he assumes effects more powerful than any empirical scholar of media in politics would claim. In these respects, Cook is no worse than many other critics of press and government, but he is not especially original either. Although he offers some thoughtful hopes for a system of institutions he sees as deeply flawed, he does not get us much closer to realistic solutions. In sum, this book is a valuable contribution to the ongoing press-in-politics dialog, and one can learn a great deal here that does not appear as a coherent package elsewhere. But like other works on the subject, it leaves out much of the story.

Religious Institutious and Minor Parties in the United States. By Christopher P. Gilbert, David A. M. Peterson, Timothy R. Johnson, and Paul A. Djupe. Westport, CT: Praeger, 1999. 192p. \$59.95.

Geoffrey C. Layman, Vanderbilt University

In recent years, the apparent growth of public disenchantment with the major parties, the Perot and Ventura phenomena, and the development of the Reform Party have contributed to an increasing scholarly focus on and understanding of third-party politics. Meanwhile, the field of religion and politics has become a scholarly growth industry, as the Christian Right's influence in the Republican Party has grown, and the religious differences between the two major party coalitions have expanded. Surprisingly, however, the paths of these two literatures have not yet crossed; religion has been noticeably absent in explanations of why Americans do or do not support minor candidates. This is the situation that Gilbert, Peterson, Johnson, and Djupe attempt to remedy by examining "systematically the impact of churches and church membership patterns on support for third parties and independent candidates" (p. 3). They contend that religious institutions and American religious adherence work against minor candidates, and they provide a firm theoretical foundation and a wide array of empirical evidence to support that

After introducing the topic in chapter 1, the authors present a cogent theory of religious institutions and minor parties in chapter 2. Drawing on the literature on contextual influences on individuals, they argue that religious institutions shape political behavior not only through their effect on individual religious beliefs but also by providing a social context through which the information that citizens receive about candidates and political parties is filtered. Involvement in the formal and informal social networks within community

institutions should reenforce the dominant values of the community and thus "should operate to the benefit of established political institutions—the major parties" (p. 22). This is particularly true of social networks within religious institutions because of the historical and contemporary ties of religious denominations to the Democratic and Republican parties and because social interaction in church tends to have a stronger effect on individual behavior than does interaction in other settings. Based on this theory, Gilbert and colleagues hypothesize that higher levels of religious adherence within local areas, specifically counties, should lead to lower vote percentages for minor candidates, and individuals with higher levels of religious devotion should be less likely than others to support third candidates.

The authors make a compelling theoretical claim for the importance of religious institutions and their social networks for the enduring political dominance of the Democratic and Republican parties. The case is made even more convincing by the thorough and thoughtful empirical analyses in chapters 4 through 7. Gilbert and colleagues employ a wide range of data—from the National Election Studies, to state-level exit polls, to aggregate data on county-level religious characteristics and election results—and combine individual and aggregate data in nice ways to examine contextual effects on individual political behavior. They also apply sophisticated statistical techniques, including Gary King's method of ecological inference to test individual-level hypotheses when survey data are not available.

In chapter 6, the book turns from national elections to assess the effect of religious factors on state-level voting behavior and minor candidate success. The state focus allows the authors to make use of variations in religious and political contexts that are not present in national elections and to test their hypotheses in situations in which minor candidates actually may have a chance to win. These scholars clearly have in-depth knowledge of state politics and campaigns, which allows them to adjust hypotheses and empirical tests in subtle ways. We learn that religious factors generally work against third-party efforts, even at the state level, but some minor candidates, through appeals targeted to religious audiences and considerable statewide name recognition, can make religion work in their favor.

An important and interesting question raised is whether minor candidates have to find ways to reverse the typically negative effect of religion in order to be successful. The initial answer seems to be in the affirmative, as the authors suggest that the successful minor candidate or party is one that infiltrates the existing social networks in churches and other community institutions: "Candidates who choose not to work through preexisting institutions such as churches are therefore highly disadvantaged from the outset ... candidates, whether major or minor, must gain access to the social networks and institutions in communities" (p. 23). Another possibility may be suggested, however, by one case they discuss—the unusually high success rates of independents in relatively nonreligious Maine-and by one that emerged after the period of their study, the gubernatorial victory in Minnesota of Jesse Ventura, a candidate with no apparent ties to church-based institutions and an outspoken critic of organized religion. Given the historic ties of religious denominations to the major parties, the candidate- and mediacentered nature of modern campaigns, and the fact that the United States is becoming, by some measures, slowly less religious, one way for a minor candidate to find electoral success may be to bypass traditional church networks in order to appeal to secular and nominally religious voters directly through television and other modern campaign technologies.

This book makes a significant contribution to our understanding of third-party politics by showing that the unusually high level of religious adherence in the United States provides yet another barrier to the success of minor candidates. It also suggests interesting paths for further research. One might be a direct test of the authors' theoretical argument. A strong case is made for the idea that the communication networks and patterns of social interaction in churches provide the mechanism for religiosity's negative effect on third-party campaigns. To be sure of that, however, direct examination of those networks and the extent to which they overlook or denigrate minor candidates is needed. An intriguing further step might be to examine whether churchbased social networks tend to give preference to a range of status quo candidates and policies in addition to the established political parties.

Another important question concerns the consequences for third-party politics of the slow growth in American secularism. The findings of this book suggest that such trends should have a positive influence on the chances of minor candidates. Yet, there is some scholarly evidence that secular individuals are becoming more likely to vote for and identify with the Democratic Party. A key question is whether that will negate the potential gains for minor candidates from growing secularism or will open up new avenues for minor candidates to appeal to religious people.

Separate Destinations: Migration, Immigration, and the Politics of Places. By James G. Gimpel. Ann Arbor: University of Michigan Press, 1999. 403p. \$57.50.

James M. Glaser, Tufts University

James Gimpel has written a deceptively complex book. He examines recent migration and immigration trends and very methodically studies the various electoral consequences of these trends. He does this in seven states and in four counties or regions within each state, which allows him to investigate what happens politically in places that gain as well as lose population. And he studies the relationship of migration and politics at two points in time, using data from the 1980 and 1990 census. There is a lot going on, but in the end one big idea shines through: Demographic trends in the United States are reinforcing racial and socioeconomic segregation that has long been in place, and this continuing balkanization of the American population is having deleterious effects on our politics.

The bottom line is not highly original, but by pulling in how migration and immigration patterns feed the balkanization and by looking at participation levels and political orientations in areas of varying degrees of in- and outmigration, Gimpel makes a genuine contribution to an underdeveloped literature. He argues that demographic "humpiness" caused by migration patterns depresses turnout by discouraging political competition (p. 210). Party regularity, "the extent to which party registration matches the balance of party voting" (p. 296), diminishes in areas of growth and change (although not so much in areas of population decline), and this is a by-product of the destabilizing effect of inmigration on a community. He also argues that U.S. demographic patterns make racial and ethnic problems extraordinarily difficult to solve. Pervading the book is a pessimism about how increased balkanization undermines the practice of pluralist politics and encourages instead "the kind of special interest centeredness characteristic of so much contemporary American electioneering," (p. 237) which "places racial and ethnic harmony even further beyond our reach" (p. 279).

A second current running through the book has to do with how immigration and migration shape the political behavior of different groups natives, immigrants, and interstate migrants. This generally involves speculation rather than empirical demonstration, but it is key to Gimpel's argument. Underlying much of the analysis is a social psychological orientation that explains individual political behavior by way of group dynamics. For instance, Gimpel shows that immigration of nonwhites often leads to aggregate political patterns that suggest native hostility, and he interprets this as a response to group threat. He also shows that immigrant groups who attain a critical mass tend to be more spatially isolated, either because of their own inclination or because of discrimination in the outside world. Gimpel demonstrates, in some of the most compelling parts of this work, that the attitudes and behavior of both natives and immigrants (and in some cases, migrants) are shaped by their own-group orientation and by the interplay of groups within some politically relevant boundaries.

Also important in Separate Destinations is a discussion of the suburbanization of the country and what that means politically. The maps illustrating the growth of suburban sprawl are vivid and reinforce the impressions one gets driving around the outskirts of many metropolitan areas. This powerful demographic trend is not without political consequence, and Gimpel demonstrates how the transient nature of suburbia has been a net minus with regard to politics. Those moving into places such as suburban Kansas City or Denver are less participatory and less connected to issues in their community or state, two things that go together. In addition to the issue of sprawl, which Gimpel generally does not address, the suburbanization of America has contributed greatly to the race- and class-based balkanization that concerns him so much.

Gimpel accomplishes a great deal, but the book is not without problems. One—that the author recognizes and frets over-stems from the extensive use of ecological data. The main unit of analysis is the county or Census tract, and Gimpel looks at the relationship between demographic balance in these units and various dependent variables, such as turnout and party regularity. He is generally careful about not using the results to make inferences about individual behavior, he tries to replicate his findings with individuallevel data, and he occasionally relies upon Gary King's ecological inference maximum likelihood technique to deal with the problem. Nonetheless, in many instances his findings are limited, and his main defense is that he has not written a book about individuals, such as Thad Brown's Migration and Politics (1988), but a book about places, about what happens politically in places that gain or lose population.

A second problem revolves around the implications of Gimpel's findings. The author wrings his hands over homogeneous, noncompetitive electoral districts and the shortcomings of single-interest representation (p. 139), but although balkanization may be a necessary cause, it is certainly not a sufficient cause of problematic districting. In addition, he raises concerns about the possible skewing of policy outcomes as a result of balkanized politics, but these arguments are not developed at all. Finally, it is clear that increased participation is a good thing and depressed participation is not, but it is never very clear why the other main dependent variable—party regularity—is a matter of great concern. Gimpel argues that "party regularity matters because partisanship is a reflection of what divides, animates, and mobilizes the electorate." Where party regularity is upset, "judgments [by representatives] based on party cues are more error prone and electoral accountability can be

undermined" (p. 333). But this justification seems a stretch (indeed, electoral accountability is likely to be enhanced by greater uncertainty in results), which is unfortunate, given how much of the analysis is dedicated to it.

Despite its shortcomings, Separate Destinations is very good. It is comprehensive and well argued. It is fascinating and fun for "mapaholics" like myself. And it is a book of importance, one that helps explain how demographic and political changes are related in this extraordinarily mobile society.

Calculating Risks?: The Spatial and Political Dimensions of Hazardous Waste Policy. By James T. Hamilton and W. Kip Viscusi. Cambridge, MA: MIT Press, 1999. 336p. \$37.50.

Brian J. Cook, Clark University

James Hamilton and Kip Viscusi have produced a wideranging, detailed examination of Superfund, the national program for cleaning up hazardous waste sites. They focus on the nature and extent of the risks posed by Superfund sites, the manner in which regulators determine risk estimates, the societal costs and benefits of site remediation decisions, and explanations for the patterns of decisions about risks, resource allocations, and the resulting distributions of benefits and burdens. This is roughly the order of the first six chapters. The final three chapters cover, respectively, the environmental justice implications of risk estimates and remediation decisions, housing market evidence regarding private risk estimates, and the effects of several proposed regulatory reforms.

The authors rely on a sample of 267 nonfederal Superfund sites, based principally on sites that had records of decisions available when the study began in 1992. The bulk of the findings, however, is based on a 150-site subsample for which they were able to compile detailed site-level risk assessments. After subjecting the subsample to detailed quantitative analysis, Hamilton and Viscusi generate a substantial volume of policy-relevant findings, about which I can only report the briefest of particulars here.

They find that the mean cumulative lifetime cancer risk for the 150-site sample is 7 in 100, with a median of 25 in 10,000. These figures are quite high, given that the trigger for required remediation under Superfund is 1 in 10,000, and the lower bound for at least considering action is 1 in one million. The authors report a median cost per cancer case averted at the 150 sites of \$388 million. This compares quite unfavorably to a reference point of \$100 million, which represents the cost-per-life-saved threshold below which "the Office of Management and Budget has never rejected a proposed regulation" (p. 223).

The whole point of the analysis in Calculating Risks? is to critique the EPA methods and the larger Superfund decision-making process that have yielded these numbers. Thus, the authors find that Superfund's statutory foundation, the EPA guidelines for preparing risk estimates, and the process for selecting the type and extent of site remediation have resulted in vastly overstated risks associated with Superfund sites. These same factors have produced remediation expenditures that far exceed any reasonable public standard for preventing both cancer and noncancer health effects. They also far exceed the private willingness-to-pay behavior exhibited by people who live in proximity to Superfund sites.

These risk estimate and cost-benefit findings, as well as the extensive details that underlie them, deserve extensive scholarly and public scrutiny because Superfund is such a highly

visible, widely recognized public policy. The author extends their analytical foray into the political dimensions of Superfund site-level decision making and its effects, and it is to these dimensions that I believe readers of this *Review* will be drawn.

Hamilton and Viscusi point out that Superfund site-level risk assessment and remediation processes are highly decentralized. EPA regional managers are principally in charge; their decision making is restrained by statute, regulation, and agency policy guidance, but they retain considerable discretion. Hamilton and Viscusi conclude that this discretionary decision making is shaped principally by two forces: risk "biases" and local political influence. The biases may be internal to the agency regulators, meaning their own misperceptions of risk may lead them to employ overly conservative assumptions in making risk estimates. The biases may be external, reflecting the misperceptions of residents around a Superfund site, whom the regulators in some sense represent. The local political influence, the authors conclude, is manifested in varying levels of collective action. That is, if residents are highly mobilized politically (as measured by the authors through the proxy of voter turnout), then regulators are likely to reach overly protective risk estimates and to pursue overly stringent cleanup efforts.

In response to political pressure, and because of deviations from "full information versions of expected utility theory" that produce "perceptional biases," "irrational responses to risk," and "errors in risk judgment" (p. 129), EPA managers do not behave in accord with "efficient policy design" (p. 154). This seems an oddly narrow and overly restrictive interpretation of what is going on, reflecting the authors' exclusive devotion to economic rationality. I would argue that rational choice theory and research on risk perception and risk communication suggest that both the residents and EPA managers are behaving in politically rational ways. The managers are responding to forces in their environment that touch on critical components of their utility calculus. Residents are responding to what resources are available to them and what they can and cannot control but would like to achieve with respect to their exposure to health and environmental risks.

This is a very interesting, complex collective action problem, and I find it curious that the authors fail to examine it from this perspective. The reason appears to lie with the unidimensional measuring stick they use to judge regulator behavior. "Our results indicate that greater scrutiny from residents pushes regulators away from decisions likely to maximize social welfare" (p. 155), but this is social welfare narrowly defined. It does not take into account the value to residents of engaging in collective action or the value of being able to control through public means what they cannot control through private, market transactions and limited private resources. For public managers there is the value of being responsive to the public being served.

All this is not to say that in the end the "social welfare" calculation would come out differently. Hamilton and Viscusi make the extremely powerful point that by paying greater heed to efficiency considerations, EPA managers may in fact "enhance equity" (p. 188), because most of the sites in their sample around which substantial minority populations reside would pass the benefit-cost test they propose to apply. Strangely, however, they recommend more discretion for program managers as a way to achieve greater program efficiencies, without explaining how the managers will be able to resist the political pressure to which they are already subject. One can infer from their research, although this is not explicitly stated, that a more effective risk communication

strategy must be part of achieving the efficiency results they seek.

Despite the volume, depth, and detail of the quantitative analysis, the authors fail to provide a convincing empirical foundation for several of the more speculative conclusions they reach about political behavior. Such a foundation would most likely have to be compiled through more qualitative research, looking in depth at the perceptions and decisionmaking strategies of both residents and regulators. Nevertheless, I find much to appreciate in Calculating Risks? It is a significant achievement in terms of substantively comprehensive and methodologically sophisticated policy analysis. The book deserves the attention of scholars and practitioners in the policy and decision sciences as well as in environmental studies. Because the book offers a range and depth of material that is far more than one review can adequately assess, I call on other scholars to give it close scrutiny and engage the authors in an exchange of ideas over the full spectrum of issues they raise about Superfund.

Reading Public Opinion: How Political Actors View the Democratic Process. By Susan Herbst. Chicago: University of Chicago Press, 1998. 256p. \$41.00 cloth, \$16.00 paper.

John Brehm, Duke University

Susan Herbst tackles the vital question of how political decision makers conceive of public opinion and its role in representative democracy. The preponderance of scholarship asks questions about the nature of opinion itself. What does the public believe, and why does it hold such opinions? Some scholars asks whether public opinion translates into policy outcomes. But research is considerably sparser when one turns to the mechanisms by which public opinion influences or fails to influence policymakers, and it is this role that Herbst's splendid book considers.

Specifically, Herbst combines intensive interviewing and survey analysis to study how legislative staffers, journalists, and party activists understand public opinion and its relation to policymaking. The intensive interview sample draws from Illinois state government for the staffers and journalists, and a national sample of party activists is used for the survey analysis. Herbst is quite persuasive in her argument that subnational politics serves as a useful and important lens for examining the role of public opinion: Much of the policymaking that matters to people takes its form in the decisions by state government. Furthermore, the pragmatic issues of state government may create opportunities for citizen opinion to influence concrete decisions by state legislators. Illinois is an important state, composed of urban, suburban, and rural constituencies, and a reasonable basis on which to consider how political elites might attend to the opinions of the public. Herbst is careful, however, to avoid overgeneralizations from one state at one moment in time.

Herbst's reliance on in-depth interviewing carries the usual advantages and disadvantages. The reader learns more about the causal reasoning of the diverse actors than might be available from a more rigid survey instrument. Their logic is made all the more palpable by Herbst's liberal use of direct quotations from interviewees. Of course, there are always the questions of the replicability of such intensive conversations, whether interviewees responded along the lines of what they thought Herbst wanted to hear (demand effects), and whether the findings can be generalized to a broader population. Herbst takes considerable pains to document her methods, and the appendices provide ample detail on the

interview protocol and sample selection. Furthermore, survey-based methods are not immune from demand effects.

Herbst finds that staffers think of public opinion in terms of what "groups" have to say but do not put much stake in public opinion polls or even elections. The staffers report that polls are not very useful in policymaking; they consider the public too fickle and uninformed, and the polls themselves are too vague to provide much guidance. In this regard, Herbst's work is quite consistent with John Kingdon's observations about the decisions of members of Congress. The staffers view the public as more reactive than influential in the formation of policy.

Like the staffers, the journalists tend not to think of public opinion as aggregate opinion; they tend to regard public opinion as a "conversation" sensed by members of the press. The journalists are remarkably asystematic in their assessment of public opinion, relying more on an "imagined" public than on more careful study.

In the chapter on party activists, Herbst makes an abrupt shift from intensive interviews with people in Springfield, Illinois, to a national sample. Unlike the staffers and the journalists, the party activists do consult polls as a principal means for assessing public opinion. Like the staffers and journalists, the activists also generalize based on conversations with real individuals. The shift from state to national is too abrupt, and one questions whether the greater use of aggregate data by the national activists is the result simply of a shift in venue. The national public may be more occupied with less technical issues, more frequently surveyed than state populations, may be more heterogeneous, and perhaps more amenable to aggregation.

It is also surprising that, apparently, none of the three groups mentioned any real constraints on their authority that stem from the public. For the journalists, readership, advertising dollars, and market share all lead to important tensions in their presentation of public attitudes. For the staffers and party activists, it is surprising that they apparently put little stock in elections when considering what the public wants.

Herbst's discussion could be better connected to relevant work on the micro- and macroanalyses of public opinion and representation. James Stimson, Michael MacKuen, and Robert Erikson ("Dynamic Representation," American Political Science Review 89 [September 1995]: 543-65) surface only in a footnote, although their idea is clearly pertinent and in may ways quite sympathetic to the general thrust of Herbst's argument. The literature on "framing" effects enters only minimally, yet it clearly focuses on an important relationship among representatives, the media, and the public.

Of course, scholars in the field know that "public opinion" means more than a simple aggregation of surveys and polls. Herbst's book is particularly compelling in its argument that political actors themselves conceive public opinion in broader terms, as a conversation with rich reasonings and casual understandings that can elude surveys. Reading Public Opinion is an important and sizable step in the direction of understanding how public opinion works in representative democracy.

The Unsteady March: The Rise and Decline of Racial Equality in America. By Philip A. Klinkner with Rogers M. Smith. Chicago: University of Chicago Press, 1999. 417p. \$32.50.

Lawrence D. Bobo, Harvard University

In a theory-driven political history of race in the United States, Klinkner and his collaborator openly challenge two

modern shibboleths. First, they debunk the claim that the force of lofty values of equality and fairness—the American creed-will lead inexorably to racial progress. Second, they debunk the claim that the current level of black advancement is secure and now self-sustaining. In the place of these bulwarks of "the new color blindism," Klinkner and Smith erect a three-part analysis of the halting state of racial progress in America. They suggest that significant improvements in the status of African Americans have occurred only in certain circumstances: in the wake of a major war effort that requires the mobilization of large numbers of African Americans; when the nation's enemies are such that wartime ideology stresses egalitarianism, inclusiveness, and achieving greater democracy; and when strong domestic protest movements demand fulfillment of democratic ideals. To wit, progress for blacks has always required serious external threats and depended upon vigorous internal pressures from below. Without these conditions, history makes it clear that institutions of American government, and indeed the mass of white Americans, are quite ready to tolerate the oppression and exploitation of African Americans.

Accordingly, Klinkner and Smith find only three periods of significant improvement in the status of African Americans: immediately following the Revolutionary War, immediately following the Civil War, and in the two decades following World War II. After each period, however, there was substantial retrogression and sometimes quite disastrous deterioration in the status of blacks. If not openly pessimistic, this highly contingent and "two steps forward, one step back" model of racial change cautions strongly against naive claims that America's high moral values always prevail. As the authors argue, "many Americans have always been able to tell themselves that they were remaining true to their best ideals, the advancement of white Protestant civilization, a noble common cause endangered by what they genuinely believed to be misguided and destructive radical egalitarianism" (pp. 6–7).

Although not a work of primary historical analysis, the book's argument is an historical one. In chronological fashion. Unsteady March traces sequences of events, the changing views of significant historical figures (e.g., George Washington, Abraham Lincoln, Harry Truman), and tries to craft a rich portrait of different eras and their human meaning. The authors show quite convincingly how wartime exigencies facing Washington in the Revolutionary era, and Lincoln later in the Civil War era, resulted in substantial military roles for African Americans and the concomitant effect of such service in buttressing blacks' claims for better treatment. Most telling, Klinkner and Smith also make it clear why World War I, the Spanish American War, and other military engagements failed to rise to the level of mobilization and threat needed to inspire domestic protest or to provide leverage for democratic reform. They also give telling illustrations of how some black allies of one era (the Civil War), such as white abolitionist Charles Francis Adams, Jr., would be important players in rationalizing a later era of retreat (rise of Jim Crow), a pattern seen later in the wake of the civil rights gains of the 1960s (e.g., Daniel P. Moynihan and "benien neglect").

In each period, Klinkner and Smith devote commendable attention to the currents of thought and protest among African Americans. For example, they give extensive consideration to the roles of Booker T. Washington, W. E. B. DuBois and the NAACP, Marcus Garvey, and later A. Philip Randolph and the "March on Washington" movement. They also do much to convey the great symbolic import of such events as Joe Louis's defeat of German boxer Max Schmel-

ing. The episode, like many they recount, is not just historical embroidery. It bears directly on their argument about the importance of external threats and democratic rhetoric. When Louis lost his first bout with Schmeling in 1936, most American news media and elites treated the outcome as a victory for the white race (including the U.S. Senate, which burst into applause and stopped business when the result was announced). Yet, by 1938, with a mounting Nazi threat in Europe, Louis's victory was more often read as a triumph for freedom and democracy against an emissary of fascist government.

Unsteady March offers a painstaking analysis of how Cold War imperatives profoundly reshaped the level of national elite support (or tolerance) for Jim Crow racism. Klinkner and Smith are equally careful to document how the service and sacrifices of black troops during World War II emboldened blacks and increased the moral leverage behind their claims on American democracy. A quite strong case is made that federal government responsiveness to black demands for civil rights grew at least as much from a self-interested concern for how "the leader of the free world" was viewed abroad in the struggle against communism as from any genuine sense of morality or deepening comity with African Americans.

Among the most provocative aspects of the book is its enumeration of eleven strong parallels between the current social context and the rise of Jim Crow racism from the ashes of Reconstruction during the nineteenth century. The first four are particularly troubling, since they are notable aspects of the current climate and definitely set a stage for the next round of "one step backward." These are: a resurgent emphasis on states' rights and local decision making, and a retreat from national government authority; calls for "colorblind" policies and a retreat from "special treatment" of minorities; resurgence of free market or laissez-faire ideologies; and a resurgent respectability for scientific racism. The full effect of these then, and perhaps now as well, was deterioration in the status and living conditions of many African Americans.

There are three problems with the argument Klinkner and Smith develop. First, perhaps because this is theory-driven history, the book sometimes reads as if only the evidence in support of the basic argument is selected for inclusion. To be sure, in the end I am largely persuaded by the argument. A primary historical account, I suspect, would have dealt more extensively with the countercurrents that surely did exist. Second, and more troubling, the argument leans too heavily toward privileging politics; it is a sociologically underdeveloped theory. Too little attention is dedicated to the economic, demographic, and other social forces that have played substantial roles in facilitating progressive racial reform. For example, such profoundly important works as William Julius Wilson's The Declining Significance of Race (1978) and The Truly Disadvantaged (1987) are not mentioned at all. And the central importance to black progress of an expanding economy, for instance, is only dealt with implicitly.

Third, the argument lacks an explicit conceptual underpinning about the nature of racism and the well-springs of white animosity to African Americans. Klinkner and Smith repeatedly use the phrase "racial hierarchy," invoke Du Bois's notion of the "psychological wages" of whiteness, and generally craft a power and interest-group analysis of the dynamics of race relations. It is ironic, then, that at the end of the book they endorse Donald R. Kinder and Lynn M. Sanders's racial resentment theory (Divided by Color, 1996), rather than Blumer's group position approach, Jackman's ideological control and paternalism theory (The Velvet Glove, 1994), or

other power and conflict models of racism (again, perhaps, reflecting the lack of engagement with much of the closely allied sociological research).

In the main, however, *Unsteady March* is a serious political history that must be attended to by anyone concerned with the American racial divide. Indeed, it is a long overdue antidote to much recent cavalier theorizing that race only became "political" after the civil rights era and to the equally ahistorical and misplaced calls for a "color-blind" polity. In an age of much political and scholarly mendacity on the matter of black-white relations, Klinkner and Smith pose the tough questions and offer a sobering set of answers about the nation's complex and enduring struggle over race.

Legal Advocacy: Lawyers and Nonlawyers at Work. By Herbert M. Kritzer. Ann Arbor: University of Michigan Press, 1999. 269p. \$44.50.

Marc-Georges Pufong, Valdosta State University

What difference does a lawyer make? That is the central question of Kritzer's book, which asks to what degree non-lawyers should be prevented from providing services in competition with licensed attorneys. Can nonlawyers be effective legal advocates? Should licensed lawyers continue to hold an unfettered monopoly over the provision of legal services? The effectiveness issue hinges on competency in actual legal representation afforded to the public, whereas the licensing issue has to do with the monopoly of legal services.

The exclusion of nonlawyers from providing law-related services is a twentieth-century phenomenon, largely a product of the development and growth of the organized bar associations during the 1920s and 1930s (see Richard Abel, American Lawyer, 1989). The rising concern over the high cost and quality of legal representation in the United States has led many people to argue that formally trained lawyers should not be the only effective advocates. A handful of states have conducted studies and others are considering legislation that will allow nonlawyers under specific and limited circumstances to provide routine legal services to the public.

Since its inception in 1878, the American Bar Association has fought any move in that direction and has generally been successful in limiting potential competitors. The United States is alone among common law countries in prohibiting nonlawyers from handling certain kinds of representation outside the courtroom. In England, solicitors are free to represent injured persons up to the stage of actually filling an action in the court. In Canada and specifically Ontario, nonlawyers routinely provide representation in the traffic courts as well in other lower court proceedings that include workers' compensation boards (see Harry Abraham, Judicial Process, 1993).

In the United States, most of the debate has taken place in the absence of meaningful empirical data about how lawyers and nonlawyers perform similar tasks or about the benefits of nonlawyer provision of legal services. Kritzer presents more than benchmark data. He provides a conceptually parsimonious way of looking at the debate backed by systematic research of these issues of legal practice by lawyers and nonlawyers. He focuses on four substantive areas: unemployment compensation appeals (chap. 2), tax appeals (chap. 3), Social Security disability appeals (chap. 4), and labor arbitration cases (chap. 5).

To compare differences between nonlawyers and lawyers, Kritzer uses both quantitative and qualitative data to evaluate advocacy and representational skills and effectiveness, broadly defined. For example, for the qualitative evaluation, he examines key attributes of efficacy that establish whether an advocate (1) recognizes the core questions at issue in a case, (2) engages in quality questioning of the witnesses, (3) presents evidence that is related to the issue being decided, (4) has adequate resources, (5) prepares clients for hearings, and (6) has the ability to counter or respond to issues raised by the opposing side or by the third party, such as adjudicators and mediators (p. 21). For the quantitative assessment, Kritzer uses samples from actual representation in the four key issue settings to compare lawyer and nonlawyer advocacy styles and abilities. His analysis shows that nonlawyers can be effective advocates and, in some situations, are more effective than professional lawyers.

In the area of unemployment compensation appeals (chap. 2), Kritzer focuses on Wisconsin, which has a fairly restricted eligibility criterion for unemployment compensation that results in numerous disputes between claimants and former employers or the agency administering the program. Kritzer found that although the judges in the appeals are experts and know the law and the evidence, the presence of a knowledgeable advocate makes a great deal of difference to the outcome of the case (p. 50). He concludes that knowledgeable and experienced nonlawyers can be effective in representing parties in these hearings, and formal training in law is less crucial than day-to-day experience in specific situations (pp. 76-7).

In the area of state tax appeals (chap. 3), Kritzer focuses on the Wisconsin Tax Appeal Commission to unravel the question of whether a lawyer or nonlawyer makes a difference (p. 79). Since taxpayers by right can turn to the commission to resolve their disagreements with the Wisconsin Department of Revenue, this arena attracts a pool of tax cases coupled with a highly technical and complex tax code. It also attracts different kinds of taxpayer advocates, namely, lawyers and nonlawyers who are tax specialists. Kritzer maintains that in tax disputes an expert representative is crucial to a successful appeal. In considering the comparative effectiveness of lawyers and nonlawyers, he observes that even though nonlawyers have an extensive "substantive expertise" in tax matters, they lack procedural expertise in court. The problem, Kritzer notes, is that they simply do not know how to appear as advocates in a formal legal hearing. They know what constitutes "a record" for tax purposes, but they lack legal knowledge about how those records could be used for evidentiary purposes in court (p. 108). Kritzer concludes that lawyers are far better equipped with "procedural expertise" than nonlawyers, especially in identifying and presenting evidence.

In examining representation in Social Security disability appeals (chap. 4), Kritzer reveals a small and statistically insignificant difference in effectiveness that favors lawyers over nonlawyers (p. 116). He argues that the small ratio is due to the fact that a clearly defined group of lawyers have substantial expertise in this area, whereas in the area of unemployment compensation there are few, if any, lawyers who are specialists in either the law or the hearing processes (p. 148). He also maintains that the difference in the lawyernonlawyer success ratio reflects the type of cases handled by each group. Licensed lawyers work on a fee-for-service or contingent fee basis, which may create incentives for them to screen out weak cases. In contrast, nonlawyers often work for agencies whose mission is to provide representation to lowincome persons and therefore do not select their cases on ability to win or monetary compensation (pp. 116-7).

In chapter 5, Kritzer focuses on labor grievance cases that usually result in arbitration. This area is interesting and worthy of examination because it regularly pits experienced

lawyers against experienced nonlawyers (p. 151). Kritzer maintains that arbitration hearings most closely resemble the traditional courtroom situation (but without the jury). These decisions are taken seriously by the parties involved because the ruling in one case may affect future cases, contract negotiations, and/or relationships among the parties (pp. 152–3).

Using data from the Wisconsin Employment Relations Commission, Kritzer concludes that there is no difference between nonlawyers and lawyers in terms of the quality of legal representation provided in the arbitration cases. To Kritzer, however, attributes such as experience and substantive expertise do matter. Therefore, despite the sheer percentage of cases won by lawyers on behalf of the unions over nonlawyers, the difference is not statistically significant, which means it is more likely a chance occurrence than otherwise. Similar findings have been noted by Doris Marie Provine (Judging Credentials: Nonlawyer Judges and the Politics of Profession, 1986) for nonlawyer and lawyer judges, but Kritzer maintains that the findings in chapter 5 contrast with those of Richard Block and Jack Stieber (The Impact of Attorney and an Arbitrator on Arbitration Awards, 1987), who reported differences between lawyers and nonlawyers.

Kritzer provides a solid basis for understanding the differences in legal advocacy, effectiveness, and ability of advocates with radically different legal training. This is a noteworthy book, and there is value in extending this approach to other areas, such as medicine, where similar debates about licensing are taking place. It cannot be said that all chapters in the book are equally well written, and aberrations can hardly be avoided. They may perhaps be attributed to the differences in the approach adopted and, especially, the laborious method of data gathering, which seemingly was improvised for each of the four case areas. For example, because of the differences in the legal processes, the advocates, and the cases themselves, there is no single integrated analysis either of the quantitative portion of the indicators or of the qualitative data used in the study. This is somewhat complicated and confusing, even if the approach is perfectly suitable for this study. Nevertheless, faculty and students of the sociology of professions and public law will find this book extremely important for research and seminar reading.

Debating War and Peace: Media Coverage of U.S. Intervention in the Post-Vletnam Era. By Jonathan Mermin. Princeton, NJ: Princeton University Press, 1999. 162p. \$45.00 cloth, \$14.95 paper.

Richard A. Brody, Stanford University

Debating War and Peace is energized by empirical and normative questions about the American people and foreign policy. The empirical questions deal with the breadth of information and policy debate available to the public about U.S. military interventions. Because most Americans are dependent upon news media for this information, Jonathan Mermin examines press practices that shape the gathering of information during periods of U.S. military deployment in the service of foreign policy. The empirical analyses lead to the conclusion that "coverage of U.S. intervention in the post-Vietnam era [in the mass media sources studied] made no independent contributions (except at the margins) to foreign policy debate in the United States. The spectrum of debate in Washington... has determined the spectrum of debate in the news" (p. 143).

The normative concerns of the book stem from Mermin's opinion that the news media short-change the American

people. By narrowly restricting debate about military intervention to the views of government officials, the press fails in its First Amendment responsibility to give the public the information it needs to play its role in the American democracy. If, Mermin opines, "the source of information on American foreign policy is the U.S. government, then what is being reported is not the story, but a story, one told by a powerful interested parties: Democrats and Republicans... much less of a difference than meets the eye" (p. 145). By restricting the debate to that offered by official Washington, the press neither acts as an independent watchdog nor fully mirrors the policy debate in the country at large. Under these circumstances, "it is hard to argue that the First Amendment ideal is being fulfilled" (p. 145).

It is easier to assess the empirical claims of the book than to evaluate its normative opinion that the press has not fulfilled its First Amendment responsibilities. The empirical analyses seek to establish to whom the press turns for comment on U.S. military intervention in foreign affairs. Mermin addresses this question in an appropriate and fully satisfactory manner. The source options for the media are either elites within or closely connected to government or those who can offer an independent perspective.

The study results are unambiguous: The media overwhelmingly rely on those now in government, those formerly in government, or on other journalists for the terms of debate. Mermin illustrates the narrowness of the circle with data on the affiliation of guests on the MacNeil/Lehrer Newshour programs covering eight military interventions after the Vietnam War (Table 2.1, p. 31). More than 70% were former government officials (31%), other journalist (21%), and current government officials (20%). Four percent of the guests were from Washington think tanks, and foreign observers comprised another 11% of the "experts" on the show. Only one commentator in eight came from outside Washington or the press corps. Data from the case studies-Grenada, Panama, and the crisis, military buildup, and war phases of the Gulf War-are equally unambiguous: The news presented to the American people in the first two weeks of these military interventions was derived primarily from sources in Washington. Moreover, Mermin's analyses of the direction of commentary on these military interventions shows that in the main it was supportive of the president's actions.

Mermin's normative complaints are harder to evaluate and can be addressed from several points of view. One can raise questions about Mermin's characterization of the First Amendment "ideal." Surely, there is a literature upon which he could have drawn to buttress his version of press responsibilities and privileges under the First Amendment. His criticism of a press that covers foreign affairs by "indexing" the debate among elected officials seems to reject republicanism. He, of course, has the right to reject republicanism, but in light of the structures built into the Constitution, that is an odd right to exercise.

Mermin's complaint about the restricted set of news sources ignores evidence on the public opinion consequences of elite criticism of presidential actions in an international crisis. If the media report a breakdown in support for the president's handling of a crisis, among those Mermin considers "insiders," a rally of public support will fail to materialize or will be terminated. It would be interesting to know if "outsider" criticism affects public opinion in a similar manner.

More generally, Debating War and Peace proposes "prescriptions" for "grant[ing] sources outside of Washington greater access to the news" (pp. 145-50) and, presumably in

consequence, for changing the balance of information available to the American people. Mermin argues that the press could better match the First Amendment ideal if, among other things, it consulted foreign policy experts, especially academics, outside Washington; reported the reactions of engaged citizens, such as those organized in groups like Peace Action or the American Friends Service Committee; and expanded critical analysis of the execution of U.S. policy to encompass the policy itself. These are interesting suggestions, but whether they would add to the knowledge people bring to bear in evaluating U.S. foreign policy is an empirical question, not merely a matter of the author's opinion. These suggestions seem more appropriate as ways of opening up the debate on U.S. foreign policy, in general, and less apposite to the fast-breaking, critical situations in which military power is used, such as the cases that comprise the empirical foundation of the book. The daily press may not be the best forum for the wider debate, but "outside" sources—as coverage in Seattle showed on the WTO and in Washington on the IMF and World Bank—certainly affect the information we get on foreign policy in general.

The Missing Majority: The Recruitment of Women as State Legislative Candidates. By David Niven. Westport, CT: Praeger, 1998. 208p. \$59.95.

Edith J. Barrett, University of Texas at Arlungton

For the past quarter-century, women politicians have been slowly gaining access to more and higher elected positions. Given where women politicians were not long ago, the progress seems great, but based on the radical changes in women's nonpolitical lives, one would have predicted greater strides in politics as well. David Niven asks why women have not gotten farther than they have. This is not a new question, but as long as advancement remains minimal, it is one that should be continually addressed.

Niven tests the hypothesis that there are few women political candidates because male party leaders, acting as gatekeepers, prefer candidates similar to themselves. The author pits this hypothesis against the rival notion that women candidates do not advance because they are not in the occupations that traditionally lead to public office. He focuses on the recruitment stage, because that is the point at which women enter the scene. Unless they are recruited as candidates, they cannot be elected to office. To begin to understand why there are disproportionately few women in public office, it makes sense to begin by asking why there are disproportionately few women running for public office.

Niven surveyed a sample of county party chairs and women holding local elective office, who are taken to represent potential state legislative candidates, in California, New Jersey, Ohio, and Tennessee. The party chairs were asked what they see as traits of a winning candidate (e.g., occupation, personality, gender); they were also asked about the recruitment process they use, their perception of biases in the process, and the strength and ideology of their locals party. Local female elected officials were asked similar questions to preferred characteristic of officeholders, how the recruitment process works, biases in the process, and ideology of local parties. The women were also asked about their political career aspirations. The study surveyed 280 county party chairs (a response rate of 54%) and 276 potential women candidates (a 53% response rate).

Although the recent presidential candidacy of Elizabeth Dole probably convinced most in the public that the party elite treats women candidates differently from male candidates, the issue is still debated in the literature (see, e.g., R. Darcy, Susan Welch, and Janet Clark, Women, Elections, and Representation, 1994; and Brian Werner, "Bias in the Elec-

toral Process: Mass and Elite Attitudes and Female State Legislative Candidates, 1982-1990," Ph.D. diss., Washington University, 1993). Dole, similar to rival George W. Bush in so many of her leadership credentials as well as in her political style, was given virtually no attention by the party elites. Indeed, the findings from Niven's survey suggest that there is some gender bias in the selection of candidates. The majority of the surveyed women officials reported that they had experienced bias personally, and the responses from the party chairs indicated that bias may exist within that group. For example, the latter tend to believe that female candidates have less chance of winning an election than male candidates, and especially the male party chairs rated male personality characteristics as more positive in a candidate than female characteristics. An examination of potential causes and ramifications of this bias fills the remainder of the book.

Twenty years ago, Rosabeth Kanter ("Some Effects of Proportions of Group Life: Skewed Sex Ratios and Responses to Token Women," American Journal of Sociology 82 [1977]: 965-90) suggested that once women enter the business world in large numbers, their actions will no longer be seen by men as inferior, and they will gain the respect of their male colleagues. Niven, too, found that party leaders in counties with a high number of women elected officials are less likely to see women as an outgroup than are leaders with fewer contacts with women colleagues. Party leaders, like humans in general, make assumptions about people with whom they have little personal experience. To the extent that the leaders have little information about the abilities of potential female candidates, they are more likely to use stereotypical assumptions, which quite often tend to be biased against the women. Leaders who are older, who have been in their position longer, and who wield a great deal of power in their party tend to be most likely to see women candidates as different from themselves and, hence, less appropriate for elected office. As these leaders retire or leave office and are replaced by younger, less singularly powerful party chairs, Niven predicts that bias against women candidates will decrease. Scholars of women in politics have been making this prediction for years, and it does seem to be bearing out. But the process is slow, and women still remain grossly underrepresented in high public office.

My major complaint about this book is that the writing style is dull. Although an occasional student can write a dissertation that reads as good as a book, most cannot. The dissertation genre of writing—an extensive literature review, followed by methods, findings, and conclusions serves its purpose well. When dissertations become books, however, the style needs to change to keep the reader interested in what the author has to say. The Missing Majority could have been so much more if the author had taken a little extra time to flesh it out beyond the original single hypothesis, to put the findings into a larger context of minorities seeking public office guarded by majority gatekeepers, and to add recommendations for change in the concluding remarks. Readers will find the contents worthwhile, but they may feel as though the same information could have been conveyed through one or two well-written journal articles.

Policy Reform in American Agriculture: Analysis and Prognosis. By David Orden, Robert Paarlberg, and Terry Roe. Chicago: University of Chicago Press, 1999. 270p. \$38.00.

James Anderson, Texas A&M University

Two economists and a political scientist have joined forces to produce this thoughtful, thorough, and carefully constructed study of agricultural policy reform. Specifically, the focus is on farm price and income support programs (or simply, the farm programs), which historically have combined financial assistance to farms with restrictions on crop production. Reform, as the authors specify it, involves reducing the intrusiveness of these programs, ideally culminating in their elimination. Although they aver that their "analytic tone remains more positive than normative" (p. 11), their ideological preference for the market shapes the narrative.

Reform of the farm programs is analyzed using the categories developed in a four-cell matrix. Reform can be instituted gradually over time or quickly; compensation may or may not be provided to farmers affected by reform. On this basis, a gradual, compensated closeout of farm programs becomes a cash out, whereas quicker, compensated elimination is a buyout. Slow reform without compensation is a squeeze out; quick, uncompensated repeal of the farm programs have been cast, whether successful or not, as cash outs.

The authors acknowledge, albeit a bit grudgingly, that the farm programs were justifiable in the 1930s because of the Great Depression. Due to the scientific, technological, and economic transformation of American agriculture, however, they are no longer needed. Briefly stated, their case for reform is this. The farm programs have done little to increase agricultural productivity, although other government policies have been helpful here, and they have not done much to reduce regular poverty. Producers of commodities that account for 60% of farm sales—livestock, fruits and vegetables, and specialty crops-receive no direct benefits from farm programs. Moreover, about 1.5 million farm units derive only a small fraction, if any, of their income from farming. For 536,000 large commercial farms, income from farming in 1995 averaged \$38,000 for those with sales under \$250,000 and \$303,000 for those with sales above that figure. They receive most of the farm program benefits. Finally, the farm programs hinder the movement of commodities in world markets. These points are well taken and collectively support the need for some kind of reform.

Orden, Paarlberg, and Roe want to get government out of agriculture and assume that the market will yield satisfactory results. They are so confident of this that they do not provide facts, analysis, or speculation about how the market will improve things. If under a market regime more farm sectors go the way of serine and poultry, where industrialized agriculture prevails, will the nation and consumers be better off? Will there be no social costs to pay?

The best two chapters discuss the adoption in 1996 of the Federal Agriculture Improvement and Reform Act, also referred to as FAIR and the Freedom to Farm act. It decoupled benefit payments to producers of export crops—wheat, corn, feed grains, cotton, and rice—from production controls, which were eliminated. Payments to farmers totaling \$36 billion between 1996 and 2002 are based on past production records. It is not surprising that almost all eligible farmers signed up for benefits, which are independent of market prices and require no production cutbacks. If this scheme works, and if benefits end as scheduled in 2000, it represents a partial cash out. Programs for nonexport competing crops, such as sugar and peanuts, remain in place.

FAIR represents the most drastic change in the farm programs since their adoption in the 1930s. The authors cite two factors as critical for its adoption. First, the Republican congressional majorities elected in 1994 were amenable to reform. Second, high farm prices in 1996 would have reduced payments to farmers under existing programs. They would receive more money under Freedom to Farm, which lessened resistance to change.

Orden, Paarlberg, and Roe are somewhat pessimistic about the future of FAIR. Will it be extended to other crops? Low farm prices or a return to Democratic control of Congress, for instance, could reduce the act's appeal and lead to its demise. They make various prognostications about the future of farm policy. The tenuous state of Freedom to Farm is indicated by the 1998 and 1999 congressional bailouts of farmers because of low prices and crop failures. These totaled \$15 billion.

The book does not do as good a job as it might in explaining why farmers have been so successful in getting what they want. The authors remark on a "three-sided nexus (farm lobbyists, congressional agriculture committees, and the USDA) that so often works to perpetuate existing farm programs" (p. 53), which they call the agricultural policy establishment. Its structure and workings are maintained to justify "fully staffed" agriculture committees and a large USDA. The political analysis in the book does not do much to help us understand why Congress adopted the bailout legislation in 1998 and 1999 and is likely to do so again in 2000. Of course, few call this reform.

Even readers who disagree with the perspective of this book will find it a valuable and accessible source of information and insight on agricultural policy and politics. The authors know their subject. Students of public policymaking will find this a solid contribution to the literature. It can, for example, help us understand why policy termination is so difficult and so uncommon. It is a fine example of qualitative policy analysis.

Remaking Chicago: The Political Origins of Urban Industrial Change. By Joel Rast. DeKalb: Northern Illinois University Press, 1999. \$36.00.

Martin Saiz, California State University, Northridge

Joel Rast forcefully challenges those who promote the notion that the policy choices in U.S. cities can be explained with reference to economic and demographic changes caused by a shift from a manufacturing to a service-based, postindustrial economy. Instead, he argues, Chicago's downtown-centered growth strategy was a consequence of an uncontested ideology of urban development informed by contemporary theories of urban political economy. Current models of economic restructuring, according to Rast, depict central cities as obsolete sites for industrial production and point to the adoption of a "corporate-center strategy" focused on the development of downtown office space and supporting land uses that require the replacement of newer-downtown industrial and lower-income residential districts with back office and upper-income housing, as well as the building of hotel, cultural, and sports facilities. Yet, through an examination of Chicago's redevelopment efforts between 1955 and 1997, Rast shows that the rise of advanced corporate services and the corresponding decline of near-downtown industrial districts was not a foregone conclusion. To the contrary, Chicago's transition to a corporate-centered service economy was a product of policies created by an alliance between the Daley machine and key representatives from the downtown business community.

As further proof that Chicago's renewal and gentrification strategies were not products of economic pressure, Rast documents the rise of a viable development alternative. Only relatively large, vertically integrated manufacturers were interested in cheaper land and labor costs on the urban periphery and elsewhere. The small- to medium-sized man-

ufactures engaged in contracting or subcontracting for local or regional markets showed little interest in suburban locations. Through case studies of the printing and garment industries, Rast shows how these firms were spatially rooted—bound to one another through supportive networks and trust—yet were almost completely uprooted by Chicago's redevelopment programs and land use regulations. It was not until lower-income and blue-collar neighborhoods shifted from social protest to neighborhood economic development efforts and forged new relationships with local manufacturers that what Rast calls a "local-producer strategy" began to emerge. This new coalition found sympathy within the Harold Washington administration for the idea of industrial retention and eventually challenged the city's myopic emphasis on downtown redevelopment.

Rast argues that Chicago's efforts to transform its economic base from manufacturing to services were not required to stem the post-World War II tide of central city residential and industrial decline. In an insightful twist of conventional wisdom, he shows that Chicago's downtown revival was successful because city officials could indeed influence the form and trajectory of corporate investment. Yet, these same officials overrated the effects of corporate restructuring, technological innovation, transportation improvements, and regional variations in land and labor costs on near-downtown manufacturers, and they underestimated their ability to protect industrial jobs through land use regulation. Rast boldly argues that politics and policy choice (or the lack thereof), not economics, determine the course of cities.

Even as an advocate of the political economy approach to understanding social behavior in cities, I did not find it difficult to accept the argument that politics and policies shape cities. John Mollenkopf demonstrated almost twenty years ago that local growth coalitions are instrumental in gaining the federal policies (subsidized home mortgages and interstate highways) that lead to the development of American suburbs. The policies of public school integration, mortgage redlining, deed restrictions, and exclusionary zoning help complete the project and prompt center city decline. Although Rast clearly proves his thesis and makes a significant contribution to the field of urban politics, many of his theoretical points are scored against what borders on straw theory. Not even the most dogmatic structural Marxist believes that the economy is completely coercive. Most adherents of urban political economy are more sensitive to the social nature of markets, the pervasive power of growth politics, and the policy consequences of regime change.

Rast freely borrows from political economists John Logan and Harvey Molotch. Their notion of community power structures dominated by a local land-based growth coalition explains much of Chicago's policy behavior, and it is here that Rast makes his most notable theoretical contribution. Refusing to view business monolithically, he expands the notion of use value by showing that Chicago's industrial users develop neighborhoods of informal support similar to those created within residential districts. It is the spatial rootedness of these industrial agglomerations and the possibilities for forming alliances with other groups that value urban land for its use that expands the policy options of cities.

Rast's real concern may be with economic development practitioners, planners, and journalists who give certain credence to the global logic of contemporary urban development. The faithful acceptance of autonomous markets, arrant capital mobility, and the location-driven rationality of firm behavior seem to blind policymakers to the possibilities of an alternative growth strategy. Facile models of global economic

restructuring imply that cities can do very little about their economies, save ride the waves of the postindustrial future and deal with the consequences as best they can. Unfortunately, the development strategies following from these assumptions have led to the replacement of well-paying manufacturing jobs with low-paying service sector occupations and increasing disparity in the distribution of wealth and income. At the same time, public officials, concerned with discouraging mobile investors, are cautious of instituting redistributive policies as a way of reducing class tensions exacerbated by their policy choices. Rast's perceptive analysis showed that a rival growth coalition composed of locally rooted producers, workers, and community-based economic development organizations can support an alternative growth strategy that employs local productive assets and may reduce the very need for redistributive policies.

Reconstructing Times Square: Politics and Culture in Urban Development. By Alexander J. Reichl. Lawrence: University Press of Kansas, 1999. 239p. \$39.95 cloth, \$19.95 paper.

Charles J. Spindler, Auburn University

Since its official creation in 1904, New York City's Times Square has experienced a remarkable rise and fall, constant reinvention, transformation, and rebirth. In this detailed case study, Alexander Reichl documents urban policy directed toward the redevelopment of Times Square between the late 1960s and the late 1990s. The detailed analysis is a valuable contribution to the historically informed study of urban development politics.

The use of symbolism to manage conflict and identify space is a central theme throughout the book. Reichl considers various arenas of conflict: between development interests and neighborhood groups, between the public discourse focused on historic preservation and the arts and the real politics of redevelopment, between the interest of large national and multinational developers and landowners, and between the interest of developers and politicians and the interests of African American and Latino youths. Reichl analyzes symbolism and the symbolic use of space within each of these arenas. For example, he suggests that historic preservation and the arts became symbols used by a progrowth coalition to create public support for redevelopment. Images of Times Square as the Great White Way became symbolic of an idealized past, present, and future and framed public discussion of redevelopment. Low-income African Americans and Latinos were presented as symbols of physical threat in Times Square by the progrowth coalition. Disney's participation in the redevelopment symbolized to the development community both the economic feasibility of investing in Times Square and the security of that investment. To Reichl, Disney's participation also became symbolic of the corporate domination of so-called public space (which Reichl calls "Disneyspace"). Ultimately, Times Square was transformed from a symbolic image of urban decline to a symbol of urban renaissance.

Reichl clearly aligns his analysis with the urban regime model developed by Clarence Stone ("Systemic Power in Community Decision Making: A Restatement of Stratification Theory," American Political Science Review 74 [1980]: 978–90; "Summing Up: Urban Regimes, Development Policy, and Political Arrangements," in Clarence Stone and Harry Sanders, eds., The Politics of Urban Development, 1987, pp. 269–90; Regime Politics: Governing Atlanta, 1946–1988, 1989; and "Urban Regimes and the Capacity to Govern: A

Political Economy Approach," Journal of Urban Affairs 15 [1993]: 1–29). The model emphasizes cooperation between political and economic interests in order to develop electoral coalitions and to govern. The conflict between opposing economic and political interests is resolved through the creation of governing regimes. The model suggests that the local political context frames policy choices, and the governing regime is a primary determinant of policy choices. By contrast, the economic determinism model developed by Paul Peterson in City Limits (1981) emphasizes an economic approach to policy choices. It suggests that larger state and national economic forces impose certain policy choices upon a city. Peterson argues that cities have a unitary interest in promoting economic development.

Reichl focuses on the management of conflict among different economic, cultural, and ethnic interests, which leads him to reject the economic determinism model. He acknowledges that "the overwhelming predominance of support [for redevelopment]...might reasonably approximate the kind of broad-based support for development described by Peterson" (p. 142), but he suggests that this support was politically constructed "by means of a coalition strategy that centered on cultural politics," which recast the political debate in a way "that predisposes higher-income urban residents to support redevelopment" (p. 142). Ultimately, Reichl argues, economic factors alone do not explain the process of postindustrial restructuring experienced by many American cities. "The argument presented here is that political factors play a central causal role in changing patterns of urban redevelopment" (p. 10).

Yet, Reichl presents valuable evidence that supports Peterson's economic determinism model. For example, the economic potential of Times Square was vastly underdeveloped: "No new construction had occurred in the immediate environs of the Forty-second Street project area ... since 1931. Consequently property taxes generated from the area-... were but a fraction of the potential, in fact the total tax revenue was less than that received from a single building a block east at Sixth Avenue" (p. 67). Reichl found that business, civic, and political leaders were convinced that real and perceived problems, such as "crime, drugs, pornography, prostitution and menacing African-American and Latino youths," threatened the "economic and social health of the entire west side of midtown Manhattan" (p. 5). The decline of the city's real estate market in the 1990s was in part the result of the stock market crash of 1987. Reichl's analysis also makes clear that the emergence of a postindustrial economy in New York City widened the gap between rich and poor, which was a factor contributing to the push for redevelopment. The growth of the global economy led to the emergence of the postindustrial economy in New York.

At times Reichl blurs the distinction between political regimes and growth coalitions or growth machines. "I use the term 'pro-growth coalition' and 'pro-growth regime' to refer to the enduring coalition of interests that were unified behind a broader pro-growth agenda for New York City" (p. 190, n. 56). The urban development literature generally differentiates between growth coalition and regime. Both approaches focus on cooperation between public and private elites. In Urban Fortunes, The Political Economy of Place (1987), Logan and Molotch argue that the politics of local economic development are driven by the vested interests of property owners who seek to realize their economic interest through a progrowth coalition. The emphasis of urban regime analysis on cooperation between political and economic interests in order to develop electoral coalitions and to govern.

Reichl calls for some alternative to corporate control of public space through the "theme-park experience" (p. 179). He believes in the "importance of pursuing democracy and authenticity in urban development" (p. 177), "areas of cultural diversity" (p. 181), and "a more enlightened vision for the future of cities" (p. 181). Reichl does not explain how an opportunity-expanding regime is to be created in New York City, how political support for urban diversity is to be achieved, or how opposition is to be overcome.

In Reconstructing Times Square we begin to see some movement toward integrating conflicting urban policy models. It is hoped that Reichl will continue in the work he started.

Just Institutions Matter: The Moral and Political Logic of the Universal Welfare State. By Bo Rothstein. New York: Cambridge University Press, 1998. 254p. \$59.95 cloth. \$19.95 paper.

Max Neiman, University of California, Riverside

Politics is the serious, sometimes grim, contest over the distribution of advantages and disadvantages. Scholars ought to have some interest, therefore, in the important stakes involved in politics. Yet, somehow, many political scientists have made a professional virtue of disguising the important stakes and have abdicated much of the serious discussion about policy effects to branches of economics and to partisans among ideologically contentious think tanks. That is why Bo Rothstein's audacious statement in Just Institutions Matter is so welcome: "My aim in this book is to unite two incompatible ambitions: speculation and discipline" (p. 1). Furthermore, "in contrast to most social scientific research undertaken today, this book is both openly normative and empirical" (p. 1). Of course, although speculation and discipline are different, they are related in critical ways. Often the point of scholarship is to produce disciplined speculation that incites further scholarship. Just Institutions Matter should be a frequently cited work among those interested in comparative politics and policy and the general issue of policy evaluation.

Focusing on the nature of the modern welfare state in advanced societies, Rothstein tries to do what he claims he aspires to do: demonstrate the interplay among values, policy formation, and policy outcomes. He is largely successful, although his accomplishment is mostly in the theoretical and speculative rather than the empirical realm. Nevertheless, his achievements in this book are laudable. His application of Rawls, through the lens of Dworkin's "principle of equal concern," leads to a rights-based foundation for the universal welfare state, an effort that should be of major interest to scholars working on the formal connection between social welfare principles and policy implications. Some might even find the normative analysis curious. After all, the principle of equal concern might slip easily into a kind of Paretooptimization limit on state intervention. Instead, it is interesting to see how Rothstein arrives at the view that equal concern and respect for citizens produce a universal welfare commitment to proved a wide array of public goods and services, he doe this in a sweeping second chapter, "The Universal Welfare State and the Question of Individual Autonomy."

Rothstein generalizes from the Swedish and Scandinavian experience, which is emblematic of his desideratum of the "universal welfare" principle, contrast with the principle of "selective welfare." The two principles are posited as ideal types and coexist in varying mixtures in all states, with one

usually prevailing. Universal programs entitle virtually all people within the society as recipients, whereas selective systems limit programs, often by some needs-based principle. Rothstein argues that in democratically organized welfare state, the interests of the less-well off and the more affluent converge, even in the face of higher taxes, since these are significantly offset by social harmony and greater levels of consumption through public systems of distribution. Apart from issues of effectiveness, the success of such universalistic programs also rests on the legitimacy accorded them by broad segments of empowered and informed citizens who are in a position to assess and act on the outcome of policy.

In contrast, policies based on selective, means-tested principles are far more complicated to formulate (requiring a determination of who is deserving); are complex to implement (requiring policing and regulating to ensure that recipients are qualified); and incite resentment on the part of those who are not "qualified" for benefits. As a consequence, selective welfare programs are perhaps less effective from an implementation view; they also are more subject to political resistance and less likely to be supported, particularly if nonqualifiers are numerous and politically effective, as is the middle-class in the United States. In the United States the principle of program selectivity is raised to another level. Programs are very selective, organized around particular clients, but those for the poor are delivered primarily through direct expenditures, whereas those for the more affluent and powerful are very often delivered through the tax code. The effect is to obscure the extent of welfare support for some and exaggerate and make more vulnerable the benefits provided to the less well-off, or those who do not file the long form.

Rothstein points out that the universalistic principle has been under stress throughout the world, including in Sweden, as a result of fiscal crises, concern over economic stagnation, worries about welfare dependency, increased infatuation with privatization, and the pressures of global competition. Nevertheless, even when retrenchment in large welfare states is required, the universalistic principle can apply. For example, reductions can be shared broadly rather than borne by certain parts of the society. If the housing benefits for the poor are reduced, should we not also reduce the tax benefits of housing for the affluent, which is how they receive housing support? This issue is implicit in Rothstein's analysis, although he does not dwell on the universal principle as applied to program retrenchment.

Although committed to an interventionist state and universalistic principles to guide program design, Rothstein is not a knee-jerk, uncritical supporter of open-ended public sector growth. His overview and reworking of the implementation literature clearly suggest guidelines for when state intervention is more or less likely to succeed. He makes a strong case that adequate knowledge, political support, and appropriate institutional design are needed for policy success. Good intentions should not substitute for verified knowledge about the problems being addressed. Rothstein proceeds to develop an evaluative framework to specify what the state can do, based on considerations of policy design, organizational/institutional setting, and legitimacy. Each is further elaborated to include such matters as policy substance and a variety of different avenues and techniques for establishing policy legitimacy.

The framework developed in the middle chapters permits Rothstein to compare and interpret historically and crossnationally with respect to support for welfare policies. It also permits him to evaluate critically the neoliberal, public choice orientation toward policy evaluation or implementation, although clearly Rothstein accepts certain aspects of the public choice bottom line, such as the need for competitiveness and choice in public sector service delivery.

The main strengths of *Just Institutions Matter* are primarily analytical and theoretical rather than empirical. Rothstein is eloquent in his argument for universalistic principles to guide social welfare policy, and he is very creative and analytically agile in providing a language and framework for connecting values and policy. Still, the strengths of this book, considerable as they are, will have no real effect unless others are incited to equally committed empirical work.

Interest Groups in American Campaigns: The New Face of Electioneering. By Mark J. Rozell and Clyde Wilcox. Washington, DC: CQ Press, 1999. 192p. \$20.95 paper.

Stephen J. Stambough, North Dakota State University

This book is timely, particularly considering the public debates about campaign finance reform and the growing use of interest group money with independent expenditure campaigns. The authors remind us that interest groups do more than just give money to campaigns and explain this behavior with a fairly straightforward argument. They claim that the activities of interest groups can be understood by knowing the opportunity structure in which groups operate and the goals they wish to achieve. The goals and opportunity structure combine to form a strategic context, a concept that Rozell and Wilcox then use to examine interest group interactions with political parties, political candidates, and voters.

In the first substantive chapter, the authors present a good combination of qualitative and simple quantitative analyses. They demonstrate the types of activities of interest groups in different stages of the election process. Three principal areas of interaction between interest groups and political parties are explored: candidate recruitment and training, party rules and platforms, and financing. The role of interest groups in candidate recruitment and training is undoubtedly growing. but more information on the influence of more open primary laws might be in order. The section about party rules and platforms is good but somewhat overstated. Although they discuss Bob Dole's decision to ignore the 1996 Republican Party platform, the authors give the impression that party platforms have a significant effect. A large percentage of the party platform becomes law after an election, but usually the less controversial sections with a strong level of agreement between the party and the interest groups. A better test would be to see the real influence of interest groups in truly reshaping the party's actions when in government, I was particularly impressed with the section about state and local politics, which presents several arguments and suggestions that can lead to significant future research.

The next chapter examines the relationship between interest groups and candidates. At first I was disappointed because it did not tell me anything new, but upon reflection I believe the authors do a very good job of explaining these activities within a well-developed context. They show the work by groups in providing money in all its various forms as well as in-kind services, and they discuss the role of interest groups in developing candidates. The section that outlines how groups give beyond the normal limit is especially instructive.

The final substantive chapter examines the role of interest groups with the voters. Again, the authors do an excellent job of organizing topics and findings; there are no surprises, but they provide a well-developed framework. Too much emphasis is given to the practice of endorsements, which have little effect on the average voter, and not enough is given to the role of interest groups in initiative elections. Direct democracy has become a more attractive avenue for these groups, most obviously because they can appeal directly to voters in a form of venue shopping. If the elevated branches of government are not receptive to an agenda, the group can go straight to the voters. Groups also use initiatives to show support for issues within selected electoral districts (even if the initiative loses at the state level), as a lobbying strategy. Local initiatives and referenda may be even more attractive to interest groups at the local level. More work is needed in this area.

This is a good, comprehensive examination of the changing role of interest groups in American politics. The authors do a fine job of including the effect of technological changes in all the areas discussed. They also explain the values and issues involved in reform proposals. This book is best suited for undergraduate courses and early graduate classes.

Explaining Congressional-Presidential Relations: A Multiple Perspective Approach. By Steven A. Shull and Thomas C. Shaw. Albany: State University of New York Press, 1999. 224p. \$59.50 cloth, \$19.95 paper.

Glen S. Krutz, Arizona State University

Because Congress and the president reside at the vortex of lawmaking in national government, it is vital to understand their interaction. This book examines multiple patterns in their relationship. It covers four discrete behaviors or dependent variables (presidential position-taking, legislative support of presidents' positions, presidents' propensity to veto legislation, and budget agreement between the branches). To explain these behaviors, the book adopts three perspectives or environments: executive, legislative, and exogenous. The result is an impressively complete and balanced approach to the topic. In the spirit of their balanced approach, the authors even alphabetize the order of the two institutions rather than use the conventional "presidential-congressional relations."

The first three chapters set up the subsequent analyses. In chapter 1, Shull and Shaw review the literature and argue that a broader perspective is needed. In particular, they argue for more analysis of policy formulation to join the volumes of research on policy outcomes. They also preview the four interactions analyzed in later chapters: position-taking, legislative support, veto propensity, and budget agreement.

Chapter 2 presents the theoretical framework. Shull and Shaw explain: "We examine political decisions within the context of a multiple perspectives model that encompasses necessary activities and resources as being important to subsequent relations between Congress and presidents" (p. 38). The authors assume that both Congress and the president use available resources and conduct activities to further their policy preferences. To explain variation in the dependent variables, Shull and Shaw use measures of resources and activities from each of the environments: executive, legislative, and exogenous. The executive explanatory variables are the president's public support, the size of the executive office, and speechmaking. For Congress the explanatory variables are size of committee staff, the party margin in Congress, and the volume of legislation. The exogenous category is grounded in David Easton's systems theory (Framework for Political Analysis, 1965). The authors expect that forces outside the institutions influence presidential-congressional

relations. They use a deficit/spending index and the size of government to tap into this third environment. They also control for policy type (domestic versus foreign) and political time.

Chapter 3 describes the measures and methods used for the subsequent analyses. All the models are aggregated by year. On presidential position-taking, for example, Shull and Shaw examine variation in the number of positions taken by presidents per year from 1949 to 1995. They do not examine why presidents stake out a position on some bills and not on others.

Chapters 4-7 analyze, in turn, the four interbranch behaviors. In chapter 4, the authors tackle presidential positiontaking. They show that it is increasing over time and that unpopular presidents stake out more positions. A larger party margin also leads to more presidential positions. In chapter 5, Shull and Shaw examine legislative support for presidents, which is decreasing. The support model (the strongest of the four) suggests a variety of explanations, most notably the economic situation and party margin. These findings make sense because deficits and divided government are more common in recent times, the period of the decline in support. Chapters 6 and 7 examine veto propensity and budget agreement, both of which exhibit more volatile trends than the other two behaviors. Veto propensity appears driven by party margin and popular approval. The model of budget agreement (the weakest of the four) suggests that popular presidents agree more with Congress.

This is a strong book in certain respects. First, it is thorough and empirically impressive. As the author repeat throughout the book, most other scholars in the field (myself included) test their models on one type of behavior at a time (e.g., legislative roll-call outcomes or confirmation choices). Shull and Shaw analyze four different behaviors with many explanations.

A second major strength is the multivariate analysis employed to explain each of the four behaviors. A helpful methodological appendix answers many of the questions one might pose about the quantitative analysis. This technique improves on the bivariate analyses that Shull used (and many criticized) in his otherwise well-received *Presidential-Congressional Relations: Policy and Time Approaches* (1997). The analysis demonstrates that the same factors that drive behavior at the policy outcome stage do not apply wholesale to other behaviors. This is a significant finding that could have alternatively found its way into a major scholarly journal.

The effort to be complete, however, comes at a cost. Beyond the point of being thorough, the book lacks a clear argument or theory. To illustrate: "The primary thesis is that [congressional-presidential] behaviors are constrained by environments such that neither presidents nor Congress dominate any of the four types of interaction. As we have asserted, influences on each activity are complex" (p. 2). To paraphrase, lots of things explain lots of things. Hence, readers who desire a crisp theoretical hook will not find one in this book. Simply put, Shull and Shaw advance our empirical understanding of presidential-congressional relations more than our theoretical understanding.

A second shortcoming is the operationalization of policy environment, which is an important component of all four models. The authors choose to employ only a dichotomy between foreign and domestic policy. The empirical analysis would be strengthened by observing as well how the phenomena vary across more clearly delineated policy areas, perhaps using the various policy categories employed in Shull's 1997 book.

Despite these shortcomings, scholars of Congress and the

presidency will no doubt find the book valuable. I plan to adapt many of the explanations offered by Shull and Shaw into my own research. I also look forward to further work from these authors; there are many more questions that

could be answered with their impressive evidence. All in all, Shull and Shaw should be commended for moving forward the scholarly literature on presidential-congressional relations.

Comparative Politics

Greener Pastures: Politics, Markets, and Community among a Migrant Pastoral People. By Arun Agrawal. Durham, NC: Duke University Press, 1999. 219p. \$49.95 cloth, \$17.95 paper.

David B. H. Denoon, New York University

This intriguing volume uses rational choice theory at the intersection of political science, anthropology, and sociology to explain the behavior of the Raika shepherds in western India. Why should this matter to political scientists? First, the book is an impressive and clearly written monograph that addresses important topics in the field and demonstrates the insight that can be drawn from a consistent theoretical approach. Second, by providing a comprehensive analysis of the shepherds' behavior, Agrawal shows the advantages of interdisciplinary work. Third, by explaining the choices of migratory groups, Agrawal gives us a window on developments in other parts of the world (central Asia, western China, Laos and Burma, and sub-Saharan Africa), where yearly—not permanent—migrants have important political consequences.

Agrawal focuses on two main questions: Why do the Raikas migrate? Why do they migrate in large groups? In the process of answering these questions, he presents useful generalizations on three related topics: hierarchy, bargaining, and political economy.

Why do the Raikas migrate? Agrawal presents a convincing case that the Raikas move annually because they can increase their real incomes substantially above the level they could plausibly attain if they were sedentary within their home villages. Agrawal concentrates on the village of Patawal (near Jodhpur in Rajasthan) but shows that the behavior of the migrants he observes is similar to that of others throughout western India. Raikas are a lower caste who pursue one of the world's oldest occupations, but they are neither destitute nor at the bottom of the social hierarchy. Raikas own mostly sheep and goats, and in Patawal they have more than 6,000 animals in their flocks.

The Raikas' dilemma comes from their inability to form political coalitions within their home village. If they could get the untouchables and scheduled castes to vote with them, they could gain access to more of the village grazing lands set aside for cattle owned by the upper castes. The Raikas are unsuccessful at forming political coalitions with other lower castes because those below them are so poor and vulnerable that they are unwilling to risk alienating the village's higher status groups. Annual migration solves two problems: it provides fodder for the animals during the dry season, and extra income is earned because farmers are willing to pay for the fertilizer left by the flock.

Why migrate in large groups? The size of the migratory group is determined by both economic and political factors. There are scale economies in shepherding. One agent can negotiate with farmers to have the sheep and goats graze down a field just before planting, and a large flock can predictably cover a defined area in an overnight stay. Also, politically, there is safety in numbers, as the Raikas can assign one leader to pay the appropriate bribes to local police and

government officials as they cross others' terrain. Small groups could more easily be expropriated and intimidated.

What does this tell those of us who do not follow India or the issue of migration? Regarding hierarchy, Agrawal has made a significant advance over Charlotte Wiser's Behind Mud Walls (1971) by showing that neither numbers of voters nor assets are adequate to elevate the Raikas' interests in Patawal. Greener Pastures also provides a more complex explanation of rural poverty than such classic Marxist views as Franz Fanon's The Wretched of the Earth (1963). Hence, in sophistication, Agrawal's work is closer to Pranab Bardhan's "Analytics of the Institutions of Informal Cooperation in Rural Development," World Development 21 (1983): 633-9.

Elaborate bargaining is the centerpiece of Greener Pastures and illustrates that the Raika shepherds are far from operating in a world of pure oppression. In fact, they are involved in frequent bargaining: deciding who should represent them to the outside world, determining how to ward off financial predators, and making group decisions about the prices and quantities of the wool and animals they sell. Since dealing in quantity is an advantage, selecting a clever agent is critical and much debated among the Raikas. Therefore, Agrawal's findings fit nicely into current microeconomic and gametheoretic views of agency and human behavior (e.g., Eugene E. Fama, "Agency Problems and the Theory of the Firm," Journal of Political Economy 88 [1980]: 288-307). Sadly, the Raikas' experience also confirms long-held views about corruption in India, previously analyzed in J. Jagdish Bhagwati and T. N. Srinivasan, India, 1975. The Raikas understand, however, that pay-offs vary with the power and venality of the official, and they develop an intricate set of guidelines for dealing with extortion.

Agrawal has two principal contributions of interest for political economy. He strongly endorses the view of peasants as rational calculators of their own interests (see Samuel Popkin, *The Rational Peasant*, 1979), and he makes a clear distinction between annual migrants, who stay within their home social hierarchy, and permanent migrants, who take greater risks and seek greater rewards (see Oscar Handlin, *The Uprooted*, 1973).

In sum, *Greener Pastures* nicely illustrates how a consistent theoretical approach links many different types of complex behavior. It will be essential reading for specialists on India and rural development, but it is accessible and of interest to a very broad audience.

India against Itself: Assam and the Politics of Nationality. By Sanjib Baruah. Philadelphia: University of Pennsylvania Press, 1999. 257p. \$36.50.

Democracy and the State: Welfare, Secularism, and Development in Contemporary India. By Niraja Gopal Jayal. New Delhi: Oxford University Press, 1999. 289p. \$32.00.

Development and Democracy in India. By Shalendra D. Sharma. Boulder, CO: Lynne Reiner, 1999. 281p. \$55.00.

Sikata Banerjee, University of Lethbridge

All three books stem from a general concern with India's inability to live up to Nehru's promise of a better life for Indians symbolized by his famous "Tryst with Destiny"

speech delivered in 1947 on the eve of independence. The underlying implication of these books is not only that this promise has not been fulfilled but also that the Indian state has been plagued by misguided policymaking, indecisiveness, and a lack of a political will to tackle important problems. The authors' articulate this general concern by focusing on three specific aspects of the Indian polity: the robustness of democracy and citizenship (Jayal), rural poverty (Sharma), and subnationalist challenges to Indian federalism (Baruah).

Niraja Gopal Jayal states: "Citizenship indeed lies at the heart of democracy and has been posited here as its very test" (p. 249). She concludes: "A fundamental shortcoming of Indian democracy is its inauthenticity and unrepresentativeness" (p. 255). By using extremely detailed empirical data on the politics surrounding a famine in the state of Orissa, the issues informing the Muslim Women (Protection of Rights on Divorce) Act of 1986, and the political furor surrounding the Narmada Valley Dam projects, the author makes a compelling case for the Indian state's failure to provide social conditions that enable the "effective exercise of citizenship," by which she means the ability of ordinary folk to interrogate and modify state policy through democratic procedures such as civil disobedience to create a more equitable society.

Each of the three cases illustrates the failure of citizenship in India. In the Orissa case, famine victims' inability to influence state policy delayed emergency aid; in the Divorce Act case the voice and rights of female citizens were overcome by a vocal and powerful male minority feared by the state; and in the Narmada Valley episode the voices of almost a million citizens who would be displaced by the proposed dam were only heard because of the intervention of certain elites on their behalf.

The strength of Jayal's book lies in its amassing of empirical detail. Indeed, given that the chapters on Orissa and Narmada Valley are, respectively, 70 and 85 pages long, some of this detail might well have been edited without detracting from the major arguments. The theoretical chapter also would have benefited from editing. The author does a good job of synthesizing the "state-society" literature, discussing its relevance to India, and applying observations drawn from works by Joel Migdal (Strong Societies and Weak States, 1988), Atul Kohli (The State and Poverty in India, 1987), Bertell Ollman ("Going Beyond the State," American Political Science Review 86 [December 1992]: 1014-7), Adam Pzerworksi (Sustainable Democracy, 1995), and Theda Skocpol ("Bringing the State Back," in Peter Evans et al., eds., Bringing the State Back In, 1985). Some of this theoretical discussion may not have been necessary, since Jayal is not arguing for significant reorientation of political analysis introduced by

Jayal's major argument is that the project of democracy cannot be successful unless most citizens of a state, no matter how disadvantaged, can exercise their rights to limit state power and hold it accountable. Indeed, given this standard, Indian democracy has failed. It seems to me that such a lengthy discussion of the "state-society" literature was not needed to precede such a conclusion. Furthermore, there is no significant theorizing of the author's own except for the observation that the Indian state has failed to provide conditions for most citizens to exercise their voice in the polity. Yet, since in the tradition of Thomas Kuhn (The Structure of Scientific Revolution, 1962) most of us are normal scientists within a given theoretical perspective, this is not necessarily a problem. Jayal's contribution lies in the empirical detail of her work, which buttresses her compelling critique of the limits of citizenship in India, but the study would have been more provocative if Jayal had in her conclusions discussed some ways in which it would be possible to overcome these limits.

Sharma also begins his analysis with a discussion of the state-society approach, but his is more to the point, as it situates the Indian state within this debate as a "weak-strong state" that has been unable to solve rural poverty for several reasons: Policy was implemented in a piecemeal fashion, institutional structures of administration were weak, and "structures are easily penetrated by organized and powerful social and clientelistic forces that then confine the state to operating within an ever narrowing range of choices" (p. 88). This statement both underlines Sharma's rejection of the minimal state intervention, neoliberal solution to poverty for an approach that relies on innovative state intervention in the style of the East Asian states and locates this work in the tradition of Joel Migdal (Strong Societies and Weak States, 1988). Migdal argues that state policies are substantially altered when resources are hijacked by local strongmen. In Sharma's analysis, these are "rural elites" who have "limited the range of options available to the Indian state to institute even mild reformist and distributive policies" (p. 120).

Sharma's contribution lies in his detailed discussion of these elites, who have emerged as a result of what he terms the "politics of new agrarianism," which is based on the activism of middle-sized and/or large peasants who have been enriched by the green revolution and nascent agrarian capitalism. Although they claim to speak for village India, in actuality they promote and defend "landed" interests and do not represent the landless and the marginal farmer. Sharma argues that the state can resist the rural elite's ability to distort distributive policy by strengthening administrative structures in India. He buttresses his proposed solution by comparing India's dismal record in dealing with rural poverty with the new Chilean democracy's success in promoting human development and social programs. He explains Chile's success by pointing to the absence of strongmen like the new rural elites and the presence of strong/disciplined administrative institutions, which India lacks. Like Jayal, Sharma does not produce much theoretical reorientation, but his effective explanation for why the Indian state has failed in its distributive function is supported by original empirical work and an intriguing comparison with Chile.

As in the other two studies, the point of departure for Baruah is also the Indian state, more specifically its implication within subnationalist politics. He uses the emergence of subnationalist politics in the northeastern Indian state of Assam to argue that (1) Indian federalism is very weak because politicians view any sharing of power with regions as an attack on the integrity of India and (2) the constitutional framework of India makes it remarkably easy for the union government to override regional authority. These two factors exacerbated the situation in Assam and facilitated much of the violence there.

Furthermore, Baruah reveals the spiral of oppression hidden in the political desire to construct an ethnically and/or linguistically homogeneous national or subnational community in a multinational context. In other words, such Assamese groups as the United Liberation Front of Assam refuse to admit that although they may make a compelling case for "oppression" when faced with the pan-Indian totalizing vision of the union government, other marginalized groups in this state, such as the Bodos, may offer similar arguments when confronted with the exclusionary and assimilative tendencies of Assamese subnationalism. In order to move away from such endless suspicion, Baruah argues, a renewed federalism based on the Canadian model of charter federal-

ism is needed, "which combines autonomy for states with a strong regime of individual and group rights" (p. 206).

Baruah's point is well taken, but as a citizen of Canada I am not so optimistic about this solution. Canada's federalism has not solved the problem of Quebec subnationalism. A federal government more than willing to negotiate with the province has given it administrative control over immigration, cultural/linguistic policy, and economic development (the three areas crucial to the Assamese subnationalist debate). Yet, the calls for Quebec sovereignty remain, and the Canadian federal government is still viewed by Quebec nationalists as the oppressor. True, Quebec subnationalism has not led to the type of violence Baruah finds in Assam, but it is hard to argue that Canadian federalism rather than other factors—such as a small population (30 million), a lack of economic desperation, the history of being a British settler colony with cordial relations with the empire, and a political tradition that attempts to circumvent confrontation—has prevented this violence.

None of the authors end their study with an optimistic view of the future. Sharma and Baruah do try to provide options that the Indian state may wish to pursue in order to ameliorate rural poverty and renew federalism. Jayal does not attempt to discuss how, given her analysis of the limits of citizenship in India, it would be possible to rejuvenate effective democratic participation, but such an analysis should spawn further study of the issue. In the final analysis, only vigorous citizen participation can facilitate the solutions offered by Sharma and Baruah.

Social Movements and Political Reform in Hong Kong. By Linda Butenhoff. Westport, CT: Praeger, 1999. 134p. \$55.00.

James C. Haiung, New York University

This is an unusual study of the activism of social movements in Hong Kong. Both in the book and in the publisher's blurb announcing its publication, the author is portrayed as presenting an antithesis to the usual claim of "political apathy" in Hong Kong under colonial rule. Using three case studies, Butenhoff endeavors to show how social movements influenced the process of political reform.

The three case studies are the independent labor movement, the nontraditional Christian movement, and the democracy movement, which are analyzed within a social-movement framework. There are introductory and concluding chapters, one chapter each on the three case studies, and two chapters (chaps. 2 and 3) that sketch the background of the social movements and the structural (or political-governmental) parameters within which they emerged.

Butenhoff maintains that Hong Kong during the colonial era did have a "civil society," albeit characterized by elite consultation and indirect participation, in the absence of democracy. This claim is imaginative if not in all cases convincing. The success or failure of her attempt to disprove the apathy thesis turns on her ability to demonstrate the existence of a civil society, as such, which in turn hinges on whether she can prove the necessary links between social-movement activism and political reform.

Only the independent labor movement (the first case study) has historical roots and precursors before 1984, the demarcating line, when the Sino-British joint declaration announced Hong Kong's reversion to China in 1997. In the other two case studies, activism occurred only after 1984 and throughout the 1990s. Butenhoff could have demonstrated a

civil society before 1984 only in chapter 4, on Hong Kong's labor movement, but although she cites instances of labor strikes and unrest going back to the 1920s, she shows no evidence linking labor demands with political reform by the colonial government in response.

Parts of the book provide evidence that some inputs from the public were made during the drafting of the Basic Law, the miniconstitution of the future Hong Kong special administrative region (SAR). References are made to the "activities" of the independent labor movement during the drafting process (p. 48) and to the "involvement in politics" of the Christian churches (p. 80), but the more detailed discussion of political participation are reserved for the democracy movement (chap. 6).

Chapter 3 notes an ensemble of inputs from the public that were juxtaposed to the original British and Chinese proposals (pp. 36, 38–9), punctuating the long debates in the drafting process of the Basic Law (July 1985 through February 1990). But the author does not show that these inputs came from the social movements, as distinct from the wide range of Hong Kong notables who either served on the Basic Law Drafting Committee or were consulted by Beijing.

Even assuming that inputs from the social movements influenced the Basic Law, there is no evidence of a link to political reforms, unless, of course, the making of inputs itself is considered a political reform. If anything, the fact that the public—including those consulted by the Beijing-appointed Drafting Committee, which consisted of 23 Hong Kong residents in the total membership of 59—was invited to participate is more an indication of Beijing's patronage (from the top down), as well as a change in British attitude about public participation after 1984, than of civil society pressures (from the bottom up).

Even if political reform is loosely defined to include influences by Hong Kong residents on the Basic Law and British efforts after 1984 to protect human rights in the colony, one has to give due credit to two extraneous sources. These are British initiative and the 1989 Tiananmen tragedy. The latter steeled the determination of some groups, such as the Christian churches, to seek guarantees for their freedoms after 1997, such as freedom of worship (p. 78). Equally, it galvanized the British to make changes, such as enacting the long-missing Bill of Rights Ordinance (1991) and amending the Societies Ordinance and the Public Order Ordinance, which removed curbs on the right to assemble and restricted the vast (at times arbitrary) power of the local police. The primogenitors of reform were the last two British governors, most especially Chris Patten, who arrived in 1992. The ultimate purpose of the belated British effort to democratize on the eve of their departure was not political reform for its own sake (i.e., to rectify past neglect) but to cushion the Hong Kong people against possible encroachments after the Chinese takeover (see David Chang and Richard Chuang, Politics of Hong Kong's Reversion to China, 1998). If this is the political reform to which Butenhoff's title refers, it was not exactly due to the activism of social movements.

One aim of the book is to gauge how social movements are likely to fare in the new environment. This proves too ambitious an undertaking, as her interviews ended in July 1996. Generalizing from the Chinese disputes with Governor Patten in the early 1990s to the installation of a provisional legislature in 1996, Butenhoff concludes that the civil society she studied "is being dismantled" (p. 105). Events following the completion of her manuscript, including the inauguration of a legislature duly elected in May 1998, have proven her conclusion premature at best.

To sum up, the data presented do not refute the thesis of

political apathy in Hong Kong. The author does not demonstrate the existence of a civil society except during the unrepresentative period of the late 1980s through 1990s. Today, a common problem besetting all political parties in Hong Kong is that those under age 30 are not interested in politics, much less in joining political parties (James C. Hsiung, Hong Kong the Super Paradox, 2000, p. 340). The prevalence of political apathy is unmistakable. The question is not whether a civil society is being dismantled but whether it ever existed. Although the book reads like a Ph.D. dissertation, it provides an interesting theoretical framework that merits the attention of anyone interested in pursuing the study of social movements.

Subnational Politics and Democratization in Mexico. Edited by Wayne A. Cornelius, Todd A. Eisenstadt, and Jane Hindley. La Jolla: Center for U.S.-Mexican Studies, University of California, San Diego, 1999. 369p. \$69.95 cloth, \$25.00 paper.

Edward J. Williams, University of Arizona

Treatments of subnational politics in Mexico and other Latin American countries have been tardy in coming, but the recent literature is heartening. Spurred by the contemporary fashion for political devolution in the affairs of government, scholars have begun to get to this long-forgotten dimension of Latin American politics. Subnational Politics and Democratization in Mexico is a valuable contribution to that emerging literature.

The book contains an introductory essay and 14 selections divided into three parts. The section topics are subnational party organizations and democratization, popular movements and democratization, and the implications of centerperiphery conflicts for democratization. Popular movements receive rather more attention than the other two foci, and indigenous movements predominate.

The volume evolves from a four-year project by a core group of researchers in residence at the Center for U.S.-Mexican Studies, University of California, San Diego. A workshop was convened in Mexico City at midstudy to critique the first drafts.

As are all such enterprises, the result is a mixed product. On the positive side, scholars must welcome whatever they find on subnational groups in a time when their competence is burgeoning. The "damnificados" of the Federal District, the Navista movement of San Luis Potosí, the PANistas of Baja California (and elsewhere), the COCEI in Juchitán, and the Barzón in several parts of the Republic define extraordinarily significant contributions to the evolution of participatory democracy in Mexico. Their inclusion in the volume is, of course, predictable but also quite valuable.

In a somewhat different guise, the introductory chapter, by Wayne Cornelius, also strikes a valuable chord in reminding that devolution contains a mixed message for advocates of authentic democratic participation. The despotic cacique tradition lives on in Mexico, and well-meaning constitutional "reforms" frequently buttress and reinforce parochial tyrants rather than liberate local democrats. The ham-fisted Rubén Figueroas of this world are a long way from idealized Jeffersonian democrats. To make matters worse, narcopolitics adds a special contribution to the forces of state-level tyranny, as the experience of Quintana Roo exemplifies. The devolution of political authority is a double-edged sword wont to suppress as well as liberate.

That theme also forms the focus for a splendid chapter by Richard Snyder, the best of the lot. Beyond political devolution, Synder fastens upon socioeconomic neoliberal "reforms" as making significant contributions to local political authoritarianism. The national deregulation and state-level reregulation of the coffee industry provided an opportunity for authoritarian state governors to strengthen their grip through the imposition of neocorporatist mechanisms and the cultivation of crony capitalism on the state level.

The chapter by Gabriel Torres on the Barzón debtors' movement is a trifle dated and a little loose, but it is chock full of useful insights into the bewildering divisions, contradictions, redefinitions, and coalitions of the several currents and extrapolations of the movement. The Barzonistas form the most potent of the contemporary social movements. Their story has yet to be definitively told, but Torres makes a useful contribution to the literature.

The four chapters on the role of indigenous communities, customs, and traditions in Chiapas, Guerrero, and Oaxaca seem a little too much of an admittedly good thing. To be sure, the indigenous reality defines an important element in the evolving panorama of subnational government in Mexico—and beyond. The aborted San Andres Accords in Chiapas exemplify how indigenous traditions promise to contribute to the parameters of newly defined subnational institutions and processes.

Yet, other regions of Mexico have been significantly more influential in the movement toward decentralization and devolution. For example, the collection makes the occasional bow to Chihuahua, but there is no comprehensive analysis of its contribution to contemporary devolution in Mexico. The case of Jalisco may be a trifle less compelling than Chihuahua, but it is certainly significant in understanding subnational politics and democratization in Mexico. In truth, Mexico's northern and west-central regions have been far more important than the South in the politics of devolution, no matter how theoretically evocative (and romantic) the message of the indigenous South may appear.

The volume also does not offer any measurable analysis of exogenous influences. How soon we forget our dependencista lessons! The North American Free Trade Agreement specifically and the relationship with the United States more generally nudged both presidents Carlos Salinas and Ernesto Zedillo toward the devolutionary option (albeit, in different modes). The Central American conflagration and Mexican decision makers' fears of the contagion wafting north helps explain Mexico City's policies vis-à-vis the southern part of the country. Public and private international bankers urged Mexico to embrace neoliberalism, the economic handmaiden of political devolution.

Finally, in the calculation of positives and negatives, the book boasts a splendid list of acronyms, but it has no index. A list of acronyms is quite useful, but an index is an indispensable attribute of a good scholarly book. The volume is good enough; we should be pleased with it, but it could be much better.

The Logic of Japanese Politics: Leaders, Institutions, and the Limits of Change. By Gerald L. Curtis. New York: Columbia University Press, 1999. 303p. \$27.95.

Regime Shift: Comparative Dynamics of the Japanese Political Economy. By T. J. Pempel. Ithaca, NY: Cornell University Press, 1998. 263p. \$65.00.

Leonard J. Schoppa, University of Virginia

The 1990s were a tumultuous decade for Japan. After four decades of steady economic growth and stable one-party rule by the Liberal Democratic Party (LDP), the economy slowed

to a crawl, and politics went through a series of surprising twists and turns. The LDP split in summer 1993, which allowed the break-away groups to join a coalition of opposition parties that pushed the LDP out of power for the first time since 1955. After staying together long enough to pass electoral reforms, however, the coalition broke up in spring 1994, which opened the door for the LDP to come back into power together with its long-standing arch-rival, the Socialist Party. The LDP did well in the first election held under the new electoral rules in 1996, but it has struggled ever since to maintain its grip on political power in the face of a continuous stream of bad economic news and politics that are much more fluid than in earlier decades.

These books by T. J. Pempel and Gerald Curtis, both of whom are leading Japan specialists in political science, are extremely readable efforts to make sense of these developments. Pempel emphasizes how much change has taken place and argues that the developments of the past decade can be seen as part of a "regime shift" under way since the 1960s. Curtis stresses the "limits of change," as stated in the title. Nevertheless, both books offer arguments with enough nuance (perhaps too much) to account for the mix of change and continuity seen over the last ten years.

Regime Shift argues that the transformation going on in Japan is an example of changes that have taken place at the "middle level" of politics and economics in all the advanced industrialized nations in response to changes in their socioeconomic demography and international environments. At various times in the past, Pempel argues, all these nations developed "regimes"—defined as mutually reinforcing combinations of socioeconomic alliances, political-economic institutions, and public policy profiles—that were suited to the demographic and international circumstances of their day. Because various features of these regimes supported one another, they tended to be quite durable and in many cases lasted for decades. In all these countries, however, trends such as declines in the agricultural share of the economy and rising levels of capital mobility eventually led to tensions among various elements that have produced periodic sharp shifts to new "equilibrium points," where the three elements are once again in harmony. Japan, he argues, is currently going through one of those shifts.

The first half of *Regime Shift* describes how the old regime in place in Japan in the 1960s constituted a mutually reinforcing system that was distinct in important respects from the regimes that grew up in other advanced industrialized societies. After providing brief sketches of the regimes in Sweden, the United States, Britain, and Italy, Pempel shows how the old Japanese regime differed from these: It was based on a social coalition of big business, small business, and agriculture; featured a one-party dominant political system; and pursued a policy of "embedded mercantilism" that propelled Japan's rapid climb up the product cycle through a combination of protectionism and industrial policy while circulating some of the nation's economic gains to inefficient sectors, such as agriculture. This description will be familiar, of course, to many readers.

More novel is the second section of Pempel's book, which describes a thirty-year transition (still under way) from the regime of the 1960s to a new and as yet unformed equilibrium. As have other advanced nations, Pempel argues, Japan has experienced changes in socioeconomic demography and the international economic environment that have put its social coalition under stress and limited its ability to continue the old policy line. In his telling, two forces seem to be most responsible for undermining the stability of the old system. First, agriculture and small business have shrunk to such an

extent, relative to the rest of the Japanese economy, that they can no longer supply the LDP with the votes it needs to stay in power. This forces the LDP to rely on the votes of urban salaried voters, who often prefer policies at odds with those needed to keep the party's traditional supporters happy. Second, growing foreign investment and reliance on foreign markets have led the interests of the export-oriented segment of Japanese industry to diverge from those of import-competing sectors, which strains the LDP's ability to keep both sides happy.

Pempel is convincing in his depiction of the growing stresses in the LDP's support coalition, but he ultimately has difficulty linking these developments to the specific events of the 1990s described at the outset of this review. The problem is that he seems stymied by the contradictory evidence as to exactly how much Japan has changed. For example, he is impressed enough by the evidence of change to describe the nation as having made a "sharp break" from its old policy of embedded mercantilism during the 1990s (p. 146), but just a few pages later he reports data showing how levels of foreign investment in Japan and the share of manufactured imports coming from non-Japanese firms remain distinctly low, to the point that "the old wine of mercantilism filled much of the new wine of internationalism" (p. 149). Similarly, he states at one point that the old regime "collapsed" in 1993 (p. 197) but later admits that political forces advocating reform have yet to establish an effective electoral vehicle for their cause. Consequently, Pempel is able to imagine a scenario for the future in which the current "muddied mix," despite the tensions within the regime, is able to survive for quite some time. One wonders how much analytical leverage can be gained from Pempel's concept if "regimes" are able to survive transitional periods of thirty to forty years in which their elements are in growing tension.

Curtis is less ambitious in that he does not attempt to advance the case for employing a new analytical concept in order to understand what is happening in Japanese politics. His aim is simply to show how formal and informal institutions, as well as the quirks of individual leaders, have constrained the pace and direction of the changes seen during the 1990s. In advancing this claim, Curtis challenges the new generation of political scientists (including his own student Frances Rosenbluth) who have advanced rational choice accounts of Japanese politics. He argues that these scholars, in assuming that all politicians are motivated by nothing more than reelection and that this single concern predictably limits their options, "miss most of what is interesting, and important, about politics" (p. 3). Curtis claims that politicians are complex individuals with mixed motives who frequently have many choices about how to act in a given situation. Their environment, especially institutions, constrain their behavior and point them in certain directions, but they are frequently able to "maneuver to exploit opportunities" (p. 4) in ways simple rational choice models cannot capture.

Curtis is careful, however, to make it clear that just because he challenges rational choice models does not mean he is a culturalist, and he devotes seven pages to making sure that no one misunderstands his position. What interests him are the unique personalities, motivations, and talents of specific individuals, whose choices at key moments have shaped the twists and turns of Japanese politics in the 1990s. Setting up rational choice and culturalist arguments as his foil in this way allows Curtis to regale us with story after story of the infighting and intrigue behind the scenes in Nagatacho and Kasumigaseki (the political and bureaucratic districts of Tokyo) to show how complex individuals shaped political

developments. Curtis had unparalleled access to leading politicians, including the numerous former prime ministers whose interviews are cited in the footnotes, and he offers fascinating blow-by-blow accounts of the LDP's split m 1993, the maneuvering that led to the Hosokawa coalition cabinet that same year, the electoral reform of 1994, and specific policy decisions. In each case, he argues, quirks of the individuals involved, such as personal rivalries among faction leaders, led to outcomes that are difficult to characterize as "rational." Politicians were frequently so focused on short-term "tactics" that they failed to pursue their long-term strategic interests (p. 156).

Although Curtis emphasizes personalities, he recognizes the important role played by institutions in constraining political behavior. He devotes a segment of one chapter, for example, to detailing the little-known rules of Diet procedure that encourage small parties to join together in larger parliamentary caucuses (*innai kaiha*), and he shows how this led politicians to make key decisions that split the anti-LDP coalition in spring 1994. As this example suggests, Curtis is sympathetic to the rational choice argument that institutions create incentives.

In the end, therefore, Curtis's perspective on Japanese politics is probably not as far removed from those of the younger generation as Curtis thinks. He sees enough incentives operating in Japanese politics to use the word "logic" in the title of his book. At the same time, most rational choice scholars are sensitive enough to the complexity of the real world to recognize that few incentives are strong enough to leave politicians with just one choice. They should take Curtis's book as a challenge to begin addressing political situations in which mixed motives and tensions between short-term and long-term incentives create room for political actors to take advantage of their "agency" and make a difference in the world.

The Stable Minority: Civilian Rule in Africa, 1960-1990. By Samuel Decalo. Gainesville, FL: Academic Press, 1998. 344p. \$47.50.

Yomi Durotoye, Wake Forest University

When in 1990 the Beninois inaugurated a revival of electoral democracy in Africa, many observers proclaimed the beginning of the end of both civilian and military dictatorships in the continent, but reversals in Nigeria (1993), the Gambia (1994), Niger (1996), Republic of the Congo (1997), and Cote d'Ivoire (1999), among others, have dampened optimism that the military, in particular, are retreating to their barracks under the onslaught of electoral democracy. At this juncture, the pressing question for many is what Africa needs to do to avoid taking two steps back toward military dictatorship for each step it takes forward toward democracy. Stable Minority, which sets out to examine "the features and policies of civilian leaders and the strategies [that] bond military leaders to postures supportive of civilian rule" (p. 4), offers hope for an answer to this puzzle. Those familiar with the work of Decalo know that he is a significant contributor to the study of civil-military relations in Africa (e.g., Coups and Army Rule in Africa, 1976). Therefore, no one can argue with his proposition that the lacuna in the study of civilmilitary stability (rather than civil-military instability) needs to be addressed. The first paragraph of the opening chapter as well as the title of the final chapter signify that Decalo recognizes that the survival of democracy in the continent is at stake. Thus, one quickly begins to expect Stable Minority to contribute significantly to both civil-military and democratization studies.

The five chapters can be usefully divided into three parts. Chapter 1 develops a theory of civil-military stability. Chapters 2, 3, and 4 are case studies of three of the dozen African countries that in 1990 had never experienced military rule. The final chapter assesses the factors that initiated and currently challenge the redemocratization process in the continent. I agree with Decalo that because the "stable minority" of twelve countries has very little in common, a "search for systemic conditions that act as causes of stability is a false trail" (p. 17). It also validates Decalo's methodological preference for focusing on discrete control policies and strategies. This approach justifiably undermines the persistent tendency to assume that Africans and African countries respond alike to common afflictions.

Decalo's major finding is that the only effective method of subordinating the military to civilian authority in Africa is to neutralize the security forces. Of course, there are many strategies for achieving this control, and Decalo identifies three. The first, the external guarantor modality, speaks to "the existence of external guarantees of military assistance in case of domestic upheavals" (p. 30). Until 1990, France was the only country that assumed such responsibility in Africa, based on bilateral defense agreements signed at independence with its colonies. The agreements were given teeth by, among others, the presence of thousands of French troops in several bases in Africa, a rapid deployment force of 40,000 in Provence for use in Africa, and an oft-demonstrated muscular will to intervene on behalf of clients. Cameroon, Cote D'Ivoire, Senegal, Swaziland, Tanzania, and Gabon are said to rely on this strategy to secure civilian-military stability. The problem is that it is really not an African but a French strategy. Indeed, the end of the Cold War has removed the slight African agency inherent in the modality. To illustrate, while holding out in the French barracks, President Bedie of Cote D'Ivoire lost power to General Guei in a 1999 coup. It was simply not his call to make. Nevertheless, it is ironic that of the three modalities Decalo identifies, the external guarantor is the only testable and sustainable strategy. The efficacy of the other two is impossible to test a priori.

The second broad strategy is the trade-off modality, which "rests on a tacit but visible trade-off of material benefits (to the military as a corporate body, and to officers as individuals) in exchange for political fealty" (p. 36). Decalo admits that although this strategy is theoretically less reliable and stable than the external guarantor modality, it has proven somewhat effective in Kenya and Zambia. As a testimony to its unreliability, attempted coups and conspiracies by the military in Kenya occurred in 1964, 1971, 1978, and 1982 (pp. 237–46). Furthermore, this strategy, which is widely used in the continent by both the civilian and military governments, has in most cases failed to serve the practitioners well. One can only claim its efficacy so long as no coup attempt succeeds, which is little comfort for those who seek reliable ways to keep the military in the barracks.

The legitimized modality is the third broad strategy for maintaining civil-military stability, whereby the military internalizes the concept of civilian supremacy. During the period under study, the six countries that relied on this strategy were Botswana, Gambia (now off the list), Malawi, Mauritius, Swaziland, and Tanzania. Several factors are said to promote the observed systemic legitimacy, including the absence of divisive subnationalities (or where it is present, mitigated by the force of the personality of the founding fathers) and the permeation of traditional values consistent with competitive democracy. Malawi is not, in my opinion, an appropriate case

study of this modality. Botswana might have served the author better. The control policies adopted by President Banda of Malawi have often been cited by incoming juntas as the reason for their coup. The policies include a "network of spies and ancillary structures (including the Young Pioneers)" (p. 40), privatization of political power, regional/ethnic politics, terror, personal charisma, and the appointment of loyal commanders to discourage military rule. The coup in the Gambia in 1994 further questions the adequacy of the policies that constitute the legitimized modality. These considerations lead to the question of why a combination of some selected policies deters a coup in one country but instigates a coup in another. Decalo's claims remain unpersuasive until this question is addressed.

The final chapter examines the sources of the "democratic upheavals" in the continent in the early 1990s and assesses the constraints and challenges of the redemocratization process. The issues addressed are valid but are only tangentially relevant to the main project. The chapter confirms my lingering feeling that much of the analysis in the book is not rigorously deployed to show why there is civil-military stability in the cases examined. It becomes, then, a study of the politics of three African countries that happen to have evaded military rule. Considering the paucity of concise literature on the politics of Gabon and Malawi, the effort is certainly worthwhile. For the most part, however, Decalo's discussion of the politics of these countries does not reveal how such politics uniquely foster the civil-military stability that has eluded dozens of other African countries.

It turns out that the expectations developed from reading the cover and chapter titles of the book remain largely unfulfilled. One does not learn how the nascent process of redemocratization in the continent can be kept safe from praetorian (mis)adventurers. Except perhaps in the peculiar case of countries that rely on France for protection against military rule, the "unstable majority" bountifully deploys such antidemocratic tactics as authoritarianism, bribery, corruption, nepotism, unaccountability, repression, and economic mismanagement. The book asks the right questions, but the reader awaits more rigorous comparative treatment.

Developing Democracy: Toward Consolidation. By Larry Diamond. Baltimore, MD: Johns Hopkins University Press, 1999. 261p. \$55.00.

Problems of Democratic Transition and Consolidation: Southern Europe, South America, and Post-Communist Europe. By Juan J. Linz and Alfred Stepan. Baltimore, MD: Johns Hopkins University Press, 1996. 479p. \$55.00 cloth, \$18.95 paper.

Timothy J. Power, Florida International University

In recent years, the global advance of polyarchy has fostered the integration of comparative politics by encouraging area specialists to look outside their regions and examine the process of democratization in a truly comparative and cross-regional fashion for the first time. More important, the maturation of the literature on the third wave of democratization (which is now older than the undergraduates reading about it) has made possible the emergence of some truly cumulative efforts to make sense of this phenomenon.

The two books under review are easily the best of these new integrative and cumulative efforts. Topically, they are comprehensive introductions to the general phenomenon of democratization, superseding such earlier landmark studies as O'Donnell, Schmitter, and Whitehead's Transitions from Authoritarian Rule (1986) and Huntington's The Third Wave

(1991). Geographically, both works cast extraordinarily wide nets, drawing on cases from three or more regions. Temporally, both volumes focus on the third wave (post-1974), although they occasionally draw on the lessons of the earlier waves and reverse waves.

It is not surprising that both books have similar overarching questions and concerns, given that all three authors have worked together closely, and many of the scholars they cite have cooperated on various projects in the past. Without downplaying the many novel and individual contributions made by each book, it is clear—as the authors acknowledge in their respective prefaces—that these volumes can also be viewed as distillations and critiques of the work of the international network of democratization scholars that has taken shape over the past two decades (and whose debates have been featured most prominently in the *Journal of Democracy* since 1990). Scholarly networking, dialogues, and cumulative refinements of theory are used to spectacular effect in each volume, setting an example for the profession.

Notwithstanding the broad similarities between these two books, there are important differences. These are most visible in the scope and depth of each, in their conceptual emphases, and in the key independent variables upon which they draw to make several important arguments about contemporary democratization.

With regard to empirical depth, Linz and Stepan are more ambitious than Diamond. Their book is divided into four parts; the five-chapter "Theoretical Overview" is followed by three regional sections. Part 2, on southern Europe, features chapter-length case studies of democratization in Spain, Portugal, and Greece. Part 3, on South America, covers Brazil, Argentina, and Chile. Part 4, on postcommunist Europe, examines Poland, Bulgaria, Romania, USSR/Russia, Estonia, and Latvia. Part 4 is the longest section, occupying nearly half the book, and is the most original and interesting, given the ample secondary literatures on the other two regions. Although approximately 80% of the book is devoted to the case studies, that material is impressively well connected to the conceptual introduction, thus bridging the gap between theory and exposition that is often found in such ambitious projects. Linz and Stepan move easily among historical legacies, social-structural contexts, and the play-byplay of contemporary transitions in each country, building a set of case studies of remarkable depth and richness.

In contrast, Diamond does not rely on country-based studies and is more thematic in treating the conditions for sustainable democracy, with a focus on such undertheorized variables as political culture and the relationship between polity size and democracy. Another major difference between the two books is the level of analysis, which for Diamond is global. He is much more interested in democratization as a world-historical phenomenon, asking whether the third wave is over and under what conditions there might be a fourth.

In the analytical and conceptual domain, there are crucial differences in how democratization is problematized, as reflected in the respective titles. Linz and Stepan examine both democratic transition and consolidation, whereas Diamond is exclusively concerned with the latter. Linz and Stepan extend their focus retroactively to the preceding nondemocratic regimes in each case, offering a wide-angle lens on the dynamics of political change. An updating of Linz's classic typology of nondemocratic regimes (chap. 3) figures prominently in the theoretical framework; in fact, this book has nearly as much to say about authoritarianism[s] as it does about democracy. As one would expect, Linz and Stepan's more case-driven approach facilitates this greater historical sensitivity; in some cases their reach extends back

to earlier historical episodes of democracy, which allows them to examine prior experience as an independent variable in explaining contemporary processes, particularly in the lesser known East European cases. Diamond, in contrast, states explicitly that his book "picks up where Huntington's [The Third Wave] left off" (p. xi). Huntington successfully explained how and why thirty countries transitioned to democracy between 1974 and 1990. Diamond, picking up the trail, seeks to know whether these new democracies will be sustainable, that is, whether they will "consolidate."

The concept of democratic consolidation is controversial and contested. Recent critics pronounce it tautological, teleological, and impossible to operationalize. To raise the topic at all is to enter a theoretical minefield, but to their credit, the authors do not shrink from the challenge. Linz and Stepan immediately pose an ideal-type definition of consolidation; democracy becomes "the only game in town" in three major dimensions: behavioral, attitudinal, and constitutional. The use of an ideal type relieves them from attempting controversial "threshold" operationalizations; rather, they analyze the "crafting" of consolidation in each country-case and make considered, qualitative judgments about the direction of change in each of the three dimensions.

Whereas Linz and Stepan dispense with the consolidation debate in their very brief first chapter, Diamond undertakes a far more nuanced and reflective discussion of the concept (chap. 3). His model of democratic consolidation is broadly similar to that of Linz and Stepan but is more formalized. Democratic consolidation takes place in two dimensions norms and behavior—on three levels. The first level is top elites and decision makers; the second is intermediating organizations, such as parties, unions, and interest groups; the third is the mass public. This conceptual scheme yields an elegant two-by-three matrix for ordering the data. Diamond argues that "when all six of these cells show substantial normative commitment to democracy and behavioral compliance with its rule and limits, democracy is consolidated" (pp. 68-9). His discussion of exactly what to look for in these cells is considerably more concrete, direct, and user-friendly than the comparable material in Linz and Stepan. For example, the assertions that mass support for democracy should reach the level of 70-75% in public opinion polls, and that active rejection of democracy should not exceed 15%, may be "arbitrary," as Diamond admits, but at least this brings us closer to a usable yardstick for measuring consolidation.

Unlike Linz and Stepan, Diamond explicitly recognizes some of the recent criticisms of the democratic consolidation concept and thoughtfully rebuts them. He also develops an interesting typology of nonconsolidated democracies, regimes that meet minimal definitions of polyarchy but clearly do not fit his model of consolidation, and illustrates how the external contours of these regimes may deceive analysts. Thus, Diamond's theoretical treatment of consolidation improves considerably on the Linz and Stepan effort, and it is one of the finest available introductions to this thorny topic. Yet, Diamond's more global, conceptual, and future-oriented framework means that his empirical examples are less well developed than the rich, mature case studies of Linz and Stepan.

Each book trumpets a series of independent variables considered crucial to successful democratization. The choices are foreshadowed in the earlier work of these distinguished scholars. For Linz and Stepan, some of the key factors are the nature of the prior nondemocratic regime, legitimacy and the global *zeitgeist*, civil-military relations, effective citizenship, civil society, and the presence of a "usable state." They place considerable emphasis on "stateness," linking this variable to

nationalism and ethnic heterogeneity, most effectively in their treatment of Spain and in their paired comparison of Estonia and Latvia. Diamond agrees with several of these theoretical emphases—particularly on civil society, which merits an entire chapter—but gives much more weight to culture, values, and mass opinion. His chapter 5, on the role of political culture in democratic consolidation, is a tour de force, capable of causing the most committed anticulturalists to rethink their views. This chapter is by far the most empirically impressive in his book, drawing on cutting-edge public opinion research from several continents.

Diamond also gives substantial attention to the transnational dimension of democratization.

Perhaps the most distinctive feature of the third wave is the considerable contribution that international actors have made to democratic development by enhancing the resources, skills, techniques, ideas, linkages, and legitimacy of civil society organizations, civic education efforts, the mass media, legislatures, local governments, judicial systems, political parties, and election commissions in the developing and postcommunist worlds" (p. 272).

His final chapter is devoted to the need to fortify these linkages so as to secure the continued advance of democracy in the coming century. For Diamond, a continuous agenda of democratic reform is as healthy and as necessary for long-established polyarchies as it is for nascent democracies. If both groups of countries can pursue such an agenda simultaneously, a fourth wave might be generated. As Diamond notes, however, the third wave taught us to expect the unexpected.

In summary, these two excellent books represent the state of the art in the study of global democratization. Taken together, they encompass an exhaustive panoply of variables, debates, and empirical referents that should be digested by anyone interested in democratic change. Any shortcomings lie in their minimal attention to the role of macroeconomic policy and performance in achieving democratic sustainability, a lacuna best filled by Haggard and Kaufman's The Political Economy of Democratic Transitions (1995). Both volumes are likely to become classics in the field of comparative politics. Not only will they take their deserved places on the shelves of scholars, but also they may be used quite profitably in the classroom. Problems of Democratic Transition and Consolidation is certainly a must for a wide range of graduate seminars. Developing Democracy, written in a more accessible and practical style, is better suited for undergraduates. Reading Diamond's book as he intended it, in conjunction with Huntington's The Third Wave, would provide students with an unparalleled overview of democratization in the late twentieth century and beyond.

Capitalism Russian-Style. By Thane Gustafson. New York: Cambridge University Press, 1999. 264p. \$54.95 cloth, \$19.95 paper.

Timothy Frye, Ohio State University

Few observers are better prepared to comment on the transformation of the Russian economy than Thane Gustafson. A self-described old Soviet hand, he has spent the last decade analyzing economic developments in Russia both as an academic and as a consultant to some of the largest oil companies in the world. He has traveled widely in the region and has interviewed many of the important players on the Russian economic stage. This background gives him a keen sensitivity to the context in which economic reforms have

unfolded in Russia and access to decision makers at the highest level. Gustafson puts these assets to good use in *Capitalusm Russian-Style*, an excellent overview of developments across a range of sectors in the Russian economy during the last decade.

The book has chapters on industrial privatization, the equities market, the banking sector, small business, crime, the rule of law, the Russian state, and the social aspects of transition. The first three are especially good. Detailed, thorough, and argued with good sense, these chapters recognize the scale of the changes that have occurred in the Russian economy without understating the distance yet to travel. Gustafson notes that a "revolution has indeed taken place in Russia. . . Yet its main legacy so far is a weak state, a private sector deformed by the manner of its birth, an economy stuck halfway between the command economy and the market, and an unsettled ambivalent relationship between state and society" (p. 33).

Three themes recur throughout the work. First, Russia's transition has been very difficult, but the scope of economic change in the last ten years is "revolutionary" and "irreversible." This revolution was brought on by the creation of a money-based economy despite extensive barter among industrial enterprises, the transfer of property from state to private hands by various legal and illegal means, and the collapse of the Soviet state. Gustafson draws on a range of evidence, including the financial crash of 1998, to support his view that economic change has been extensive. He argues that the crash not only laid bare the weak foundations of the new market economy but also "revealed how far Russia as a whole had moved away from the command economy. Russia's very vulnerability to world commodities prices and movements of capital showed how open the economy had become" (p. 16).

Second, the weak Russian state has distorted reform efforts across all sectors. In Gustafson's view the state is too weak and disorganized to provide public goods to support reform, but it is still strong enough to expropriate wealth. For example, the disorganized state has stifled small business by oppressive taxes and regulation, and the central bank has used its regulatory power to enrich itself as much as to bring order to the market. Indeed, Gustafson identifies the reconstruction of the state as a key task for building a market economy in Russia in the twenty-first century.

Third, perspectives that view the transformation either as a transition to a market economy or as a nomenklatura takeover are "true as far as they go, but fail to capture the full sweep of what is happening" (p. 16). Gustafson uses the privatization of industrial enterprises to illustrate his preferred view, a synthesis of the transition and takeover perspectives. Granting that old regime insiders have benefited the most from economic reform in Russia, Gustafson argues that in some cases they are also leading the transition. For example, insiders in many sectors shun restructuring, but in some oil companies and some auto companies they have adapted quickly to the market. Gustafson makes a similar point when recounting the battles over industrial privatization. "Given the circumstances of Russia in 1991-1994, insider takeover was inevitable, and there is little alternative to insider control today. As for the behavior of the insider managers, it is on the whole a reasonably sensible response to an environment that still discourages changes in most (but not all) sectors of the economy, and the mode of privatization is only partly to blame" (p. 38). Such provocative arguments make the book a joy to read but will spark disagreement in

Because the target audiences for the book include academics, policymakers, businesspeople, and Russia watchers of all

stripes, Gustafson must walk a fine line between simplifying the argument for a general audience and making it sophisticated enough to satisfy academics. He manages quite well. For example, the quotes that begin each chapter are typically as insightful as they are entertaining.

On the negative side, there is insufficient attention to politics. Given the close connection between politics and economics in Russia, one might have expected greater focus on the changing political dynamics that shape economic reform. (Those awaiting a tell-all book on the oil and gas industry in Russia, which Gustafson is well positioned to write, will have to wait. Indeed, this book could have given more attention to this sector, given its importance for the economy.) In addition, there is some indication of haste. The book was finished in spring 1999, in the aftermath of the financial crisis of August 1998. This event casts a long shadow, and in places the text reads as if it were written rather quickly. Chapters on organized crime and the rule of law are somewhat less developed than others, perhaps due to their subject matter as much as to the difficulty of writing an "instant history."

Some observers will challenge Gustafson's optimism about the distance that Russia's economy has traveled since 1990, particularly given the events of August 1998. Several chapters include a barrage of dire statistics about the progress of economic reform, but they conclude with a reference to the silver lining behind the dark clouds. Debates typically focus on the proper benchmark rather than the extent of change in the economy. Pessimists declare the reforms a failure because they have not lived up to the promises made by Russian politicians, Russia has fallen behind Eastern Europe, living standards have fallen dramatically, and growth rates have been negative for most of the last ten years. Optimists argue that due to the constraints facing Russia, progress has been about as much as could be expected. Given the influence of rent-seeking lobbies, a large military industrial-complex, extensive spontaneous privatization before the collapse of the Soviet Union, the federal system, and the weak institutional environment, economic reform has not fared badly.

As a longtime Russia watcher, Gustafson implicitly compares the contemporary scene to Gorbachev's era and makes a strong case that the two bear little resemblance. "The distance is truly from one civilization to another" (p. 9). Gustafson proves an insightful and entertaining commentator. The book is an excellent primer on the key players, decisions, and events that have shaped the Russian economy today. For those who have watched the transformation more closely, the book offers some provocative arguments and much common sense. Gustafson has done an excellent job of blending primary and secondary literature on various aspects of Russian reform with his own extensive experience, perceptive anecdotes, and keen insights. Both scholarly and general audiences will find much to like in this rewarding book.

Ideas and Economic Policy in Latin America: Regional, National, and Organizational Case Studies. By Anil Hira. Westport, CT: Praeger. 1998. 337p. \$64.95 cloth, \$24.95 paper.

Javier Corrales, Amherst College

Knowledge is power. Everyone understands this. But how exactly knowledge becomes powerful is less well understood. More specifically, how do the ideas that experts embrace become influential in politics? This is the topic of Anil Hira's highly readable book. The central argument is that "ideas are vital to explain variations in reactions to international

shocks" (p. 6). Hira studies reactions at two levels, the regional and national. To assess the former, he focuses on Latin America's switch in developmental models during the twentieth century: from economic liberalism to early stages of import substitution industrialization (ISI) in mid-century; from ISI to a more capital-intensive import substitution (ISI2) in the 1960s; and from ISI2 to neoliberalism in the 1980s. To assess national responses, Hira focuses on Chile's journey from structuralism (until 1970), followed briefly by failed socialism under Salvador Allende (1970–73), to neoliberalism under Augusto Pinochet (1973–89).

The author's first accomplishment is to convert this commonly accepted sequence of economic models into a mystery in and of itself. For Hira, there is nothing inevitable about this sequence. Nothing dictated that Latin America, at each of these moments, should turn in the direction that it did or that Pinochet would privilege neoliberalism over competing ideologies. A focus exclusively on the role of economic crises, Hira argues, helps explain the timing of some of these turning points but not their direction. A focus exclusively on interest-group or class-based politics also is insufficient, because at the time of each change the dominant coalitions and classes were opposed to the chosen paths.

A complete explanation requires specifying exactly how certain ideas and not others become influential. Hira argues that not all existing ideas have an equal chance of becoming powerful. To prevail, ideas must share certain characteristics, some of which are political in nature. For instance, they must be part of what Hira labels "knowledge networks," which are "communities of organized groups of experts trained in economics who utilize this knowledge to gain a political position" (p. 13). These networks, in turn, must have a common set of political resources: strong leader, loyal followership, strong links to academic institutions or think tanks, and independence from overtly political organizations (parties). Among all these features, the last is probably the most original and possibly the most controversial. Close association with political parties renders knowledge networks less powerful, Hira argues, because it restricts their appeal beyond partisan sectors.

The other characteristic that makes ideas powerful relates to inherent features of the ideas themselves. They must have a well-developed doctrine and must provide answers to pressing policy questions of the time (e.g., how to diminish dependence, restore growth, end inflation). This specific feature is less easy to operationalize than Hira would like and, hence, less easy to test. Incidentally, this list of features is not apparent in the empirical chapters, but it is presented in a summary table toward the end of the book (p. 143).

This set of features is what unites structuralism in the postwar period and neoliberalism in the 1980s, and hence their respective ascendance in Latin America. One of Hira's important contributions—infrequent in studies of the role of ideas—is to show that ideas which do not exhibit these characteristics stand less chance of capturing the state and becoming dominant (e.g., fascism in the 1940s, radical structuralism in the 1960s; neostructuralism in the 1980s). When President Allende selected one of these politically handicapped ideas (radical structuralism), he was bound to stumble.

Despite these accomplishments, Hira's argument will not satisfy the appetites of serious students of the role of ideas. At the very least, Hira should have "talked" more to his intellectual peers, explaining exactly how his framework differs from those of scholars who also theorize about the role of ideas, such as Robert Keohane, Judith Goldstein, Peter Hall, Peter Haas, Patricio Silva, Verónica Montesinos, Kathryn Sikkink, and Jorge I. Domínguez. The parts of Hira's argument that are borrowed from these authors are more evident than the parts that are innovative.

In some ways, Hira cannot account for crucial variations in the degree of idea implementation. As he acknowledges, not all the ideas that form part of prevailing knowledge networks get implemented. At the peak of structuralism, not all Latin American countries embraced it equally forcefully. And within each country, not all tenets of prevailing ideas get implemented equally forcefully. In Chile in the 1960s, for instance, the state chose not to prepare the economy for regional integration, a central tenet of structuralism, and in the 1980s it chose not to implement antitrust policies, a central tent of neoliberalism. This asymmetry in the degree of dominance of various components of knowledge networks is problematic. It makes Hira's argument susceptible to the same criticisms that he levies against rival hypotheses, namely, difficulties in accounting for variations in the dependent variable.

Although Hira discusses alternative explanations, he does not discuss an elementary counterargument: What matters most are "the President's ears," to use Stephen Krasner's phrase. Krasner meant that whatever the president chooses to hear is what becomes influential. The president's biases determine which ideas become powerful. The president selects cabinet members on the basis of ideological affinity, and they in turn populate the bureaucracy with like-minded staff. Thus, when Pinochet appointed superministers such as Jorge Cauas or Sergio de Castro, he essentially rendered their ideas powerful. "Pinochet's respect and trust of the Chicago Boys, however, gave that group a much wider ability to present challenging opinions" (p. 85). If all boils down to the president's predilections and endorsements, then the panoply of factors that presumably render a knowledge network powerful become less relevant.

I am not suggesting that the presidential value hypothesis is superior. In fact, Hira's own framework and evidence could easily be marshaled to rebut it. Hira could have argued more explicitly that there is something about the way in which ideas come packaged that makes them more "pickable" than others. But he does not make this argument explicitly, which leaves the reader uncertain as to where the real independent variable lies.

In the end, Hira wants to think of the relationship between presidents and ideas as interactive. Presidents grant power to experts and their ideas, and these in turn empower presidents. Each actor gains from the other. At one level, this is a creative way to think of the political role of experts. At another level, it is too vague a predictor of the extent to which presidents will align themselves to certain ideas. Clearly, not all experts or ideas become political assets to presidents. Some can at times become political embarrassments.

Despite these problems, Hira's book represents a useful way of thinking about the appeal of ideas without necessarily entering into debates about their normative merits or affinity with the preferences of the population. Few other analyses of the role of ideas, especially on Chile, can claim such normative impartiality.

When the Romance Ended: Leaders of the Chilean Left, 1968-1998. By Katherine Hite. New York: Columbia University Press, 2000. 246p. \$49.50 cloth, \$22.00 paper.

Paul W. Drake, University of California, San Diego

Katherine Hite contributes to the comparative analysis of the redefinition of the Left, especially the Marxist variety, since the end of the Cold War. She studies the salient case of Chile, which has had the most successful electoral Left in Latin America. It has had global repercussions because of the attempt to create socialism through democratic means under

President Salvador Allende (1970–73), his violent overthrow by General Augusto Pinochet (1973–90), the resurgence of the socialists during redemocratization and their participation in the restored civilian government (1990–2000), and the election of the second socialist president, Ricardo Lagos (2000–06). Just as these events influenced leftist thinking around the world, so international changes on the Left affected the Chileans. To trace the evolution of Chile's leftist movement from Allende to Lagos, the author interviewed political leaders from the Socialist, Communist, and smaller leftist parties.

This book also speaks to scholars concerned with political elites and identities. In her discussion of rational-choice and identitarian explanations for the behavior of politicians, Hite faults the former school for failing to illuminate seemingly irrational actions on behalf of collective ideological goals, a common trait among Chilean progressives. She shows that leftists were defined by their traumatic formative experiences and by their social and political networks. She underscores the enduring influence of families, political parties, and historic victories and defeats. And she establishes that the orientations of these elites helped determine the trajectory of Chilean politics.

From about 100 interviews, Hite selected 25 individuals for intensive investigation and 15 as case histories for inclusion in the book. All the interviewees became leaders during the intoxicating Allende experiment, suffered exile under the demoralizing rule of Pinochet, and helped moderate the Left and reconstruct democracy thereafter. Throughout these thirty years, the Allende episode as well as their generational, class, and educational backgrounds continued to shape their beliefs and actions. Despite monumental changes in their country, in their lives, and in their political strategies, these leaders held fast to their early cognitive frameworks and memories.

In terms of their basic approach to politics, they can be clustered in four camps: political party loyalists, personal loyalists, political thinkers, and political entrepreneurs. Hite reveals how their membership in one of those camps molded their political behavior, but she says less about why they fall into a particular camp. She does not really tell us whether that membership was a result mainly of political history and learning or of social and psychological predispositions. For example, it is not fully clear whether certain people became political thinkers rather than personal loyalists because they were more influenced by universities than by Allende or because they were intrinsically more inclined toward intellectualism than personalism.

Hite discovers that party loyalists have been socialized by their immersion in the subcultures of political parties. They continue to dwell on party organizing and recruiting, even though the days of mass mobilization—whether for Allende or against Pinochet—have been superseded by consensus and caution among the ruling elites. Personal loyalists have been conditioned by their attachment to a meamerizing leader, particularly Allende. They promote that leader's vision, regardless of party postures, and they remain relevant in an era when party ties are eroding. Political thinkers have been more infatuated with ideas than with parties or personalities. They have undergone exceptional mutations, principally from Marxist to social democratic programs, and now chart the course of moderation and modernization in the new democracy. The political entrepreneurs have been motivated by pragmatic power seeking. These flexible gradualists and deal makers dominate the highest political offices in Chile

In sum, the political earthquakes from the 1960s to the

1990s left unchanged the attitudes and activities of the political party and personal loyalists but transformed the beliefs and behaviors of the political thinkers and entrepreneurs. The first two groups prevailed during the high optimism of the Allende presidency and the deep pessimism and defensiveness in the years immediately after the Pinochet coup. The thinkers took center stage from the late 1970s through the mid-1980s, when the Left renovated its ideas and its plans for reinventing democracy. From the late 1980s through the 1990s, the pragmatists took charge of engineering and managing the new regime.

Hite presents her material in supple prose. Grounded in extensive research, her interpretations are subtle, nuanced, and cogent. Although more descriptive than theoretical, her autobiographical method and typology, as well as her analyses and insights, are useful for those who want to understand the complex history of the Chilean leftists and of their counterparts in many other countries.

Inflation and Investment Controls in China: The Political Economy of Central-Local Relations during the Reform Era. By Yasheng Huang. Cambridge: Cambridge University Press, 1996. 371p. \$64.95 cloth, \$24.95 paper.

Wenfang Tang, University of Pittsburgh

This well-written and academically rigorous book examines how the central government in China controls local investment behavior and the variation of investment behavior in different provinces. The author begins with an intriguing question: Why did China not have runaway inflation as did other former centrally planned economies that underwent market reform?

There were many reasons to believe that China would experience uncontrolled investment and inflation. It was a centrally planned economy with soft budget constraint. Unprofitable investments were rarely punished, and local officials would benefit from local investment projects (chaps. 1 and 5). In addition, economic decision making was decentralized during the Cultural Revolution and was never fully recentralized in the post-Mao era. Local governments not only enjoyed extensive autonomy in output planning and goods allocation but also gained control over the lending operations of local specialized banks. Local governments also developed other financial levers, such as bond issues, extrabudgetary revenues, nonbank financial institutions, and foreign direct investments. More important, the new tax contract system made the local government the residual claimant of profit (chaps. 2 and 3). All these would lead to increased local investment and local investment interests that diverged from the center. Yet, investment was disciplined, and inflation was under control in China.

The key to this puzzle lies in the political sphere. Unlike the other centrally planned economies, in China the Communist Party never lost its grip on political power, particularly the power to appoint and remove local officials. After 1983, central control over provincial governments even strengthened, as the structure of provincial government became more unified, the length of tenure among provincial leaders was shortened, and the overlap between provincial party and government posts was reduced. Another measure to increase central control was selective integration of local officials into the central government. Those selected through this process developed a strong career incentive to obey the center, even if it meant sacrificing local investment interests (chap. 4). By using biographical data on provincial leaders and provincial investment statistics, the author shows that central control

over local bureaucratic personnel (e.g., bureaucratic integration and shortened tenure) increased local investment compliance with the center (chaps. 6, 7, and 8).

The book bears some similarity to Kornai's well-known discussion of centrally planned economies, especially in the discussion of soft budget constraint and investment hunger (chaps. 1 and 5). Yet, Huang differs from Kornai's approach to reform, which seems to suggest that the root of the problem in centrally planned economies lies in the political system and that political decentralization and democratization should go side by side with, if not precede, economic reform (Janos Kornai, The Socialist System: The Political Economy of Communism, 1992). For Huang, China's success in avoiding runaway inflation during market reform was precisely due to the lack of political decentralization. Huang goes even farther by describing China's system as federalist, with the center in control of personnel decisions and localities in control of financial decisions. The need for the center to rely on localities for financial contributions to fund central projects provided the latter with significant participatory power and made such a system more democratic (chap. 9).

It is timely to read this book in early 2000, when China reportedly has too much savings and stagnant domestic investment. The system the author describes appears too effective to be true; he has underplayed factors, such as lagging domestic capital productivity, that also determine investment behavior in China. When the market reform deepened, profitability became an important consideration for investment projects. Due to the lack of technology and managerial capacity, Chinese firms were unable to generate high investment returns. No one wanted to invest in a market economy under tightened local budget constraints if investment meant losing money. This may be another reason domestic investment was limited in the 1990s in China. The low level of domestic capital productivity could also explain the rapid increase in foreign investment during the same period. Foreign investment, usually accompanied by superior technology and managerial skills, was able to generate higher returns. The problem is not how to control investment but how to improve domestic capital productivity.

The book only focuses on the relationship between the center and the provinces. It does not talk about state-owned enterprises and their relationship with higher-level government. These enterprises are an indispensable link in the web of political and economic organizations in China. Ultimately, investment projects have to be carried out by enterprises. The political and economic incentives for enterprise officials, and the political and economic environments in which the stateowned enterprises operate, are too important to omit in discussing investment policy. This is not to say that the author's model is invalid. In fact, it often fits very well in discussing the relationship between the enterprises and higher-level government. For example, the principal-agent model and the author's description of the central-local relationship can be easily applied to the state-owned enterprises. Personnel decisions are controlled by higher-level government, but directors in these enterprises enjoy a wide range of decisionmaking power in operations, and their incentive structure is shaped both by bureaucrats' career paths and their company's self-interest. A fully developed model of investment behavior and central-local relations thus may also need to include the state-owned enterprises.

Although the highly academic writing style makes it no easy task to read this book, especially for students without sufficient vocabulary in economics, the carefully built model and ample empirical evidence make this one of the most authoritative studies of central-local economic and political

relations in China. I have seen few studies of China that incorporate the two disciplines of economics and political science so smoothly.

The Politics of Economic Restructuring and Democracy in Africa. By Obioma M. Iheduru. Westport, CT: Greenwood. 1999. 184p. \$49.95.

Anthony Tuo-Kofi Gadzey, Auburn University

Whether IMF conditionalities produce more hardship and condemnation than economic growth is a controversy difficult to pin down in terms of concrete benefits and costs. What if there is a third, unintended, positive outcome? In its barest essence, that is the conundrum Obioma Iheduru sees from the implementation of IMF conditionalities in sub-Saharan Africa during the 1990s. The infusion of external capital is said to have produced a new political dynamic, fostering greater democratization in the subcontinent than was otherwise possible.

On the brink of financial collapse, as many as 26 African states were forced to seek IMF loans, and with them came draconian IMF economic adjustment measures. These included devaluation that sharply hiked up prices of manufactured imports, which for countries lacking a manufacturing base means virtually all nonagricultural products. IMF impositions always create clear winners and losers. With their government-guaranteed huge salaries, the ruling, bureaucratic, and military elites hardly feel the pain of economic adjustment. And they are quick to use the IMF's preference for market solutions that preclude state intervention in the economy as an excuse for withdrawing state subsidies that might cushion consumers against increased prices.

That leaves the economically powerless class of farmers, private sector employees, low-paid government workerssome of whom must take pay cuts—and unemployed city dwellers as the clear losers. Traditionally, this class has never shown the organizational ability to resist their rulers' strategy of transferring to them the burden for their greed and power grab, which are responsible for the destruction of the African state in the first place. Not this time. All over the subcontinent, the announcement of IMF impositions was met immediately by spontaneous mass protests. In the past, protests lasted a day or two, but these were sustained for months, even when scores of protesters were killed, until the rulers made major concessions, including withdrawal of the harshest measures and the holding of elections. That ordinary people hitherto unknown for standing up to their leaders should show such organizational discipline and achievement is seen by Iheduru as the beginning of meaningful democratization in the subcontinent (p. 55). By inference, the author sees a linkage among the injection of external capital, subsequent economic growth (pp. 80-7), and political liberation of the people (pp. 88-94), an apparent confirmation of the longheld Lipset thesis linking economic growth and democratiza-

On its face, the theory is straightforward and powerful. Lacking the internal dynamics for achieving economic growth and democratization, Africa can obtain both through a greater infusion of external capital, provided it is channeled through the people to increase their power vis-à-vis government. Indeed, in accepting IMF impositions, rulers were seen by the people as loosening their grip on power sufficiently to open room for them in decision making. Iheduru's focus on how real people live their lives and try to cope with the declining scope and ability of the African state system is a welcome new direction in African development research.

Classical and neoclassical theories predicate African development on the institutionalization of Western democratic and open market structures, but the new emphasis is on people's interests, their skills and capabilities or the lack thereof. Institutions are important but at best are only instruments. Those who use these institutions and how they are used are more important in telling us whether real development progress is being made.

As welcome as that focus is, those who embrace that research agenda must avoid exaggerating the accomplishments of people power in Africa, a standard not met in this book. The proposition is articulated well and backed by empirical analysis, but the author does not convincingly defend the two points critical to confirmation of the Lipset thesis in the sub-Saharan case: (1) whether the injection of IMF capital actually produced economic growth, and (2) whether the emotional demonstrations over time were transformed into permanent forms of broad political participation and democratization.

The IMF loans were borrowed largely to shore up shortfalls in government revenue rather than for investment in new development. Typically, they have not generated economic growth, or even sufficient repayment capacity, so debt service capital must come from cuts in social programs. Iheduru concludes that "foreign capital was not particularly important or helpful in stimulating economic growth in Sub Saharan Africa during the period of this study" (p. 87). The evidence is equally weak regarding the link with democratization. The protest movement showed unprecedented power and potential, but democratization is a process that must be observed over time; Iheduru measures it in terms of the obvious diminished ability of ruling elites to regulate and control political participation during the crisis (p. 89). Instead, we must know what happens to these groups over time. Did they last beyond the protest objectives? Did they spur a new class of African leaders? Did any of these ad hoc coalitions go on to form permanent parties and government? Did government concessions last longer than the time required to appease angry demonstrators? On these questions the book is silent. If recent elections in Nigeria, Ghana, Cote d'Ivoire, and Kenya as well as the ongoing civil wars in Liberia, Sierra Leone, the Congo, the Sudan, Rwanda, and Burundi are any measure, then regime intolerance and elite struggles for power, not widespread inclusive politics, remain the defining forms of politics in the subcontinent, the protest movement notwithstanding.

Reflections on Revolutions. By Mark N. Katz. New York: St. Martin's, 1999. 146p. \$25.95.

Timothy Wickham-Crowley, Georgetown University

Mark Katz's method here is to juxtapose several well-known (e.g., Barrington Moore, Crane Brinton) and less well-known (e.g., Liah Greenfeld and Abdallah Laroui) theorists on revolutions with his own reading of case studies and then assess "how we're doing." There is a freshness (in both senses?) throughout this little book when he assesses the work of previous scholars. The tone is established immediately, when he suggests replacing the older metaphors (such as Brinton's revolutionary "fever") that lie behind our analyses of revolutionary regime shifts with a new one, which I would term "parricide on the farm." This analysis must be read to be appreciated (or to be irritated by); he uses a folksy-agrarian imagery to translate (there is no other term) the big theories of Gurr, Skocpol, and Goldstone. It is all good fun, but I am not sure whether it furthers the study of

revolution in any substantive way, given the lack of currency that Brinton's old fever metaphor enjoys.

Katz's assessment of Moore's Social Origins of Dictatorship and Democracy (1966) is straightforward but is unlikely to convince either those who accept Moore's pathbreaking ideas and apply them critically (e.g., Jeffery Paige, Coffee and Power, 1997) or those who reject Moore's analyses, but on different grounds than does Katz. He says, rather simply, that revolutionary successes and (later?) their failures have transnational influences that alter events elsewhere, and he also asserts that nationalist forces have often been central to revolutionary outcomes. His companion reference to the multiclass forces and coalitions in revolutionary outcomes is now a commonplace in the post-Social Origins literature, however, and is not ignored in Moore's own work, in which, for example, the crucial alliance between the landed elite and a weak bourgeoisie is the key to the fascist outcome.

Katz's analysis of the underappreciated work by Abdallah Laroui is for me a partially welcome resuscitation. Laroui's view (via Katz) is that the petite bourgeoisie are the key leaders of many Third World revolutions that seek at the start to promote "revolutionary socialism"—by the bourgeoisie?!—but create, in their own image I guess, "petit bourgeois regimes" that later succumb, with few exceptions, to "capitalist-roader" (my usage) embourgeoisement. This is certainly a different theory of revolution, yet Katz's revival of Laroui is only partially welcome, for several reasons. I find their use of the term "petit bourgeois" remarkably inconsistent and overly broad, and I am utterly unconvinced that the persons who headed these new regimes were, in fact and in disproportion, drawn from the social class that Marx and other sociologists have identified with small property owners, mainly urban, sometimes rural.

Certain artisan-type actors were deeply involved with historical urban food riots, and so on, in some (N.B.) instances critical to revolutions (e.g., France 1789, Petrograd 1917), an older observation made by E. J. Hobsbawm, George Rudé, Moore (Injustice, 1978), and others. But to say those same actors controlled the new regime is a dog that won't hunt. Indeed, Richard Hamilton and William Brustein have made persuasive cases for Europe that some of these groups (especially the rural ones) are much more likely to be fascists than revolutionary socialists, and of course the older literature (now disputed) puts the social base of fascism firmly among that same petite bourgeoisie ("the lowermiddle class thesis"). By way of final comment, that such regimes succumb to embourgeoisement and capitalist privatization is a theoretical expectation of world-system theory (of which I am not an advocate, by the way), which sees such outcomes as rather likely when socialist movements gain control of states within a world capitalist economic order that the socialists do not control, and before which they eventually

Other authors get their fifteen pages of fame as well. Liah Greenfeld, in a book unfamiliar to me, puts a nationalist sense of ressentiment, especially in its ethnic form, at the heart of many historical revolutionary surges. Katz largely agrees but insists that ressentiment can shift rapidly from one level to another (e.g., its virtual elimination in Italy following Mussolini's defeat). This all strikes me as very reasonable, but the methodologist in my ear whispers two contrarian things. How is this phenomenon being measured? If Greenfeld is assessing and asserting it in the same breezy manner as Katz, then we may have severe problems of measurement validity. Moreover, are Greenfeld and Katz also asserting that high levels of ressentiment will always or with great probability build to revolutionary upheavals and/or that its

absence will mean a low likelihood of revolution? There is no evidence I can see that such cautions were entertained here.

In contrast, I rather liked Katz's (via Ted Gurr) analysis of the successful breaking off of new states, that is, secession, which is more likely since the breakup of Eastern Europe than before, and which is deeply influenced by both international models (pro and con) and also by whether one's own nation has multiple groups that might pursue similar paths if one secedes (e.g., India). Next, Katz provides only a coda for the Jeff Goodwin-Eric Selbin debate-cum-consensus that "ballot boxes are the coffins" of revolutionary movements. He then redirects that discussion toward issues not directly addressed in the debate. His comments about different degrees of democratization, relative and changing levels of democratization by region, and the vulnerability of democracies to any type of coup or overthrow (N.B.: not just a social-revolutionary one) are interesting but are oblique to the issues Goodwin and Selbin were debating.

Finally, Katz's critical commentary on Jeane Kirkpatrick's "Dictatorships and Double Standards" (Commentary 68 [November 1979]: 34-45) is (unsurprisingly) quite effective, given that her piece was quasischolarly and was meant to promote specific policies rather than attempt to interpret a wide range of cases accurately, as Katz shows she does not do very well. Indeed, she could hardly have done it well, since he contrasts her viewpoint with later events (as he fairly notes), and it is thus tested for prediction, not retrodiction.

In the end, the hidden thematic unity of this likeable yet flawed book lies not in its metaphor of murders and farms but, rather, in its insistence that we view all revolutions and revolutionary movements in an international context, which was, and remains, deeply influenced and altered by ongoing, historically specific events that our theories are not very good at predicting.

British Politics in the Global Age: Can Social Democracy Survive? By Joel Krieger. New York: Oxford University Press, 1999. 232p. \$49.95 cloth, \$19.95 paper.

Andrew P. Geddes, University of Liverpool

National social democracy is dead! Long live global social democracy! This, essentially, is the theme of Krieger's book on the challenges for Tony Blair's New Labour government. The challenges of the global age are fairly clear. Krieger maps his vision of British social democracy in a global age, devoid of its historical agent—the organized working class—and with national sovereignty undermined by the internationalization of the economy and European integration. New Labour has, of course, addressed similar issues, but with markedly different conclusions. Krieger is concerned to locate his analysis squarely in the conceptual foundations of social democratic politics, and from it emerges a clear understanding of the changed structural conditions configuring the political space for contemporary social democratic politics.

The consequence is an important book that adds clarity to the often amorphous notion of globalization; then provides three models of social democratic politics (and shows New Labour to accord with none of them); and then examines the effects of foreign direct investment, the reorganization of production, European integration, and the "unmaking of the English working class" on British social democratic politics in a global age. The book's conceptual and theoretical rigor, combined with its concern to analyze the effect of changed structural conditions on social democratic politics, serves as a

useful antidote to the journalistic obsession with "the Blair effect" and the ubiquitous spin doctors.

Krieger's vision of global social democracy has four key elements. All jar with New Labour orthodoxy and would make uncomfortable reading for New Labour's inner circle. First, whereas New Labour's electoral coalition rests on the shifting sands of "middle England" (guided by the quest for a second term in office), Krieger advocates a more direct appeal to core social democrat constituencies via the recollectivization of labor and the development of wage-earner feminism. Indeed, as Krieger notes, it can be hard to identify the social democratic nature of either New Labour's electoral coalition or their appeal to it.

Second, in contrast to Blair's "third way," Krieger postulates a "solidaristic and egalitarian post-Fordist alternative." "To the barricades in defense of a solidaristic and egalitarian post-Fordist alternative" would hardly be much of a rallying cry. The jargon can sometimes disguise the political significance of the issues being addressed. What is of particular importance within the book's rich analysis of the changed organization of production and the effect of foreign direct investment is the attention paid to the role of women in the workplace and the critical nexus among work, family, and social policy. This prompts identification of the anomalous—in the context of the European Union—absence of state-provided child care for working mothers. The British have one of the highest rates of female labor force participation and a dismal child care record.

Third, Krieger argues that New Labour's institutional orientation marked by constitutional reform should be accompanied by economic and political integration in the European Union with membership of the single currency. Devolution to Scotland and Wales (and perhaps to Northern Ireland, but do not hold your breath) is viewed as the most radical component of New Labour in power. Indeed, it is seen as standing in marked contrast to the right-wing communitarianism of New Labour social policies. Yet, even this concern to devolve and decentralize has encountered difficulties, albeit occurring after this book was written. The travails of the Welsh assembly (the New Labour nominee for First Minister was removed after less than a year in office) and the London mayoral shambles (Ken Livingstone defied the party's electoral stitch-up to run as an independent) give the impression that New Labour's "control freak" tendency makes it hard to come to terms with the reality of devolved power. Maybe because of the grim memory of John Major's absence of control.

European integration is perhaps a more problematic aspect of the analysis. Krieger's call for deeper integration appears to view European unification in terms of a rather bland teleological, federalizing progressivism. This despite the disciplinary effects of EMU, the downward pressure on social expenditure caused by the growth and stability pact, and the EU's democratic deficit. Yet, the ascendance of center-left governments across Europe and the EU's marketoriented ethos could offer a propitious moment for marketfriendly New Labour to engage more deeply with the "European project" (if only sterling were not so strong and the Euro so weak). A more minor point, although something of an irritant for a Euro-pedant such as this reviewer, is the frequent reference to the nonbinding Social Charter; presumably Krieger means the legally binding Social Chapter of the Maastricht Treaty, from which the Conservatives opted out and into which New Labour opted.

The fourth element of Krieger's model of global social democracy stands in stark contrast to Blair's moral mission and Christian socialism (mocked as the Reverend Blair in the

satirical magazine Private Eye). Instead, in what I consider the most interesting section of this challenging book, Krieger argues for the development of a politics of recognition to connect identities and interests in a multinational and multiethnic Britain and within a multicultural EU. He develops Gellner's ideas about modular politics to argue that, rather like pieces of furniture, identities can be assembled, reassembled, and amended. This leads to both an identificational modularity and a modularity in repertoires of political action (depending on which identity is used). The problem with this kind of analysis is that it gives the impression that the "British/English/Scottish/black/Asian/whatever" will encounter huge existential crises before they can even get out of bed in the morning. Yet, what it does demonstrate very clearly is that the shifting sands of social solidarities and collective identities no longer give reliable settings from which politicians can set their compasses. The book also demonstrates the long-term social and economic disadvantages of Britain's ethnic minorities and the challenge they pose to an ostensibly social democratic government. It might also have been useful if the book had demonstrated the overwhelming loyalty of Britain's ethnic minority electorate to the Labour Party and how this support has in many ways been taken for granted. Indeed, in Manchester, where Krieger analyzes the Pakistaniorigin population, disputes over selection of candidates for local and national office have provoked significant strife within the Labour Party.

This book is an important and enriching contribution to the analysis of British politics. It eschews the language of "no alternative" to argue that in a global age New Labour can and should seek to craft a distinct form of social democratic politics equipped for a multinational and multiethnic Britain. It deserves to be widely read.

Tocqueville's Revenge: State, Society, and Economy in Contemporary France. By Jonah D. Levy. Cambridge, MA: Harvard University Press, 1999. 386p. \$55.00.

John S. Ambler, Rice University

In this thoughtful, carefully researched study of recent attempts at French economic reform, Jonah D. Levy has some important things to say about basic issues in two fields of research: economic policy and social capital. The central question posed is why decentralization reforms of the 1980s and 1990s have had such meager effects on local economic development. The principal explanation is that reform policies failed to create the resources and incentives needed to promote effective action by local officials and private firms. This theme is developed in careful examination of the effect of decentralization on economic development in two municipalities—Besançon and St. Etienne. In a penultimate chapter, the author argues that the same pattern of reform foiled by a weak civil society ("Tocqueville's revenge") is seen in the fields of industrial relations and financial institutions. The final chapter examines the means by which policies followed in Japan and Germany succeeded in enlisting the support of private groups and associations.

Levy's account of the rise and demise of French dirigisme is a reminder of the fragility of popular economic models in the postwar world. In the 1960s and early 1970s, in the midst of the French economic miracle, observers such as Andrew Shonfield (Modem Capitalism, 1969) admired the Gallic model of state-led economic growth, with its focus on planning, national champions, and economies of scale. By the late 1970s, Levy reminds us, state intervention had come to mean protection of jobs, even at the cost of subsidizing industries

destined to decline. Dirigisme had one last fling in the socialist experiment of 1981-83, before President Mitterrand and his advisors concluded that the long-term success of the Socialist Party required that it prove its competence in managing the economy by loosening state controls. What followed, Levy argues, can be characterized as "associational liberalism," whereby public oversight of economic development progressively devolved to local and regional governments. Yet, decentralization reforms left in place more than 30,000 competing municipalities, a tax structure that gave local governments very limited autonomous resources, and serious confusion over the relative authority of regions, departments, and municipalities. The most common results were wasteful competition among municipalities for new investment, failure to enlist the cooperation of businesses, and, eventually, the reappearance of direct state subsidies to firms.

One of the book's most important contributions is the weight Levy lends to the institutionalists in the debate over whether social capital should be treated as an endogenous or exogenous variable. As do Tarrow and Jackman and Miller, he argues that the strength of local governments and of private associations is in good measure the result of public policy (Sidney Tarrow, "Making Social Science Work Across Space and Time: A Critical Reflection on Robert Putnam's Making Democracy Work," American Political Science Review 90 [June 1996]: 389-97, and Robert W. Jackman and Ross A. Miller, "Social Capital and Politics," Annual Review of Political Science 1, 1 [1998]: 47-73). Unlike Germany—where the cooperation of private and local institutions was supported by federalism, "codetermination" with labor and business, and tax laws that encouraged banks to make long-term commitments to firms—and Japan—where economic policy was formulated and promulgated within the "iron triangle" of business, bureaucracy, and the LDP, with the help of broad networks of suppliers and manufacturers (the keiretsu) -France did little to strengthen local and private institutions in preparation for decentralization, deregulation, and privatization.

Tocqueville argued forcefully that the weakness of French local governments and private associations can be attributed, in large part, to the determination of rulers in Paris, from the old regime onward, to squelch potential opponents in local government and civil society. If one must agree with Tocqueville and Levy that policy has a powerful effect in the long term, there remains the question of how reformist governments can strengthen civil society in the face of powerful resistance from traditional institutions and deeply seated attitudes. As Levy is aware, the decentralization laws of the early 1980s likely would not have been passed had they included forced amalgamation of France's 30,000 municipalities. If the Auroux Law, which created workers' councils, failed to incorporate trade unions, was this not at least in part because the largest union—the communist-led General Confederation of Workers-still suspected that the proper goal was to replace capitalism, not improve it? One partial answer to the "institutions versus culture" debate lies in the time perspective: In the long term, the strength of civil society is powerfully shaped by incentives and disincentives created by public policy. In the short term, deeply entrenched institutions and attitudes may be highly resistant to change, even in the face of reformist governments.

The author offers impressive evidence of the failure of French associational liberalism, at least at the micro level. Clearly, France still struggles to find a stable middle ground between dirigisme and what in France is known as Americanstyle "hyperliberalism." Yet, the presumed weakness of civil

society must be placed in comparative perspective. With its multitude of voluntary associations and modern, efficient firms, France falls closer to Sweden than to Russia on the strength-of-civil-society scale among democratic countries. Moreover, weaknesses have appeared in both of the national models that are suggested as superior to the French. In the 1990s, the Japanese iron triangle produced cronyism, corruption, and recession. The powerful role of banks in controlling German firms now is perceived by the Schröder government as a source of economic stagnation. Even in the United States—Tocqueville's model of a strong civil society—states have engaged in the same type of wasteful competition for industrial investment that Levy deplores in France (Paul Brace, State Government and Economic Performance, 1993).

This is not to suggest that the relationship between civil society and economic growth is unimportant, only that it is complex and subject to change. Whatever the weakness of French civil society, it seems hardly to have destroyed the French economy, which achieved growth rates significantly higher than the EU average in the 1970s, dropped slightly behind from the early 1980s to the mid-1990s, and then emerged as the leader among major European economies in the late 1990s. Although France still has an unemployment rate higher than the European average, it also has a large trade surplus and a level of labor productivity higher than that of the United States. At least at the macro level, "Tocqueville's revenge" seems less than complete.

Values and Political Change in Postcommunist Europe. By William L. Miller, Stephen White, and Paul Heywood. New York: St. Martin's, 1998. 460p. \$75.00.

Vicki L. Hesli, University of Iowa

Miller, White, and Haywood have compiled an exhaustive report on coordinated public opinion surveys conducted in five East European countries. Although most of the surveys were completed in 1993, the underlying value patterns that are identified provide a concise, accurate, and enduring picture of postcommunist political culture. Critical aspects of this political culture include a commitment to equality and state welfare as well as a tenacious but not overly pervasive anti-Western nationalism. These values exist alongside a commitment to populist values and a syndrome of liberalism that includes respect for citizen rights, tolerance for minorities, and a desire for limited/constrained government.

The preface and chapter 3 provide an overview of the methods and organization of the surveys as well as a discussion of the major fault lines that are assumed to divide the publics of Russia, Ukraine, Hungary, Slovakia, and the Czech Republic. The core of the book is devoted to an evaluation of four broadly defined and appropriately subdivided political values: socialist, nationalist, liberal, and democratic. Subdivisions include a careful delineation among external nationalism, centralist nationalism, and cultural nationalism.

The initial presentation of values tends to be overly descriptive, as it relies too heavily on cross-country comparisons of aggregate percentages of support for (or lack of support for) the different value packages. No explanation is provided as to how or why a particular set of values came into being or what significance the different value packages may or may not have for the future of these countries. Most critically from a social science perspective, countries and other sociodemographic subgroups are compared without conducting statistical tests for significant differences across the means of the subsamples. The rough dichotomization of the continuous variable scales is frustrating. Another short-

coming is that composite indicators of values are constructed from items registering low levels of internal reliability (the set of liberal values, for example, has little unity). Thus, the researchers' views of what should and should not characterize value coherence is driving the study, rather than an exploration of the possibility of unanticipated cognitive structures.

This analytical choice contributes to conceptual ambiguity. The authors use the words "sophistication and maturity" (p. 191) to describe a politician's understanding of his/her dual role of both defending and criticizing the state. Yet, "coherence of opinions" is later taken as an "index" of political sophistication (p. 213). In addition, most measures used in the surveys are not only thorough but also innovative (e.g., a question about a personal collection of books is used to supplement the traditional measure of educational level), but some measures still require further extension. The frequency of attending worship services is not a good indicator of the strength of religious identification in the post-Soviet setting.

I also have a few questions about the historical-cultural overviews. In chapter 1, for example, the authors state that the communist regimes of the Soviet Union and Eastern Europe were "committed to the gradual elimination of religion" (p. 38). Yet, except for widely publicized campaigns, the Soviet regime more often turned a blind eye to the continuation of private religious practices and arguably even promoted Orthodoxy during the Great Patriotic War. Another question arises with regard to the typology of political change from below (such as Solidarity in Poland) versus change from above (as by party reformers in Bulgaria) presented in chapter 2. The implication is that the USSR experienced change from above (i.e., initiated by Gorbachev), but clearly Gorbachev had no intention of ending communist rule, and the failure of the economy, which quickly alienated the public, can hardly be categorized as a change from above. In general, the overview of the collapse of the Soviet Union is superficial and lacks adequate references to the work of other scholars who have addressed this

Overall, the important intellectual contributions of this volume by far outweigh any minor quibbles. Concrete evidence is provided to support several important conclusions. With regard to the past, "dissatisfaction with postcommunist regimes extended far beyond purely economic dissatisfaction" (p. 105). Membership in the former communist parties tended to be unrelated to beliefs in socialist values, so membership must be interpreted as a career decision rather than as a value choice. With regard to the present, the discussion of the language, land, and culture divide in Ukraine is meticulous and serves to confirm existing scholarship. Clarifications are also offered of the often confusing correlation between leftist ideology and nationalism in Russia and rightist ideology and nationalism in Ukraine. Other important clarifications include evidence of the relationship between authoritarian values and support for the progovernment party Russia's Choice, as well as evidence that the strong showing of Zhirinovsky's Liberal Democratic Party in the 1993 elections occurred as result of strategic voting by opponents of the regime.

Particularly significant are the distinctions drawn between members of parliament and their respective publics: MPs are "much less committed to socialist values and much more committed to market principles than their publics" (p. 185). MPs are also more committed to electoral principles of representative democracy (p. 211). The implication is that the political leadership was (as of early 1993) the catalyst behind reform efforts and was, in fact, often operating at odds with

the wishes of the people.

Support for private business (rather than state control) is weak throughout the region, but this should not be confused with a lack of support for democratic values. Economic losers tend to be more committed to socialist values, which implies that these values could wane with economic prosperity. In a related and powerful finding, the authors report that opposition sentiment (to the then current government) within each country was dominated by economic pessimism. Also related is the finding that "Russian MPs with socialist values tended very strongly to have nationalist values" (p. 218). To me this implies that socialists still feel threatened by capitalist encirclement. This means that the continuing danger of Western domination actually serves to maintain, into the present, a strong legacy of the communist past. Overall, this study clearly demonstrates that, whether as a result of capitalist encirclement or continued economic depression, commitment to the ideals of communism is not declining but is increasing in the post-Soviet setting.

Moderate and Radical Islamic Fundamentalism: The Quest for Modernity, Legitimacy, and the Islamic State. By Ahmad S. Moussalli. Gainesville: University Press of Florida, 1999. 250p. \$49.95.

Mehran Tamadonfar, University of Nevada Las Vegas

Since the Iranian revolution of the late 1970s and the subsequent rise of Islamist extremism, much has been written on Islam as an ideology and its growing regional and global significance. Yet, very few scholars attempt to link the basic premises of Islamic fundamentalism/"Islamism" with those of pluralism and democracy, as Moussalli does. This is a wellresearched and analytical study of contemporary Islamist thought on such major issues as community, state, ideology, politics, legitimacy, modernity, and science. By relying on a textual analysis of the main works of such thinkers as Hasan al-Banna, Sayyid Qutb, Hasan al-Turabi, and Rashid al-Ghannushi, the author presents a systematic, analytical, and relatively broad discussion of the philosophical underpinnings of the contemporary fundamentalist movements in the Arab world. He underscores the common themes in these perspectives, but he seems to be keenly aware of the divergent, and often contradictory, views on such issues as radicalism, modernity, legitimacy, and democracy. Moussallı suggests that Islamic reformists rely on reason and science to reform Islam. In contrast, the fundamentalists turn to the Quran and Sunnah (the prophet's traditions) in developing either radical exclusivist or democratic-pluralist inclusivist goals and strategies. Moussalli concludes that contemporary Islamic fundamentalist thought generally underscores such doctrines and institutions as tawhid (monotheism), shura (consultation), and yma (consensus). In his view, these doctrines provide the foundations of legitimacy, communal empowerment, democracy, and modernity in fundamentalist thought.

The discussions of Qutb's radical thought, the origins and scope of fundamentalist radicalism, and al-Turabi's notion of individual empowerment are especially informative. Moussalli capably illustrates how these two thinkers, who represent opposite perspectives and differ from mainstream Islamic thought, embrace change and modernity. He accurately and constructively criticizes Western writings that reject the ability of Islam to adapt to modernity and Western pluralism, and he emphasizes the democratic-pluralist nature of shura.

The book fails to address a number of topics adequately and properly, however, and tends to overgeneralize about issues such as democracy, modernity, and legitimacy in contemporary fundamentalist thought. Moussalli's conceptualization of "fundamentalism" is ambiguous and often contradictory. Given its origin and contemporary meaning, the term has very little explanatory value when applied to these thinkers and groups. They tend to refer to themselves as "Islamist," even though that term has its own shortcomings. Moussalli's suggestion that the difference between radical and moderate fundamentalism is simply a matter of method is empirically inaccurate (p. 104). The comparison of Qutb's and al-Turabi's views shows that there are important differences that go beyond method.

Although sporadic reference is made to the two major Shi'i thinkers, Ali Shariati and Khomeini, throughout the book, their ideas are not adequately discussed. Khomeini was the only fundamentalist thinker who succeeded in establishing his own version of an Islamic state, and his thoughts are embraced by the major fundamentalist groups in Iran and the Arab world, especially in Lebanon and the Gaza Strip. An in-depth analysis of Khomeni's thought reaffirms the longheld assumption that many important fundamentalist thinkers do not advocate democracy and modernity. In fact, Khomeini's doctrine of vilayat-e faqih (leadership of the jurisprudent) is clearly elitist. Unlike the thinkers Moussalli studies, Khomeini does not link legitimacy to popular consent and underscores the need for consensus among the clerics. Of course, as a Shi'i, Khomeini cannot "liberate" himself from history, as Moussalli sees in the works of the Sunni fundamentalists. Thus, it would have been useful if the author had paid attention to the influence of sectarian differences on contemporary Islamic fundamentalist thought.

Critical Citizens: Global Support for Democratic Governance. Edited by Pippa Norris. New York: Oxford University Press, 1999. 303p. \$65.00 cloth, \$19.95 paper.

Christopher J. Anderson, Binghamton University, SUNY

Much of what political scientists do can be described as understanding how democracy works and what makes it last. Most would agree that legitimacy is at the heart of a stable democracy. Questions regarding what legitimacy is, how it can be conceptualized and measured, and how it can best be explained have long occupied a prominent place in the discipline. Yet, curiously, the study of why and under what conditions citizens support political institutions, actors, and democratic principles has long remained disjointed. Explanations and empirical tests abound, but few have tried to integrate what we know.

By attempting to take stock of what we know about political support and of the data that exist to study this important concept, Critical Citizens makes a significant contribution to our understanding of citizen support for democratic governance. It tackles the massive literature on political support both in the United States and in comparative perspective. What is more, the contributors analyze support at the individual level as well as cross-national differences; they examine old and new democracies; and they develop and test explanations for changes in political support over time. Critical Citizens is an enormous collective undertaking that produces a rich set of results.

The strengths are several. The book provides a systematic overview of the available survey data that can be used to analyze issues of political support. Moreover, the authors individually and collectively test the most common explanations found in the literature, and they do so from a variety of angles. Also, the book's focus on global coverage allows the authors to test general theories in the most general way

currently possible. Clearly, *Critical Citizens* is a benchmark in the global study of political support.

In addition to these more general contributions to scholarship, the book has some nice features that are useful for a variety of readers. First, it offers an introduction to the field that does a good job of classifying the different dimensions of support and also cleans up some of the clutter in the literature. Second, the chapters are tied together by following a similar framework, which is laid out in the beginning. Third, at the heart of this effort is the recognition that the concept of political support is multidimensional. For an edited volume, *Critical Citizens* is unusually well integrated, and it includes contributions of very high quality that easily could have been published in leading journals.

The book is organized as follows. The first part examines cross-national trends in different dimensions of political support. This is followed by several chapters that test specific theories with the help of crucial case studies. The third section examines specific explanations (political and economic performance, institutions, social trust, and so on) and the extent to which they explain trends in political support in comparative perspective. A conclusion highlights the major

findings.

Part 1 includes chapters by Hans-Dieter Klingemann (global trends), Russell Dalton (advanced industrial societies), and William Mishler and Richard Rose (postcommunist Europe). Klingemann finds that the vast majority of people worldwide support democratic principles and democracy as an ideal form of government. In fact, he reports extraordinarily high levels of support, especially in regions that traditionally have not had institutionalized democracy (such as Africa and South America, where support averages around 85%). The figures are not much lower than in the established democracies of Western Europe, although the performance of democratic institutions is evaluated more negatively in countries that have become democracies more recently. Dalton finds no general decline in support for democratic ideals but an erosion in specific support, such as people's psychological ties to political parties and their trust in political actors.

Taken together, these findings lead to a crucial conclusion that is highlighted repeatedly throughout the volume: The most critical citizens are those who want more, not less, democracy. This stands in contrast to the accepted assumption that dissatisfaction with the performance of political institutions necessarily spells the demise of democracy as a form of government. If anything, the reverse appears to be true. Mishler and Rose, in their chapter on postcommunist Europe, point out that it is difficult to know, of course, what constitutes too low a level of support for a democracy to remain stable. They argue in favor of considering both the level and the dynamics of support, especially in new democracies. Moreover, they point out that it may not be sufficient simply to compare how closely current practices approximate ideals; it also may be necessary to invite people to compare the current system with other actual systems (such as Sovietstyle communism).

In Part 2, Soren Holmberg (Sweden), Dieter Fuchs (Germany), and Richard Rose, Doh Shin, and Neil Munro (South Korea) examine particularly interesting cases that help illuminate different aspects and explanations of political support. Holmberg reports that, compared with the other Scandinavian countries, Sweden has had a much more drastic downturn in political trust and party identification over the past three decades. What explains this unique pattern? He attributes it mainly to differences in political performance. Fuchs, in a conceptually very sophisticated analysis aimed at

examining the congruence of a country's democracy type and people's underlying values, compares East and West German attitudes about government. He makes a compelling case that political support is lower in East Germany, mainly because of a different conception of the ideal democracy. The East Germans favor a socialist democracy, but the fall of the Berlin Wall brought a liberal democracy. This stands in stark contrast to the West Germans, who prefer a liberal democracy and whose values and institutions are thus congruent.

Part 3, which deals with explanations of trends, addresses the relationship between social and political trust (Ken Newton), economic explanations of trust (Ian McAllister), political performance explanations of trust (Arthur Miller and Ola Listhaug), the role of institutions in explaining trust (Pippa Norris), and how changing political values affect people's attitudes toward democratic government (Ronald Inglehart). These chapters demonstrate the wide and creative range of explanations for variation in trust across individuals, countries, and time. Newton reports that political trust appears to be fundamentally different from social trust. Moreover, he finds the latter is actually on the rise across many democracies (with the possible exception of the United States), and political trust fluctuates more than social trust. Perhaps most important, social trust does not seem to translate easily into political trust. McAllister seeks to debunk a common explanation of trust—objective government performance in the form of economic conditions, social indicators, and political performance—and finds that these factors are important, but they have less substantive influence than political culture and historical circumstance. Miller and Listhaug differentiate between the outcomes political systems generate and the political process by which they do so. Their analysis suggests that fairness in both process and output matter to citizens. Thus, not just what governments do, but how they do it, matters. This conclusion ties in nicely with institutional explanations of political support, a question Norris takes up. She finds that institutions (the rules of the political game) matter: Strong institutional guarantees of democracy, moderate multipartism, and majority electoral systems all produce higher support for political institutions. Inglehart links up transformations in people's political values with the study of trust. Specifically, he reports that postmodernization has led to a decline in support for authority but an increase in support for democratic values. Thus, as do Klingemann and Dalton, Inglehart makes the point that trust is lowest among those who want more, not less, democracy.

Overall, the volume is useful for those who want a general and up-to-date overview of the comparative literature on political support, on the available data sources, and on the general state of theorizing in this area. It marks the long road we have traveled and illuminates some of the areas to which we have paid less attention over the years. To be fair, the volume's main purpose is not to break new theoretical ground. Instead, like Russell Dalton's Cittzen Politics (1996), it provides a rich treasure trove for future analyses by mapping the boundaries of where we are. The paperback version is very affordable and is thus ideal for classroom use.

What do we learn? Not all citizens have to support all levels of the political system in order to ensure its stability or legitimacy. In fact, what Klingemann labels "dissatisfied democrats" seem to be the best insurance for a stable democracy. In the current era of global democracy, people support democratic values and principles, but they pay close attention to how well current political performance approximates the ideal. Instead of providing unquestioning loyalty, the critical democrats help democracy flourish by being vigilant. The contributors to Critical Citizens reach a sanguine

conclusion: There is little sign of a crisis of democracy. Moreover, they remind us that democratic legitimacy can only remain high when the gap between the ideal of democracy and how it actually performs is sufficiently narrow.

Nature and Nationalism: Right-Wing Ecology and the Politics of Identity in Contemporary Germany. By Jonathan Olsen. New York: St. Martin's, 1999, 326p. \$15.95 paper.

Peter H. Merkl, University of California, Santa Barbara

Obviously, the topic of this book is important, and there is no doubt that instances of a right-wing ecology can be found in pre-1945 Germany and again since the 1970s. How to define it and the related concepts properly, and hence how to classify and analyze these complex phenomena, however, remains problematic. This book is an ambitious and serious effort, and Jonathan Olsen presents a number of sound and poignant theoretical analyses. But in the end, for lack of a rigorous definition of such terms as "nature," "environmentalism," and the "radical right," the quarry threatens to escape his grasp.

The problems begin with the looseness of language common to German and English; words like "nature," "natural." and "environment" are used in contexts far different from ecology: The nature of Dewey's philosophy, naturally, may be related to the environment in which he grew up. Many of the quotations collected by Olsen from allegedly environmentalist writings and programs of known right-wingers leave doubts. For example, to say that the radical Right expresses the popular fear of "pollution" of the "purity of the Germany body politic" and responds with hostility toward immigrants and multiculturalism (p. 157), or proposes as a solution "a return to nature and her purported laws" (p. 160), seems at first blush tautological or simply right-wing boiler-plate. It hardly signifies, as the publicity for the book asserts, "that parties of the radical right are moving environmental themes to the center of their political programs." We have to define terms like "nature" and question whether this kind of rightwing ecological rhetoric is truly nature-centered or is just nationalistic or ethnocentric. What is environmentalist about "ethnopluralism" or hostility to foreigners?

The author deserves credit for casting his net wide enough to include manifestations of ecological rhetoric in the Nazi regime and its voelkisch literary antecedents. He gets onto thin ice, however, when he seeks antecedents in the nature worship of nineteenth-century romanticism or in every movement critical of Enlightenment universalism unless he can demonstrate that they were also considered on the radical Right in their day. The Enlightenment came in for much criticism from religious and left-wing circles after the French Revolution. With some figures, of course, Olsen succeeds, and the Third Reich did enact massive protection of nature legislation. Yet, there is little compelling reason to consider Heimatschutz (protection of homeland) movements as environmentalist just because they vaguely include nature and landscape among the local features they vow to protect against threats from the outside, before and after World War I, especially refugees and Slavic minorities. Heimatschutz and Naturschutz (protection of nature) are not the same. To define the topic well, we need to narrow the choice to the truly influential combinations of environmentalism and radical Right politics among the multitude of offbeat cults and gurus of every description roaming Germany at that time.

The reawakening of political environmentalism in the 1970s influenced all West German parties, even the radical Right, although its commitment to a nature-centered view

rarely went far beyond lipservice. The Greens were very heterogeneous and included some former Nazis, and "browngreen" gurus, and not a few closet bourgeois nationalists. But they also felt rather distant and alienated from the world of parties and elections, and they only reluctantly formed the required common consensus for political action. For nearly a decade, different and mutually exclusive environmentalist conceptions prevailed among the (mostly red-green) Greens, but it is misleading to attribute the dissension mainly to a kind of missing link at the "metapolitical level" between them and the writers of the new Right, or the more environmentally inclined among them. New Right figures shared the antiimmigrant, antiasylum-seeker attitudes of the moderate Right, particularly the Christian Democratic Union. Olsen's choice of who belongs to the new Right is rather idiosyncratic, and his lumping together of the radical and moderate Right is dubious at best. The confusion mars especially the last quarter of the book, in which the author becomes so absorbed with the German radical Right and antiimmigrant actions of the government that he seems to forget right-wing environmentalism.

Even the examples of Christian Democratic actions against foreign resident cultures seem poorly chosen. A Berlin municipal attempt to ban cookouts by foreign residents in the Tiergarten park on grounds of trash and air pollution is reminiscent of the ban on open fires in environmentally conscious American cities. And a Bavarian government edict against the excessive noise of prayer calls at mosques reminds me of court cases to force Christian churches to curb their bells. It is true that the German preoccupation with noise as a form of environmental pollution may seem exotic to Americans, but are these—and the German devotion to natural foods and health cults—really compelling examples of radical Right environmentalism?

In sum, this book disappoints the reader who expects profound revelations about the secret nexus between environmentalism and radical Right thinking in Germany. It is undeniable that Olsen finds isolated writers and ideas representing both, but the demonstrable connections seem to consist mostly of the dross of right-wing propaganda verbiage. In the world of political reality of mass politics, the overwhelming evidence is still that of two separate movements, nature-centered environmentalism and ethnocentric nationalism, new and old.

Banking on Privilege: The Politics of Spanish Financial Reform. By Soffa A. Pérez. Ithaca, NY, and London: Cornell University Press, 1997. 190p. \$35.00.

Richard Gunther, Ohio State University

This very important book deserves more attention than it has received. It addresses a set of issues of great importance in this era of "liberalization" and economic reform. And it is based upon solid scholarship and a thorough examination of Spanish banks and credit policies over the past century.

Pérez focuses on two key episodes of economic policy change in Spain. The first occurred in the early 1960s, with the great shift from corporatist autarky to policies intended to integrate Spain into world markets. The second occurred during the 1970s and featured abandonment of the interventionist policies that had been used by a powerful planning bureaucracy to guide Spanish economic development during the 1960s. Both are commonly referred to as examples of "liberal" reform, yet they subtly differed from one another, and they had profoundly different implications for the Span-

ish economy, society, and political system. Indeed, one of the broader lessons that can be drawn from this fine piece of scholarship is that generic terms like "reform" and "liberalization" can conceal a great deal of variation. Pérez goes beyond such superficial labels to identify the key actors, their objectives, their strategies, and the important macroeconomic consequences of implementation of these strategies. She concludes that it is an error to assume that such processes can be adequately explained as if they were direct products of inexorable global forces and international market pressures. She convincingly demonstrates that domestic social, political, and economic processes have an important influence on how seemingly global trends unfold in any given country.

The key actors in this drama are the Big Seven banks of Spain, in cooperation with a group of economic "reformers" trained in the Research Service of the Bank of Spain under the Franco regime. Pérez traces the origins of the banking oligopoly back to the early twentieth century and carefully documents how dominant it had become: By 1957, for example, the Big Seven held 72% of total bank deposits. A second set of key actors appeared on the scene in that same year, and by 1959 they had secured sufficient control over the Franco regime's economic policy processes to launch the Stabilization Plan that dismantled corporatist autarky and integrated Spain into the world economy. But these neoliberal technocrats, most of them belonged to Opus Dei (the elitist Catholic lay organization), were more diverse than they appeared to most outside observers. Initial leadership fell to the development planners, led by Laureano López Rodó, and a series of four-year plans helped spur an unprecedented decade of economic growth. They were interventionists at heart and used the apparatus of the state to reach an initial accommodation with the big banks that both preserved their oligopoly and kept credit rates low. These cheap-credit policies had two purposes: Stimulate investment in selected areas, and preserve social peace in the aftermath of an unprecedented wave of strikes and labor mobilization that emerged in reaction to some of the harsher aspects of the initial period of stabilization.

This neoliberal technocratic elite included a tightly knit faction of "reformers," who emerged from the Research Section of the Bank of Spain. They were, by ideological conviction, opposed to state intervention and policies designed to keep the cost of bank credit low. With the fall from grace of the planners, the reformers progressively assumed control over the formulation of economic policy, despite temporary setbacks, such as Franco's blockage of a proposed tax reform in 1973.

These reformers retained control over economic policy following the transition to democracy and the coming to power of the Unión de Centro Democrático (UCD) in 1977. But democratization created a new set of opponents to the reformers; the "politicians" surrounding prime minister Adolfo Suárez were much more concerned with stabilizing and consolidating the new democracy than they were with liberalizing the economy, and they saw looser monetary and fiscal policies as a means to ameliorate social tensions at a time of worldwide recession. This set up a head-on conflict between Suárez and the politicians, on the one hand, and the banks and central bank reformers, on the other. The prime minister intensified this conflict when he attempted to break the monopoly power of the Big Seven by opening up the country to foreign banks. Although the government issued a decree to that effect in 1978, Suárez and his collaborators were forced to include provisions that "imposed such heavy restrictions on [foreign bank] operations that it made it impossible for them to exert the competitive pressure that would have tangibly changed the credit conditions faced by Spanish businesses" (p. 127). In the end, Suárez effectively lost this battle, as well as control over his party. Indeed, he attributes much of the blame for his fall from power in 1981 and the ultimate collapse of his party to the animosity of the banks that his market-opening initiative provoked.

The most surprising development occurred following the election of a majority socialist government in 1982. The Partido Socialista Obrero Español (PSOE) had, as recently as 1978, called for nationalization of the banking sector. Yet, when it came to power, it appointed to the key economic policymaking positions the same central bank reformers who had emerged in the Franco era. "These appointments not only affirmed the policy leadership of the central bank but established it in a more unequivocal fashion than ever before.... The identification between these new cadres [heading the finance ministry] and the central bank . . . was so strong that one can speak of a veritable colonization by that institution of the upper ranks of the economic policy bureaucracy" (p. 138). They continued the 1970s policy of deregulated credit, combined with an accommodative partnership with the big banks. Indeed, the socialist government not only allowed the Big Seven's oligopoly to continue but also took steps to preserve that position until well into the mid-1980s, continuing to impose sharp restrictions on the ability of foreign banks to compete and strongly protecting the banking sector for seven years from the effects of European Community membership (while subjecting other economic sectors to the full blast of EC competition).

The consolidation in power of these reformers had several negative effects on the Spanish economy and politics, aside from the increased conflict inside the UCD noted above. By eliminating the credit controls that had enabled the interventionist planners of the Franco regime to keep real interest rates low during the highly expansionist 1960s, while at the same time preserving the oligopoly of the Big Seven, the neoliberal reformers made it possible for the banks to increase real credit rates drastically, which guaranteed for themselves profit margins that were much higher than for banks in other European countries. Maintaining tight-credit policies in the aftermath of the twin oil shocks of the 1970s, as well as during the early stages of the restructuring of heavy industry and the parastate sector initiated by the PSOE in 1983, moreover, plunged the country into a protracted recession, and unemployment rose to more than 21%. Only European Union integration put an end to these harsh economic circumstances. In the meantime, electoral support for the socialists and even the organizational basis of the PSOE were damaged by the political fallout.

This book has several important implications for studies of political economy, especially the effect of economic policy on politics. First, it is a mistake to take such catch-all terms as "liberalization" and "reform" at face value. It is necessary to break down these generic concepts into their components and examine their separate effects on politics and the economy. Indeed, the "liberal" policy of deregulating credit was accompanied by the preservation of a very imperfect market structure, in which seven banks controlled more than 70% of bank holdings as well as 70% of the capitalization of Spain's unusual stock market. Thus, as Pérez demonstrates, credit deregulation combined with oligopolistic control of financial and capital markets led to high rates of interest that exacerbated a long recession, increased social tensions, and contributed to the erosion of working class support for the socialists.

Second, this book suggests that the rhetoric (if not the

ideological commitments) of economists ought to be more critically examined in terms of whose interests are being served by a particular set of policies. The central bank reformers projected an image of "virtually unquestionable expertise" (p. 139), and disparaged alternative cheap-credit policies as irresponsible and demagogic preferences of "politicians," but they never discussed the dramatic incomeredistributive consequences of such policies, which enormously enriched the banking oligopoly. These implications suggest that political science might benefit from the rebirth of a "sociology of economic knowledge."

Finally, Pérez concludes that the seemingly worldwide trend toward liberalization of economics does not necessarily imply that global economic forces constitute an adequate explanation for these developments. Domestic social, political, and economic factors may lie at the heart of the way in which "liberalization" unfolds in a particular country, and these national differences may have more substantial social and political influence than the global trends themselves.

The only shortcoming of this book is that, even though it is well written, the narrative is often exceedingly dense, and its vocabulary and excessively succinct invocation of complex economic concepts may make it a difficult read for those not deeply emersed in the political economy literature. Nonetheless, it is a fine piece of scholarship that makes an important contribution. It also provides an important missing link that helps make sense of previously unsolved puzzles and anomalies concerning the Spanish transitions from authoritarianism and corporatist autarky to democracy and a market economy integrated into the European Union.

Third World Environmentalism: Case Studies from the Global South. By N. Patrick Peritore. Gainesville: University Press of Florida, 1999. 328p. \$49.95.

Kathryn Hochstetler, Colorado State University

A major debate about environmental politics asks about the sources and nature of environmental attitudes: Which individuals or groups of people place a high value on the quality of the natural environment, and why? How do those environmental values map onto other political and cultural values? The survey-based studies by Ronald Inglehart (The Silent Revolution, 1977; Culture Shift, 1990) have provided a prominent (and much debated) set of answers to those questions: Affluent individuals and nations come to hold environmental values as part of a whole set of postmaterial values related to quality of life and personal expression issues. Other sociological and economic perspectives (pp. 30-1) also argue that environmental concern comes only after economic needs are met. Such assertions have a hard time accounting for the emergence of environmental values in what Peritore calls the Global South.

Peritore provides new evidence on the existence and nature of environmental values among the environmental elites of the Global South, identifying seven clusters of environmental value profiles that are present in varying combinations in Brazil, India, Iran, Korea, Mexico, Puerto Rico, Romania. These seven countries are not representative of the Global South in any systematic way but do cover quite a bit of the globe, although no African cases were completed. A chapter is devoted to each of the seven, with sections that summarize their major environmental problems and policies in addition to Peritore's new data on environmental attitudes. In each country, Peritore and/or his research associates applied a Q-method protocol to elites in business, government, and environmental groups. The Q-method is explained

in the first chapter, and a 35-page appendix lists all the Q-sort data. Briefly, this somewhat unusual method asks respondents to sort qualitatively derived statements on a given issue—in this case, 241 elites organize 24 viewpoints on the environment—by their degree of agreement with the statements. Factor analysis then identifies enduring attitude types that emerge in the ways the respondents organize the statements.

In what Peritore calls the book's major finding (pp. 12, 243), he documents that postmodern/post-Fordist environmental value types are very strong among many Third World environmental elites. In sharp contrast to the North, that concern is much more likely to emerge as a value on its own in the South, without being linked to the whole package of postmaterial values that Inglehart and others identify. The value analysis shows broad areas of agreement among the various elites, including across sectors, which leads Peritore to suggest that "the environmental new politics could, paradoxically, come more easily in the Global South than in the North" (p. 244). Peritore, as do others who focus on attitudes and values, does not examine the ways that attitudes link to actual behaviors and policies, however, and the many environmental problems and stillborn environmental policies and institutions documented in the country chapters suggest some reasons to be skeptical about his hopeful suggestion.

The seven country chapters are generally successful. Scholars familiar with the environmental politics of individual cases will not find much new in the summaries of environmental problems and policies, but this study offers genuinely new data on elite environmental attitudes, which has been lacking. In addition, few readers are likely to be familiar with the environmental politics of all seven cases. There are inevitably lapses and gaps in a book that is this empirically broad. For example, the discussion of Brazil's environmental problems omits most of its significant urban and industrial problems, and the Iranian chapter is thin in its discussion of environmental politics and policy.

The more serious flaw of the country chapters is that Peritore does not develop the comparisons among the cases as well as he might. His overall aim is to present a composite picture of "third world environmentalism," and he goes so far as to assert that the political culture concept is irrelevant for understanding the development of environmental values in the South (pp. 36, 232). This argument leads him to overstate the similarities among the cases at the expense of their equally clear differences. For example, the attitude types Greens, Sustainable Developers, and Postmodern Managers are strongly present in Brazil, India, Korea, and Mexico but virtually absent in Romania and Iran (although the latter has a few persons who hold Sustainable Development views) (p. 8). The India chapter, which deviates from the others by including quite a bit of country-specific information, shows that the influence of the global economy and international ideas like environmentalism continue to be mediated by national political structures and ideas, rather than obliterating them, as Peritore asserts. Although the overall effort to identify and analyze nonnorthern environmental values is laudable, the attempt to shoehorn so much diversity into a single "third world environment" phenomenon is misplaced.

The country chapters are sandwiched between three chapters that map environmental values onto broader political values. Peritore graphs the attitude types onto two axes, one that extends from modernism to postmodernism, and one that extends from Fordism to post-Fordism. Descriptively, these axes help understand the factorial space occupied by the various attitude types. The overall mapping of attitudes (p. 240) supports Peritore's contention that elites in the

Global South are moving with the North from modernist Fordist attitude spaces to postmodern and post-Fordist spaces—although, as noted above, this generalization overstates the degree of movement in some of the countries. Unfortunately, Peritore also uses these chapters to attack postmodernism as a philosophy, dismissing it in florid terms as "an attitude, a pose, hip sophistry" (p. 22) and "the decay product of avant-guard leftist modernism" (p. 228). Peritore's strongest case against postmodernism is that it cannot make its arguments about the importance of discourse given the "massive biological evidence" of environmental reality (p. 228); the presence of real and direct environmental threats is his explanation of the origins of environmental concern in the South (pp. 30-7). Yet, there is no direct correlation between level of threat and level of environmental values in his study. We are told, for example, that Iran's environmental crisis is among the most critical in the world (p. 209), but in this study Iranian elites are among the slowest to adopt environmental values. Evidently, environmental threats are not only "real" but also interpreted through the values and attitudes that are the subject of this book.

Political Paranoia: The Psychopolitics of Hatred. By Robert S. Robins and Jerrold M. Post. New Haven, CT: Yale University Press, 1997. 366p. \$30.00.

Philip E. Tetlock, Ohio State University

This book offers an insightful and quite compelling blend of psychological, especially psychodynamic, theory and historical analysis. Indeed, many of the historical anecdotes alone more than justify the price of admission. Joseph Stalin's letter of condolence to Eleanor Roosevelt provides a fascinating glimpse into the mind of one of the most influential men of the twentieth century as well as into the quirky psychologic of paranoia. After expressing sympathy, the Soviet dictator went on to imply that FDR had been poisoned and offered his assistance in any investigation that Eleanor might wish to conduct. Robins and Post wrily comment: "Apparently etiquette in Stalin's Kremlin included suggesting the possibility of murder to a grieving widow and encouraging her in finding the perpetrator" (p. 2).

The authors work with a standard, seven-component definition of paranoia, so standard that paraphrases of the definition might well suffice in legitimizing mental-health reimbursements from an insurance company. The definition includes the suspicion, without sufficient basis, that others are exploiting or harming one; preoccupation with unjustified doubts about the loyalty of friends or associates; reluctance to confide in others because of unwarranted fear the information will be used maliciously against one; the reading of demeaning or threatening content into benign remarks; the bearing of grudges indefinitely; low thresholds for perceiving attacks on one's character or reputation; and recurring unjustified suspicions regarding the fidelity of a spouse or sexual partner.

Clinical diagnosis is, typically, an exercise in mapping complex and ambiguous stimuli into fuzzy, overlapping sets. And diagnosis is especially hard when the diagnostician lacks direct access to the assessee, and the assessee is already the target of intense political passions (in the current cases, almost always outrage). Obstacles to the side, however, the authors make a powerful case that a common paranoid streak runs through such disparate political characters as Hitler, David Koresh, Pol Pot, Lyndon Larouche, Shoko Asahara, Idi Amin, Elijah Muhammad, and David Duke. In the process, the book avoids one of the most frequent and often

quite justified criticism of psychobiography, namely, that it falls prey to the post hoc ergo propter hoc fallacy. The authors recognize that a paranoid personality cannot automatically be assumed to be behind every paranoid political strategy or act. Joseph McCarthy does not, for example, fit the prototype well. Of course, skeptics might still insist that Robins and Post implicitly exaggerate the statistical connection between paranoid personalities and acts as a result of cognitive biases, such as hypothesis-confirming search strategies, illusory correlation, and the fundamental attribution error. But these skeptics are likely to be mollified only by the implementation of a double-blind research design in which personality assessors independently rate the life history and personal behavior of a representative sample of political figures, without knowledge of those figures' political actions and without knowledge that hypotheses about paranoia (or any other personality attribute) are being tested.

The authors also recognize that paranoia can, in many situations, be decidedly adaptive. It is no coincidence that paranoia has been called the politician's disease. In an uncertain world, one must balance the likelihood and seriousness of making Type I errors (concluding that trustworthy people are out to get you) and Type II errors (concluding that people who are out to get you are trustworthy). Many politicians believe that the former error is both more common and more serious, so it is prudent to maintain a low threshold for being suspicious of others. To be sure, one could imagine a book that focuses on the opposite error: excessive gullibility, trust, and naiveté. Chamberlain at Munich comes quickly to mind. But I suspect it would be a thinner and less interesting volume.

Although the analytical spotlight is on individual personalities, the authors rightly insist that some social orders are manifestly more receptive to paranoid appeals than are others. In analyzing such diverse cases as the Cambodian genocide and the Salem witch trials, the authors do a fine job of delineating the conditions under which groups are especially likely to become victims of collective paranoid wrath. Collective paranoia needs to be culturally plausible (victims are chosen within a complex of well-established traditions of belief and are chosen only if they possess at least some characteristics that partly fit the profile of the paranoid delusional system) and economically advantageous (victims tend to be chosen from those whose death or humiliation can symbolically or materially enrich the accusers). Once again, there is reason in this madness.

Finally, the authors steer clear, by and large, of arguably the most common pitfall in political-psychological analysis: the tendency to coopt a psychological vocabulary to belittle or stigmatize points of view that one find objectionable. Calling someone paranoid, or a victim of groupthink, or a symbolic racist is not likely to be confused by many for a compliment. Unlike many of their political psychological colleagues in the early 1980s, Robins and Post do not, for example, classify Ronald Reagan as either a paranoid personality or an agitator of paranoia when he labeled the Soviet Union "an evil empire." This is probably wise. Most observers today would deem it a professional embarrassment to label Winston Churchill a paranoid for his prescient characterization of Nazi Germany in 1933 as a "gangster state." But these examples also illustrate the inherent ambiguity and tendentiousness that surrounds attributions of paranoia. Robins and Post define paranoia as extreme suspiciousness of others that lacks any justifiable basis. Assessing the justifiability of a suspicion in any given situation can, of course, be a tricky political as well as psychological judgment call. Rational choice theorists will be inclined to see a prudent trade-off between Type I and Type II errors, whereas psychodynamic theorists will be inclined to see "pathology" and "hiss"

Overall, my differences with the authors are relatively minor. My hunch is that they, at times, may exaggerate the tightness of the statistical connection between paranoid personality and paranoid political action (something only to be resolved through a double-blind design of the sort sketched above). And I am somewhat more inclined to rational-actor interpretations of certain decisions that the authors are inclined to assimilate into a psychopathological framework. Indeed, I wonder whether political leaders who are sufficiently self-aware and socially skilled to disguise their "paranoia" should be placed in the same analytical bin as the criminally insane paranoids who are the bulk of the database for current psychiatric knowledge of the syndrome. But these are differences of degree, linked to methodological style and theoretical temperament. Readers who seek a well-written and thoughtful overview of how paranoia can insinuate itself into the body politic now have a wonderful resource.

Learning Democracy: Democratic and Economic Values in Unified Germany. By Robert Rohrschneider. New York: Oxford University Press. 309p. \$72.00.

Kathleen Bawn, University of California, Los Angeles

Robert Rohrschneider recognized that the natural experiment created by German reunification provided an outstanding research opportunity. He used this opportunity to explore important questions about how citizens' core political values relate to their institutional environment. The resulting research contrasts East and West German attitudes about basic features of democracy.

The theoretical framework juxtaposes two competing perspectives, institutional learning and value diffusion. The former implies that one's political attitudes are shaped by the institutional environment. People who live in well-functioning democracies support democratic values (rights, pluralism, tolerance). People who do not experience successful democratic institutions do not develop these values. The diffusion perspective, in contrast, argues that ideas travel relatively easily from one political system to another, especially given modern communication technology and high education levels. People can (and do) incorporate lessons from secondand third-hand exposure to successful political systems into their own values and beliefs.

Rohrschneider does not see the two perspectives as mutually exclusive. Rather than ask which perspective is right, he focuses on which values are most likely to diffuse across political systems—or at least change quickly in the face of institutional change—and which values will continue to reflect the person's lifelong institutional environment. He argues that, in the context of Eastern Europe, the relevant characteristics are (1) the extent to which the idea requires democratic restraint and self-reliance and (2) whether the idea requires a major or minor revision of socialist beliefs. Ideas that require little of either are most likely to diffuse. Ideas with high requirements on either dimension will be shaped by institutional learning and thus are slow to change. Perhaps a more general formulation of the two criteria would be that values best diffuse when they make relatively low demands on the individual (require little in the way of restraint and self-reliance, to cite but two examples) and when they are relatively easy to reconcile with the overall value system learned from the institutional environment.

Rohrschneider considers five categories of basic values: (1)

liberal-democratic rights, such as the right to publicly oppose the government; (2) democratic ideals, a category that in part addresses the relative importance of fair outcomes (social equality) and fair process (legitimate representative institutions) and that also contains values having to do with representative versus direct democracy; (3) political tolerance; (4) pluralism; and (5) economic ideals, primarily one's attitude toward market competition. He argues that only liberal-democratic rights meet the two criteria for value diffusion; these do not require significant democratic restraint or major revision of socialist beliefs. The other four types of values do not. The overall expectation is that East-West differences will be relatively small on issues of democratic rights and relatively large in the other categories.

This basic hypothesis is tested using both elite data, which Rohrschneider collected by interviewing 168 members (79 East, 89 West) of the Berlin city parliament, and data from several existing public opinion surveys. The quantitative evidence is augmented with extensive quotation from openended interviews with the Berlin MPs. In addition to the basic East-West differences (which are studied for each value category), Rohrschneider looks at mass-elite differences, differences between the pre- and postwar generations, and (less often) differences between the immediate postreunification period (1992) and later (1995). He also considers the degree of difference in many cases, such as whether the gap between democratic rights attitudes of postwar and prewar cohorts is larger in the West than in the East. Institutional learning, Rohrschneider argues, implies that there should be a larger gap, since both cohorts in the East lived under authoritarian regimes, but the data show the opposite: Age and support for democratic rights have basically the same weak negative correlation in both regions, consistent with the prediction that value diffusion should dominate in the area of democratic rights. An important strength of the book is its careful and nuanced documentation of patterns of support for key democratic values in newly unified Germany.

How well do the data fit the theoretical predictions? Overall, pretty well. The evidence is strongest for two claims: (1) There is no systematic East-West difference in support for basic democratic rights, and (2) there is a marked difference in political tolerance. On the first point, at both the mass and the elite level, East Germans show roughly the same support as West Germans for the right to demonstrate, criticize the government, and freely express opinions. Elites in both East and West are more supportive of democratic rights than their respective mass publics, but neither elites nor masses show a clear East-West difference. Indeed, the numbers seem to imply that the eastern elites are somewhat less supportive of democratic rights than the western elites, whereas the western masses are somewhat more supportive than the eastern masses. (I am forced to say "seems to imply" because the otherwise careful presentation of quantitative data pays less attention to statistical significance than I would like.) On the second point, East Germans are notably less tolerant than West Germans in the sense that a much higher fraction of easterners is willing to deprive disliked groups of basic rights.

The evidence is more mixed with regard to pluralism. The claim that East German elites are less comfortable with the conflict generated by pluralism is supported quantitatively only by a single question, the wording of which Rohrschneider acknowledges to be controversial. A more neutrally worded question shows no difference in the responses of the eastern and the western elites. Moreover, the two mass publics show no difference on either question.

Finally, there are the issues of democratic ideals and economic values. East Germans are notably less supportive

of free markets and more inclined to believe that social equality is key to democracy. Strictly speaking, the data show that the pattern associated with the institutional learning hypothesis is consistent with Rohrschneider's overall framework. But suppose for a moment that the theory is wrong, and these are dimensions on which values diffuse easily. Would we expect the East German public, or their elected representatives, to be indistinguishable from their West German counterparts on such issues as the acceptable level of income inequality, given the notable differences in wealth, income, and employment prospects? Rohrschneider addresses this issue rather late in the book and shows that some East-West differences persist when the individual's perception of the economy and her own prospects are taken into account. But most of the evidence for different economic values and for the role of equality in democratic ideals is presented without these controls. Moreover, on many of the specific questions in the economic values and democratic ideals categories, the gap between elites in East and West is wider in 1995, when the grim reality about the costs of reunification had become clear, than in 1992, when hopes were still high. One might think that, if not self-interest, at least group interest played a role in increasing skepticism in the East about free markets.

Overall, I found the data presented in this book to be extremely stimulating and important for both German politics and the study of democratic transitions in general. Careful documentation of which values have changed quickly and which reflect years of institutional learning is a major contribution. I'do not think this will be the last word on why some values diffuse and others persist, but it will be an important part of future debates.

The Legacy of Human Rights Violations in the Southern Cone: Argentina, Chile, and Uruguay. By Luis Roniger and Mario Sznajder. New York: Oxford University Press, 1999. 384p. \$84.00.

Craig Arceneaux, California State University, San Marcos

Argentina, Chile, and Uruguay emerged from military rule shadowed by the stinging legacy of human rights violations. On the one hand, there was cause for celebration. Democratic institutions were put in place, and the elaborate repressive apparatuses of authoritarianism could now be dismantled. On the other hand, there was cause for concern. As the gravity of human rights abuses under military rule were revealed, pressures for government to confront the perpetrators and for society to assemble some common understanding of the past began to materialize. Precisely why these tasks are paramount issues is a peculiarity of authoritarian military rule itself. To an extent far greater than in other transitions from authoritarianism, governments (and societies) that follow military rule are distinguished by the fact that they must live with their authoritarian predecessors. The terms of this cohabitation thus become integral to the democratic development of the country. Given the recent experience of military rule, threats to reintervene must be taken seriously, and this causes governments and societies to hesitate as they confront the past. At the same time, failure to do so weakens the legitimacy of new democratic institutions presumably based upon truth, justice, and accountability.

Luis Roniger and Mario Sznajder analyze this dilemma, and they do so impressively. Their position is that the past must be confronted in order to aid the healing process of traumatized societies, deter future abuses, open a channel for rehabilitating perpetrators, and allow for an accurate and

agreed-upon assessment of the past (p. 55). After a detailed discussion of each case and the respective human rights policies, the authors find that no country has been able to achieve closure and reconciliation. Politicians, driven more by their fear of democratic reversals than principles of justice, strive to leave the past behind. Individuals within society share the blame. Beyond the threat of reintervention, an environment of socioeconomic instability increasingly pressures them. They fear that inquiries into past human rights violations will weaken the police and security institutions, which would threaten their own personal security. Indeed, Roniger and Sznajder recognize how such concerns contribute to human rights abuses under democracy: "The greater the real or perceived threat of destabilization in these societies, the greater the willingness of the population to support the use of force to keep order and stability" (p. 147).

The problem, as the authors adeptly document, is that without reconciliation and closure, politicians and those in society driven by more immediate concerns find themselves unable to control the return of the past to the public sphere. This occurs episodically as new allegations arise, clandestine torture centers are exposed, hidden burial sites are discovered, and officers make public confessions, or as these same events in neighboring countries or human rights investigations in the international realm find their way across the country's borders. Hence, the democracies of the Southern Cone are doomed to sporadic crises so long as reconciliation remains unachieved.

The discussion of how events in neighboring countries rekindle crises is especially interesting. It points to the fact that repression and the legacy of human rights abuses are just as much regional as country-specific phenomena. This theme resurfaces throughout the book. The authors mention the coordination of repression under the Plan Condor, the fact that most disappeared Uruguayans were murdered in Argentina, and the unifying effect of national security doctrine. Roniger and Sznajder discuss efforts in the democratic era to share government resources or even create international governmental or nongovernmental bodies devoted to the investigation of human rights violations (pp. 121-8). Moreover, they note that human rights policies of the respective countries cannot be viewed in isolation because of the learning process that occurred over time. The Chileans learned from the mistakes of Uruguayans, who in turn learned from Argentine mistakes. This raises an interesting question, since the sequence begins with the country in which the military regime collapsed and ends with the country that experienced the greatest exertion of military control over the transition. If this sequence had been reversed, would Chile be worse off and Argentina better off? Comparison at the regional level with attention to such factors as variations in sequence and transition control is an area for future research.

Much of the latter portion of the book investigates popular culture to assess how the societies of the Southern Cone have processed (or failed to process) the history of human rights violations into their collective identity. Whereas the first part of the book considers the effect of human rights abuses on regime stability, the second part concentrates on society. Noteworthy is the breadth of references to film, theater, and literature as well as the assessment of "official histories," especially in education policy and the "heritage of violence." Indeed, this portion is essential reading for political scientists in order to guard against focusing too sharply on human rights as just a public policy concern. Human rights violations and their legacy have a manifold effect, one that can be accurately assessed only with an interdisciplinary effort, as found in this book. The authors, pooling their respective

specialties, skillfully evaluate legal frameworks and public policy, assess the influence of ideological predispositions and the power balances of political forces, document the status of popular culture, and explore such psychosocial concepts as "collective catharsis," "social trauma," "social disaggregation," and "collective memory." The book is comprehensive not only in subject but also in analysis.

Roniger and Sznajder cannot but end on a pessimistic note. So long as politicians are ruled by political contingency and society is focused on private, individualized concerns, the countries of the Southern Cone will live in the shadow of human rights abuses. But there is room for some optimism if a broader view is taken. The legacy of human rights abuses is but one component in the multitude of issues that comprise civil-military relations, and an approach that places human rights issues in this context can more accurately assess how the legacy will affect future developments. Hence, although President Menem backtracked on human rights, he did maintain control over the Argentine military budget, the redefinition of military missions, and areas of military reform. Roniger and Sznajder are referring only to human rights issues when they speak of turns in the civil-military balance, but the recognition that this balance depends on a number of areas illustrates the complexity of civil-military dynamics. Gains in some areas may be used to shore up authority for exertion of control in other areas. Although a country may fail in the area of reconciliation, its success in preventing future abuses may be determined by factors far outside the sphere of human rights.

Parties, Candidates, and Constituency Campaigns in Canadian Elections. By Anthony M. Sayers. Vancouver: UBC Press, 1999. 254p. \$51.75 cloth, \$19.29 paper.

Frederick J. Fletcher, York University

This carefully researched volume is a welcome addition to the small but growing literature on election campaigns in parliamentary systems. Election studies, with some notable exceptions, tend to focus on individual voting decisions, using national samples. There has been little work on campaigns as exercises in organization, communication, and persuasion, particularly at the constituency level, even though "riding campaigns... are the basis of national elections" (p. 3) in parliamentary systems. The case studies examined by Sayers, although limited to constituencies in one Canadian province and somewhat dated (there have been two federal elections since the 1988 campaign featured in this work), are excellent examples of the use of "thick description" to build an analytic framework. This framework, the hypotheses the author draws from the case studies, and the comparisons he makes with other parliamentary systems should make this volume a useful model for further research. In particular, it provides a benchmark for studies of the effect on campaign practices of the dramatic changes in communication technologies over the past decade. In addition, it provides a sound foundation for work that compares constituency-level campaigns across provinces, with their differing political cultures, and for comparisons among parliamentary systems.

The analytic framework helps Sayers present a detailed portrait of the interaction of local and nonlocal factors in constituency campaigns, with particular attention to the relative influence of national and local party organizations on the nomination of candidates and the nature of campaign appeals. The author employs some fairly standard variables, such as contested versus uncontested nominations, mass versus cadre parties, and the nature of the riding (city,

suburban, rural) and its competitiveness, as well as some more original measures, including type of candidate (local notable, party insider, stop gap, and high profile). In telling the story of seven constituency campaigns, Sayers suggests that the nature of the nomination contest, influenced by the type of party and the competitiveness of the riding, tends to determine the kind of candidate nominated, which in turn shapes both the campaign organization and the nature of the local campaign. This discussion and the chapters on winning and losing campaigns provide an excellent summary of common campaign techniques and their effectiveness in various situations.

Among the important themes running through the analysis is the influence of media attention on local campaigns. The extent to which the competing candidates actually engage in meaningful debate on local issues, for example, appears to be a function of media attention and competitiveness. In constituencies served by major media outlets, the local contests tend to be swamped by national issues, except in some high-profile races, which command major media attention. In suburban ridings, which often lack credible local media, it is extremely difficult for local organizations to run effective campaigns, and most must settle for a campaign that parallels that of the national party. There is more variability in rural constituencies, since the relative presence of local and national media varies from riding to riding. Sayers notes that new communication technologies have had a nationalizing effect on campaigns. It would be interesting, as the author himself suggests, to explore campaigns since 1988, to see whether the Internet and emerging specialty broadcasting services and other new communications technologies have altered this trend.

Of particular interest, given the successful "suburban strategy" of the Conservative Party of Ontario in recent provincial elections, is the one suburban case studied here. Sayers found that this constituency had little sense of community, no credible media to force candidates to engage with local issues or participate in all-candidates debates, and little opportunity for candidates to purchase effective advertising. Spending limits and the poor fit between media service areas and the constituency made advertising too costly for local candidates. It would be reasonable to conclude, on the basis of Sayers's description of Surrey North, that even a "local notable" would have difficulty attracting a personal vote in such a riding. This would mean that an effective suburban strategy would involve heavy television advertising featuring party leader appeals, with little attention to local candidates or issues. Perhaps, however, innovative use of local cable services, telephone canvassing, and the Internet might alter this balance.

Sayers concludes by noting the distinctive nature of Canadian campaigns and suggests that they are an effective adaptation to regional divisions in the Canadian federal system. He observes that the successful national parties in Canada have not developed the mass party structures common in other Westminster-style parliamentary systems but have retained "elements of party discipline and organization not found in the more loosely organized parties of the United States" (p. 215). In Canada, he notes, "the balance of local as opposed to regional or national issues, the role of the candidate and the place of national party platforms are all negotiable" (p. 223). Because the local party associations play an important integrative and representative role, as a result of their jealously guarded control over nominations, the parties are responsive to local variation. Thus, the view of elections as primarily national events is misguided. Although variation in local campaigns is clearly demonstrated here, this hypothesis of its functionality requires further examination. The shifting balance among national, regional, and local campaigns is, arguably, influenced by factors other than relations between national party headquarters and local constituency associations.

The case studies necessarily limit the general applicability of the findings, but the work fulfills its stated goals admirably. It provides a clearer understanding of how parties and elections work in Canada, identifies key variables and assesses their influence, lays the foundation for more representative studies of constituency campaigns in parliamentary systems, and presents provocative hypotheses for further testing. That being said, the book sets out to assess the "democratic nature" of these local campaigns but lacks a clear theory of democracy. It ranks nomination contests in terms of the degree of democracy involved but does not link these observations to any broader concerns about representative democracy. Similarly, Sayers develops a useful typology of candidates and campaigns but does not attempt to link these to "representational styles" in subsequent parliaments. Does the nature of the candidate chosen, the local campaign, or the manner in which the winner campaigned have any influence on parliamentary behavior or public policy? Perhaps this is a question for another study. In the meantime, this book fills an important gap in Canadian campaign studies and provides a starting point for further research.

Democracy in Translation: Understanding Politics in an Unfamiliar Culture. By Frederic C. Schaffer. Ithaca, NY, and London: Cornell University Press, 1998. 168p. \$39.95.

Nelson Kasfir, Dartmouth College

Suppose we reverse our conventional approach to democratization, as Frederic Schaffer suggests in this important, closely reasoned, imaginative, and provocative study. We would need to ask what democracy means to its participants in a specific system rather than whether its practice there can be considered democratic according to external standards. Instead of determining whether the institutions that produce democracy are in place and properly used, or whether these institutions actually promote the ideals considered to be the essence of democracy by Western thinkers, Schaffer develops a sensitively nuanced method to find out what Wolof speakers in Senegal mean by demokaraasi, which he argues is their linguistic equivalent to democracy. His intention is to compare the concepts they associate with democracy to those associated with démocratie by French-speaking Senegalese and with democracy by Americans (pp. 15, 19). The words, he asserts, denote quite different concepts to their speakers. Thus, democracy changes its meaning when it is translated or, more precisely, when it is used by people from another culture.

This book is an inquiry into how political culture shapes democratic concepts, an inquiry conducted primarily through an investigation of natural and ordinary language use. Schaffer's efforts to elicit the meanings intended by ordinary voters are reminiscent of what Max Weber called verstehen. Schaffer's approach provides him with an opportunity to tap a dimension of culture that attitudinal research is unlikely to reach, but it does not allow him to claim that the concepts his informants associate with democracy can be generalized to the language group. Nevertheless, he is persuasive in arguing that comparative cultural research can identify significant differences in the meanings attached to the notion of democracy as it is understood in different cultures. He suggests that these differences are likely to be associated with divergences

in political institutions and political action that are overlooked if we rely on the conventional approach.

Schaffer's central finding is that poorly educated Wolof speakers understand demokaraasi as mutuality, emphasizing consensus, solidarity, and evenhandedness rather than the concepts he takes to be central to democracy, namely, widespread participation in governance, emphasizing meaningful choices, inclusivity, and the relative irrelevance of economic and social inequalities in political participation (pp. 80-5). In demokaraasi, "consensus" means ideally that agreeing among themselves is more important to villagers than individual deliberation. "Solidarity" reflects their expectation of mutual self-help, and "evenhandedness" invokes treating people who occupy the same hierarchical level in the same way. In elections, therefore, villagers are more likely to make their choices on the basis of strengthening community ties and acquiring economic benefits. For example, many villagers or urban residents could agree in advance to vote for the same candidate in order to avoid discord in the village or in return for money, food, or a local public project.

Schaffer argues that these different meanings at the core of demokaraasi stem from ideal responses to economic uncertainty by Senegalese peasants and poor urbanites. As in many other preindustrial societies, customary arrangements in Senegal stress participation in collective insurance to lessen family risk in conditions of great poverty. Schaffer says that demokaraasi entered Senegalese political discourse after 1974, when multiparty elections were adopted (pp. 69, 80). Less educated people modified a notion describing a set of new practices to reflect values in their existing social ideas, their "lived experience," and their "economic predicament" (pp. 75, 76).

How would we know that voters are casting their ballots on the basis of different ideas, and what differences did Schaffer find in Senegal? He restricted his inquiry to Wolofones, but since Wolof is the most widely spoken language in Senegal, the first language of almost half the population, differences are likely to affect both the electoral behavior and the possibility of a fully democratic regime—as that would be understood by Americans or Europeans. Schaffer conducted 78 open-ended interviews with Wolofones, selected from four locations by quota sampling, plus 22 with more highly educated Senegalese Francophones in order to learn what voters intended when they cast their ballots (p. 19). Within this sample, the level of education of the informants decisively predicted whether they understood demokaraasi as involving consensus, solidarity, or evenhandedness or as excluding these concepts (pp. 64-5).

Schaffer also asks whether demokaraasi strengthens or subverts Senegal's democratic aspects (p. 116). Consensus and clientelism may weaken democracy, as understood in the West, by reducing the degree to which election results reflect both individual participation and the inclusiveness of the majority of the population. To the extent that the social solidarity of the poor makes elected officials more accountable, demokaraasi strengthens democracy (pp. 129–30). Although demokaraasi could be democratized, to do so would seem to require more widespread economic security or more French education, both too costly for any Senegalese government to contemplate.

Strictly speaking, Schaffer establishes that some less educated Wolofones attach a different meaning to democracy in ordinary language than Americans express in news and magazine articles or political scientists assert as basic criteria. His point is deeply important, although his comparison is awkward, and he does not provide the grounds for general-

izing his findings about individual Wolof speakers to the whole group.

The author appropriately insists that "the main question . . . is whether . . . [American democracy, Francophone démocratie, and Wolofone demokaraasi] share ideals or standards" (p. 19). But he does not attempt a satisfactory comparison because he employs completely different methods to analyze each notion. He did not interview Americans to elicit their natural language associations with the word democracy. He interviewed some Senegalese Francophones but does not analyze the meanings they attach to démocratie. His careful discussion of metaphors used by Francophone politicians in campaign speeches does not demonstrate ordinary language but, rather, the variety of public political meanings it can sustain.

Schaffer's limited tools do not permit him to generalize the extraordinary differences discovered. Open-ended interviews can reveal unexpected meanings for particular concepts, but not how widely specific meanings are held by a particular cultural group. The author does not show us, perhaps cannot show us, how uniformly Wolofones as a group define demokaraasi as meaning the concepts he has identified. In any case, he pays too little attention to the adequacy of his sample for his conclusions.

These findings, then, are basically exploratory. Nevertheless, the work has profound importance in establishing that democratization will not have the same meanings to people in different places. It will not be possible for anyone considering the meaning of democracy after its institutional transfer to ignore Schaffer's book. His explication of ordinary language to tease out meaning suggests the rich possibilities that await other political scientists interested in the comparative cultural meaning of political concepts, and not just democracy.

Religious Politics in Latin America: Pentecostal vs. Catholic. By Brian H. Smith. Notre Dame, IN: Notre Dame University Press, 1998. 126p. \$16.00 paper.

... Anne Motley Hallum, Stetson University

Brian Smith has written a comprehensive review essay of scholarly literature assessing the Pentecostal phenomenon in Latin America as well as the Catholic response. It is not a pathbreaking book, but it is so useful in its organization and balanced summary of a growing body of literature that it will likely be widely read and cited.

Smith begins simply by acknowledging the significance of the massive growth of Protestantism in Latin America over the last thirty years, particularly the Pentecostal variety, which is the choice of the poor majority. Some observers estimate that if present trends continue, Protestants may outnumber Catholics in fifty years. Even if the movement alows down considerably, however, it has already influenced the culture greatly by introducing new religious choice, especially in Brazil and much of Central America. In the second and third chapters, Smith offers an assessment of Catholicism today and gives reasons for the break-up of the Catholic monopoly after 500 years. The explanation, garnered from numerous case studies, ranges from the negative failings of Catholicism, such as the shortage of priests and pastoral care, to the positive offerings of Pentecostalism, such as its flexibility and spiritual power. Smith also mentions the early, condescending writings of Christian Lalive d'Epinay and Jean-Pierre Bastian, who described the conversions as a phenomenon of the poor seeking authoritarian refuge from the changing world; he quickly follows with a summary of

more recent studies, which conclude that Pentecostalism "is a genuinely popular phenomenon driven by the missionary zeal of its local converts" (p. 26).

The author then lays out three possible scenarios for the future in the emerging shared space of Catholics and Pentecostals: (1) "Mutual Flight from the World" as both groups withdraw from attempts to respond to urgent social and economic problems of the region; (2) "Conflicting Religio-political Agendas," in which the churches open themselves to political manipulation by either neoliberal politicians or leftist political movements, which would be destabilizing for Latin America; or (3) "Prophetic Social Catalysts," an optimistic option in which Pentecostals and more progressive Catholics come closer together "in support of styles of citizenship and public policy that favor economic development, social equity, and constitutionalism," (p. 18).

Smith's examination of these scenarios using existing research is the book's most valuable contribution, based as it is on a thorough bibliography. Using this literature, Smith rejects the argument that either Catholics or Pentecostals as a group will retreat from social and political issues into a preoccupation with the afterlife. Given some premature pronouncements that have appeared regarding the death of liberation theology in Latin America, it is indeed refreshing to read about committed, social activist Catholics battling the effects of neoliberal policies. If one were to survey leaders of the Worker's Party in Brazil, for instance, one would likely find that they are adherents of liberation theology and are as involved as ever in taking political action on behalf of the poor. Similarly, Smith dispels the stereotype that Pentecostals are apolitical or uniformly conservative, noting that they are the poor and are showing signs of organizing against social injustice. Smith also dispenses with the notion of a dangerous polarization: "When all the evidence is weighed together, however, it seems very unlikely that either Catholicism or Pentecostalism will legitimize extremist economic or political movements in the foreseeable future" (p. 88).

Smith accepts the third option as the most likely scenario for the coexistence of Catholics and Pentecostals, that is, a collaboration on shared social and religious concerns in a way that strengthens democracy. It is clear from his closing argument, however, that it is far easier to say what the current religious environment in Latin America is not than to predict what it will be. Smith tries to be optimistic, but he also recounts numerous instances of confrontations and angry rivalries between the two groups, and he must conclude that a Pentecostal/Catholic alliance will not appear soon. With this book, Latin American scholars have a concise summary of a respectable body of research about the new religious pluralism, as we await further developments.

Stealing the State: Control and Collapse in Soviet Institutions. By Steven L. Solnick. Cambridge, MA: Harvard University Press, 1999. 337p. \$18.95 paper.

Philip G. Roeder, University of California, San Diego

Steven Solnick provides a rich, neoinstitutionalist analysis of the disintegration of bureaucracies in the last years of the Soviet Union. He adds an important element to our understanding of the complex process of the disintegration of the Soviet Union itself by explaining why bureaucrats in many hierarchies seemed all at once to resist the efforts of the central Soviet state to guide their actions. Using concepts drawn from neoinstitutional political economy, Solnick analyzes the behavior of multiple agents when their common principal can no longer monitor their behavior or sanction

their opportunism. The empirical richness of this book comes from three case studies of bureaucracies designed to mobilize youths in the Soviet Union—political mobilization by local Komsomol organizations, economic mobilization by committees that assigned graduates of higher education institutions to their first job, and military mobilization through local draft boards. Solnick compares each bureaucracy's relative success in fulfilling its chief success indicator (or quota) before the Gorbachev reforms with its performance once the reforms were in place.

A key thesis is that "the Soviet system did not fall victim to stalemate at the top or to a revolution from below... but rather to opportunism from within" (p. 3). The decisive element in the collapse of the Soviet system was the refusal of bureaucrats to obey orders from above. Ideology, elite politics, and societal demands—the three alternatives Solnick considers—cannot account for the pattern of bureaucratic behavior during the reforms. This can only be explained by the opportunism of bureaucrats and the manner in which the reforms led to escalation in opportunism.

Solnick builds on a careful analysis of the behavior of bureaucrats in the Soviet Union before the Gorbachev reforms. Consistent with analyses of other Soviet bureaucracies, he finds that "street-level" bureaucrats in the Komsomol, job-assignment committees, and draft boards focused on the primary indicators used by superiors to evaluate their success—gross output of new members, job assignments, and conscripts. Alternatively, they shirked on secondary indicators, which were less likely to bring either rewards or punishments from principals. Many of these secondary objectives were compromised in the rush to "simulate success" with regard to the primary indicators; local Komsomol organizations continued to add new members without concern for their ideological preparation, and draft boards inducted young men without concern for their health. Solnick argues that the ability of a principal to ensure that agents continue to exercise the principal's "property rights" over the organizational resources in the principal's interests (and not expand their opportunistic behavior so that it compromises fulfillment of the primary success indicator) depends upon the principal's continuing ability to monitor and sanction behavior with regard to the primary objectives.

This leads Solnick to an interesting perspective on how the reforms contributed to organizational collapse. Under Gorbachev's reforms many principals were unable to prevent or punish opportunism, which resulted in a "cascading breakdown of authority." Solnick's graphic metaphor is a run on a bank, but with an important twist: Once it became known that a principal could not sanction a lower-level bureaucrat for violating the "contract" between principal and agent, the latter was more likely to defect—to exercise the property rights in the agent's rather than the principal's interest, as though the organization's resources were the agent's property. More important, all other agents of that same principal then faced a stark choice. If an agent did not defect but others did, the principal could "go bankrupt," and others might seize the assets from the agent's control. An agent who defected risked sanctions but might gain control of a portion of the principal's assets. Agents in different bureaucracies might still differ in their responses to this same cue because the resources they controlled might not be easily reassigned to other purposes (asset specificity). Yet, in the face of information that the principal had not imposed sanctions on the first agents, many defections were likely to follow.

For example, once the Komsomol abandoned binding enrollment targets, street-level agents began to shirk recruitment tasks. Komsomol membership dropped precipitously. The resulting fall in membership dues and cuts in subsidies from the center led local leaders to transform many Komsomol operations into money-making subsidiaries, such as travel bureaus and youth hostels. With their own locally generated resources and little fear of sanctions from the center, these agents were able to assert their independence from the Komsomol organization. At both the center and in the local organizations, officials began to transfer these assets into private commercial investments entirely outside the control of the Komsomol organization. They stole the state.

Solnick provides a compelling picture of the mechanisms of bureaucratic collapse. In the conclusion he explains how this might help us understand the different outcomes of reform in the Soviet Union and China. A sure tribute to the importance of this book in the study of the transition from communism is the debate it should generate among specialists. Two closely related issues may be central to this debate. First, the explanation for the collapse of individual bureaucracies including how opportunism to fulfill organizational objectives, such as meeting a quota, became opportunism that dismantled these very same organizations—may lie outside those bureaucracies. Through much of the story about changes in the youth bureaucracies, the driving force is a series of exogenous shocks (particularly, decisions by the General Secretary) that are, of course, not investigated. These decisions also placed limits on what central bureaucrats could do in sanctioning subordinates. Constraints originating outside the youth bureaucracies may be the essential reason some bureaucracies collapsed and others transformed.

Second, the exogenous factors in the story about youth organizations may contain the essential causes of the collapse of the Soviet Union itself. Indeed, in a very imaginative counterfactual, if Gorbachev's reforms had succeeded and the Soviet Union survived, one still would have expected many of the outcomes that Solnick describes so well—no Komsomol, greater university autonomy and expanded direct contracting for first jobs, and draft boards that continued to deliver youths to the front lines. This may lead some to suspect that the changes in street-level agents were epiphenomenal—at most symptoms of, but not the core reasons for, the collapse of the Soviet Union. Solnick has staked out an important position in what should be a lively and fruitful debate.

From East Germans to Germans?: The New Postcommunist Elites. By Jennifer A. Yoder. Durham, NC, and London: Duke University Press, 1999. 287p. \$54.95 cloth, \$18.95 paper.

Helga A. Welsh, Wake Forest University

The unification of the two Germanys has led to a surge of scholarly literature. Between 1990 and 1995 alone, approximately 3,000 German-language studies analyzed the political, social, and economic transformation processes in the former East Germany. Clearly, in the English-speaking world, the interest in and analysis of German unification have been more muted. Indeed, due to the singularity of unification and the dominant role of the former West Germany in shaping it, the East German case has been neglected in the vast body of literature that focuses on the transition from communism to democracy. This is especially true for the analysis of political elites. The results seem obvious: At the level of national elites, West German politicians govern, and, in general, the East German elite was almost completely replaced. In contrast, in the rest of the former communist world the combi-

nation of elite circulation with elite reproduction has received extensive attention.

Jennifer A. Yoder links elements of elite building in the former East Germany to democratization. She suggests that the implantation of the West German system in the east has led to alienation from, and disappointment with, the current political system that can be lessened by fostering a new indigenous elite. She focuses on one subset of political elites: members of parliament in the five states of former East Germany. These state parliaments are new; the previous state structures were abolished by the communist regime in 1952; and, in contrast to other institutions of governance, elite transfer from the west is negligible. With very few exceptions, East Germans are elected to the parliaments, and the author examines their background, changing expectations, values, and orientations. They are challenged to conform to West German standards, adjust to rapidly changing conditions, and try to create links to the electorate that foster democracy. Thus, Yoder's study of elite building is embedded in a broader analysis of German unification and democratization.

The book is based on a thorough analysis of the German and English literature and is enriched by in-depth interviews with more than eighty parliamentarians, selected from three of the five state legislatures. To set the stage, the author makes a strong, although not original, case to explain why the communist regime was particularly inimical to the development of a counterelite in the German Democratic Republic (GDR). She points out the value and behavioral implications that communist principles of socialization and recruitment have for the first generation of postcommunist elites. She then analyzes the extraordinary elite changes in the equally extraordinary months of 1989-90 that led to the exclusion of former communist officeholders. Once unification and elections to the state parliaments had taken place in October 1990, testing in the "real laboratory for the merger of eastern culture and western institutions" (p. 112) could begin.

At the core of the study is the question: "Will German unification result in elite uniformity, or will the merger of two societies under one political institutional framework allow for unique patterns of political orientations and behavior at the elite level" (p. 113)? Yoder skillfully circumvents the pitfalls of simple answers and highlights the "dissonance between the formal requirements of the new political system and the old patterns of political thinking and behavior" (p. 152). Her work is most interesting and novel in the area of horizontal

elite integration. In particular, the interviews with state legislators shed light on their motivations to become and stay politically active, their initial struggles, and their learning processes. The role model and expertise provided by the west accelerated and facilitated the process, on the one hand, but also led to disappointment and resentment, on the other hand. Distinct differences between eastern and western politicians persist; many politicians in the former GDR struggle to find a synthesis between assimilation to western cultural norms and preservation of those that are specific to their constituencies. In the author's view, finding this role is essential to overcoming existing weak elite-mass linkages. For Yoder, the new parliamentary elites must "link the public to the political system and bridge the political gap between the east and the west" (p. 158).

Although the assessment of existing elite-mass linkages is negative, Yoder is optimistic. East German elites have successfully mastered the immediate tasks of professionalization and normalization; more and more politicians are trying to act as a bridge between exogenous and indigenous policy styles, behaviors, and norms. The analysis of legitimacy deficits among the East German electorate puts the author in familiar and well-traveled territory. Her portrayal is balanced and thoughtful, using indicators such as voter turnout, party membership, and trust in institutions. The question lingers to what extent doubts about the legitimacy of the new system can be attributed to the specific conditions of German unification or to the uncertainties and vagaries of any transition. After all, similar developments can be found in other postcommunist settings, a fact to which she alludes too briefly.

Yoder fills a void in the literature by demonstrating how, in the years immediately following the collapse of communism in East Germany, elite composition and elite culture were recast. She makes a strong case for the influence of regional elites in providing regime legitimacy in Germany and elsewhere. Her synthesis of past developments also raises stimulating questions about the future: Will former communist settings generate new patterns of elite structure and culture that are substantially different from western models? Will eastern German politicians emulate western patterns or develop their own? Although it is too early to settle on answers, Yoder's study will inform those who want to pursue them—in eastern Germany and elsewhere in the former communist countries of Central and Eastern Europe.

International Relations

Cultures of Antimilitarism: National Security in Germany and Japan. By Thomas U. Berger. Baltimore, MD: Johns Hopkins University Press, 1998. 256p. \$38.00.

Richard Leitch, Gustavus Adolphus College

Thomas Berger joins an expanding cadre of international relations and comparative politics scholars who have focused on the cultural differences between nation-states to explain foreign (and more specifically, military) policy decision making. Nations will act and react to changes in the international system differently and distinctively, Berger argues, depending on their "domestic political forces and institutions" and their political actors' "socially negotiated understandings of past historical events" (p. xl). These conclusions should not surprise many readers, nor should Berger's prepared defense

against an anticipated attack by those he expects will criticize the tautological risks inherent in cultural approaches (read: system determinists and material rationalists).

As an economically ascendant Japan and Germany assumed a more visible international role in the mid- to late 1980s, international relations scholars and analysts revisited and reissued hegemonic stability theories and theories to explain and predict the rise and fall of "great powers." Laden with these presuppositions, Berger planned to watch the militarist rise of Japan and Germany and, presumably, craft his doctoral dissertation from all that transpired. Contrary to what Berger and some others expected, however, and challenging his reading of Hans Morgenthau, neither Japan nor Germany had any intention of assuming the military side of great power status. Instead of explaining what happened, Berger was forced to understand why it did not. His conclusion is fairly obvious: Japanese and German military and

foreign policies were constrained by domestic memories of decisive military defeats, which created in both an enduring antimilitarist sentiment.

Berger contends his study bridges the realms of the institutionalist/elite level political culture perspective, on the one hand (as advanced by John Odell, U.S. International Monetary Policy: Markets, Power and Ideas as Sources of Change, 1982, and especially Peter Hall, Governing the Economy, 1986), and, on the other hand, that of scholars who approach political culture "as it is rooted in the microlevel institutions of society—the family, the church, and so forth" (Ronald Inglehart, Culture Shift in Advanced Industrial Societtes, 1990; Robert Putnam, Making Democracy Work: Civic Traditions in Modern Italy, 1993) (pp. 204-5). In applying this two-pronged method to a fifty-year period of the evolution, development, and entrenchment of antimilitarist sentiment in two nations, Berger inevitably must sacrifice something in a book of this length. His indebtedness to the institutionalist/ elite perspective is apparent, as he does not sufficiently delve into the "microlevel" side, let alone parse its distinct elements. The result is an aggregated "society" as expressed primarily through public opinion data. Behind these numbers are there not generational differences that must be considered, especially in a work that claims to understand why antimilitarist sentiment remains so deep in both countries, despite citizens who have lived fifty years without memories of the war?

Berger draws from a wealth of original German and Japanese language sources and provides supportive interviews with foreign policy decision makers, academics, and other experts in both countries. Yet, the analysis is deficient in fully explaining the way(s) in which antimilitarist messages are perpetuated in either Japan or Germany. Challenges to this antimilitarist sentiment are presented in the form of (among others) official visits by leaders to memorials that honor soldiers killed in the war and, paradoxically, the consistent support for these leaders (Nakasone and Kohl) following an initial public uproar.

But is that the only way, or even the primary way, in which antimilitarist thought and policies are reinforced in the collective psyche of a nation—through a perceived attack on the antimilitarist status quo, with reaction to that attack providing the principal opportunity to defend and simultaneously advance that sentiment? Or are there other, more consistent ways in which antimilitarism is underlied as the "socially negotiated understanding," ways that Berger largely fails to address (Putnam 1993)? Why does an antimilitarist sentiment persist among generations that have not experienced war, especially since, as Berger admits, Japan suffers from collective "historical amnesia," and the issue of Japan's role in World War II is virtually glossed over by Japanese textbooks? Are those Germans and Japanese born after the war, particularly after what is for Berger the defining period of the 1950s and 1960s, antimilitarist because military/security issues are of no apparent interest to them, or are they antimilitarists by conviction? Berger does discuss the rise of the Greens in Germany and the Komeito in Japan (the former more explicitly linked to a challenge to antimilitarism than the latter), but his examination of the nonelite view of political culture remains largely at the level of political parties.

As a comparative study of postwar domestic factors (and to a lesser extent international factors) that contribute to military policymaking, this is a well-researched and generally well-presented piece of scholarship. For scholars of Japanese and German military politics, the analytical perspective and considerations will be familiar (military-related interpreta-

tions of the German Basic Law and Japan's postwar constitution; neighboring states' concerns of a military revival; alliance politics; civilian control of the military). The actors (Adenauer and Yoshida, Kohl and Nakasone, among them), their assumed perceptions, and their actions are also familiar. Again, one can cover only so much ground and cover it thoroughly in a book of this length. Yet, I believe a significant amount of familiar territory could have been shelved in favor of an investigation of more analytically compelling issues germane to the book's thesis. One also should not dismiss the influence of a range of issues created by Japan's protracted recession or German reunification in stifling any plans of either nation to assume or pursue a more militarily visible and active role. These circumstances confronted the respective governments during the time Berger had expected them to be more militarily committed and engaged. Berger does not consider either of these explanations. That assumes, of course, that significant groups of domestic decision makers in Japan and/or Germany, or a significant percentage of the electorate in either of these two democracies, harbored goals of great power status in the first place.

Statecraft and Security: The Cold War and Beyond. Edited by Ken Booth. Cambridge: Cambridge University Press, 1998. 358p. \$59.95 cloth, \$19.95 paper.

Mark Neufeld, Trent University

With the collapse of the Soviet Union and the end of the Cold War, an obvious question presents itself: What next? One can discern at least two distinct categories of response from students of world politics. Some scholars view the history of the Cold War, from start to finish, as being consistent with—indeed, a clear confirmation of—established theories of world politics. For them, what is required is simply new analysis of the changes wrought by the passing of the East-West conflict. Other scholars view the history of the Cold War less as an occasion to reaffirm the value of conventional wisdom than as an opportunity to reflect on the (in)adequacy of our theoretical frameworks. Not just new analysis is needed, they argue, but new ways of analyzing.

This distinction provides a means of assessing the individual contributions to the volume under review. The book is divided into three parts, each reflecting a different point of entry for considering the Cold War. In Part 1, the lessons and legacies of the Cold War are taken up, including debates surrounding its origins and ending as well as the place of nuclear weapons. In Part 2, the focus shifts to an exploration of the actors and expected behaviors in the post–Cold War world. Questions addressed range from the leadership potential of the United States, the prospects for postcommunist Russia and post–Cold War Europe, the role for middle powers and for Japan and China, and the difficulties facing the societies of the African continent. Part 3 casts an eye to long-range trends and possibilities, including the reform of international institutions, questions of human identity, and life politics under conditions of globalization.

It is noteworthy that this book shows considerable openness to the idea that new ways of thinking (not just new analysis using conventional frameworks) are required to meet current challenges. This should not come as a surprise, since Ken Booth is a long-time critic of established orthodoxies and an advocate of "new thinking" in the study of world politics. A good example of both is the opening chapter, in which Booth explores the ways the Cold War both structured and was perpetuated by the divisions it created in human minds. Other noteworthy contributions come from Raymond Garthoff (who addresses the question of "blame" for the Cold War), Denis Stairs (who reflects on the category of "medium powers" and their potential roles), and Anthony

Giddens (who explores the significance of the transition from "emancipatory politics" to "life politics").

Perhaps one of the more useful contributions of this volume is the call, in a number of separate contributions, to widen the definition of security beyond that of traditional state-centric notions. John Steinbruner, for example, argues that, given the need for stable employment as a means of assuring civil order, a state's employment strategy is increasingly an international security issue. It is no great leap from this position, moreover, to that promoted in Booth's concluding chapter. Questions of security and statecraft in a post—Cold War world must be framed in terms of the larger question of global transformation, including globalization (the effects of an increasingly wired global capitalist economy), global governance (the organization of world politics), and global moral science (the relevance of dominant political and ethical ideas).

Not all contributors show the same openness to rethinking radically basic assumptions about world politics. At best this lack of openness leads to an excessively tentative restatement of some of the more obvious points long made by less "responsible" scholars (e.g., the chapter by Richard Ned Lebow and Janice Gross Stein cautiously allows that the nuclear war-fighting strategies adopted by the two superpowers may have contributed to political instability). At worst it leads to an unreflective promotion of the worst kind of neoliberal "end-of-history" thesis; as suggested by Barry Blechman's contribution, the values of democracy, peace, and respect for the rule of law-as exemplified in U.S. foreign policy—need only time and commitment to become fully formative of the politics of the global polity more generally. Truly, "more of the same" seems in no danger of losing its secure place within the offerings of the academic community for the foreseeable future.

At the risk of seeming unappreciative of the ideas of those who have tried to break free of established conventions of thought, it is worth noting the self-imposed limits within which even the more iconoclastic chapters are written. It is surely not insignificant that those who break with mainstream thinking draw almost exclusively on neoidealist currents for inspiration. What is not engaged in any sustained way are the arguments of more radical theorists regarding the Cold War: neither historical materialists, such as E. P. Thompson, Noam Chomsky, Immanuel Wallerstein, and Robert Cox, nor feminist scholars, such as Cynthia Enloe and Jean Elshtain. Perhaps for this reason the laudable critique of limited thinking is not linked up in any systematic way with an analysis of material interests or gender, an analysis that might clarify how certain kinds of deformed thinking, while running counter to the general interest, can nonetheless be extremely beneficial to certain social groupings.

All of which is rather disappointing. For those who share a normative commitment to a more just, more humane post—Cold War world, engaging the full range of understandings of where we came from would seem vital to help us get to where we want to go. Still, although this volume may not go as far as some would like, taken as whole it does start us down a very important path. For that reason alone it merits consideration.

The Past and Future of Nuclear Deterrence. By Stephen J. Cimbala. Westport, CT, and London: Praeger, 1998. 235p. \$55.00.

Gregory Paul Domin, Mercer University

Stephen J. Cimbala provides a welcome addition to the ongoing debate over the utility or danger of the spread of nuclear weapons, "now that they have been freed of their

Cold War dependency and nuclear diplomacy [is freed] from its rigid connection to the US-Soviet power struggle" (p. 2). Academics as well as policymakers have fervently debated the possible consequences of the spread of nuclear weapons. Most analysts argue that the spread will ultimately destabilize the international system and lead to nuclear war. Others counter, however, that the prospects of nuclear war are reason enough for states to adopt sagacious security policies. Cimbala's book is a wonderful synthesis of Cold War history/examples and post—Cold War policy that is full of lessons for students interested in the subject.

Cimbala offers a well-written and well-conceived volume. Over the course of seven chapters, he examines "the nuclear past in order to foretell at least in some part of the nuclear future" (p. 1). He assesses how nuclear weapons and nuclear deterrence vis-à-vis the United States and the Soviet Union functioned during the Cold War and how such weapons may be leashed in the post-Cold War era. Cimbala examines several case studies, from the Cuban Missile Crisis, to START II, to limited nuclear war in the old and new world orders. He looks closely at these cases through the analytic lens of deterrence and concludes that "what we learned about nuclear weapons and nuclear deterrence from the Cold War should not be forgotten, lest we repent" (p. 7). One can make the argument that the logic, or illogic, of nuclear proliferation in the current era does not necessarily destabilize international relations and may even reduce the risk of war (which neorealist Kenneth Waltz has been arguing for years).

Although clearly aware that his conclusions may extend well beyond the case studies, Cimbala does not attempt to draw unnecessarily sweeping generalizations about the future of nuclear deterrence. Instead, he allows his conclusions to emerge from the case studies. He suggests, for example, there is little evidence that during the Cold War either the United States or the Soviet Union was preparing for a possible preemptive nuclear strike, as the post-Cold War literature has suggested for the past ten years. Rather, both sides had accepted the logic of deterrence. This makes Cimbala somewhat optimistic about the future, because the two superpowers demonstrate that deterrence works. Nuclear "weapons can be used in two ways: by threatening to physically deny the opponent his objectives in battle or by threatening to destroy enough of the opponent's social values in retaliation so that any attack does not pay. Nuclear weapons, then and now, deter threat or retaliation posing unacceptable damage" (p. 213). Cimbala provides important arguments in favor of the logic of deterrence versus the logic of nonproliferation in a very straightforward manner.

This volume needs to be read by academics and policymakers alike. Its clear and relatively idiom-free prose makes it accessible to students interested in regionalized conflict. It should be required reading for anyone with an interest in this most important issue.

The Sanctions Paradox: Economic Statecraft and International Relations. By Daniel W. Drezner. Cambridge: Cambridge University Press, 1999. 342p. \$64.95 cloth, \$24.95 paper.

T. Clifton Morgan, Rice University

The last quarter of the twentieth century was characterized by a substantial increase in the use of economic sanctions as an instrument of foreign policy. They became the weapon of choice for many, particularly in the United States, because they are perceived to be a less costly and more moral alternative to the use of military force. Curiously, however, this occurred in the face of a strong consensus among scholars and pundits that economic sanctions are ineffective at bringing about change in the target states' policies. One must wonder whether policymakers know something that analysts do not or are so callous as to enact policies known to be ineffective even though there will be costs imposed on their own citizens. Drezner reports on part of a growing body of research that addresses the puzzle of economic sanctions. He develops a theoretical argument capable of explaining when sanctions will be imposed (even when failure is expected) and when they will induce the target state to alter its policies. He subjects this theoretical argument to a wide variety of empirical tests.

A common argument suggests that sanctions continue to be employed in spite of their apparent ineffectiveness because they serve domestic political or symbolic purposes. That is, decision makers impose economic sanctions that they know will not affect the target state's behavior in order to satisfy domestic constituents and/or convey a principled stance. One of the most significant contributions of Drezner's book rests in its attack on this argument. Through a review of empirical studies regarding initiation of sanctions as well as through his own statistical tests, Drezner argues convincingly that sanctions are generally imposed to achieve policy goals, not to pursue domestic or symbolic ends. Drezner builds a particularly nice case on this point. Instead of viewing each side of the debate (symbolic/domestic versus policy) as an hypothesis, he treats each as a theoretical argument. Thus, rather than try to test each directly, he derives implications from each and identifies instances in which contradictory expectations regarding specific relationships occur. For example, he suggests that if the domestic politics argument is correct, then we should expect democracies to achieve their policy goals through sanctions less often than do other states. If sanctions are intended to serve policy goals, then there should be no difference in success rates between democracies and other types of states. Drezner makes his point empirically through a series of critical experiments on such contradictory expectations. The results consistently support the notion that sanctions are generally imposed to serve policy goals.

The major focus of the book is the theory Drezner develops to explain when sanctions will be imposed and with what effect. This theory is based on a relatively simple game-theoretic model. Drezner develops a basic, complete information model in which the actors' utilities are a function of only the costs associated with sanctions and the benefits associated with various policy outcomes. The fascinating aspect of this model is that the primary conclusion to which it leads is that sanctions should never be observed. If the costs of sanctions will be sufficient to induce a change in behavior on the part of the target, then the target can anticipate this and acquiesce before those costs are paid; if the costs are not sufficient to induce a change in the target's behavior, then the sanctioner, who can also anticipate the outcome, will not impose sanctions.

Drezner draws the appropriate conclusion from this result: Something else must be added to the model. Game theorists will probably anticipate that the next step is to incorporate incomplete information, but Drezner follows a different path. He develops the model in a manner suggesting that states' expectations regarding the future of their relationship strongly affect their behavior in sanctions situations. In particular, states' conflict expectations (the probability of becoming involved in additional conflicts with one another) determine whether they are more concerned with absolute or relative gains. If they anticipate that any advantage gained by the opponent in the current conflict can be used to gain

additional advantages in future conflicts, then they will be far more willing to suffer costs that might otherwise appear irrational. Thus, states will be more concerned with relative gains when dealing with an enemy and with absolute gains when dealing with an ally. Incorporating this insight into the model leads to a number of interesting, and testable, propositions. In brief, the model leads us to expect that sanctions will rarely be imposed on allies, but when they are, they are likely to be successful. States will be willing to impose, and maintain, sanctions on enemies even when there is little chance of success, and in these cases the target is likely to hold out even under extreme costs.

Much of the book is devoted to testing these hypotheses in a number of contexts. Statistical tests are based on large samples of sanctions episodes. In particularly detailed and interesting accounts, Drezner uses case studies of economic coercion by Russia on other states from the former Soviet empire. The case studies are well done and are carefully, and creatively, designed to provide tests of expectations derived from the theory, rather than just used to provide real-world support for ad hoc assertions. Each study follows a set pattern, which suggests they were done systematically, and this makes it easy for the reader to gauge the support for the conclusions. Not surprisingly, the empirical results provide strong support for the conflict expectations model.

This book makes a very real contribution to the literature on economic sanctions and should be taken seriously by all who work in this area. The major criticism is that the model is not taken very far, and not much indication is provided regarding what can be done in the future. That conflict expectations matter in some simple and predictable ways is useful, but the model is not used to explain more than a few aspects of the sanctions phenomenon. This criticism can be addressed through future work, but if the model cannot lead to more hypotheses than are presented in this book, it is destined to be superceded by a more encompassing approach. The ideas and evidence are sufficiently intriguing, however, to suggest that Drezner, and others, should push the model farther.

Inventing International Society: A History of the English School. By Tim Dunne. London: Macmillan in association with St. Antony's College, Oxford, 1998; New York: St. Martin's, 1998. 207p. \$65.00.

Robert Jackson, University of British Columbia

The classical international society approach to the study of international relations, that is, the "English School," owes its origins to the financial generosity of the Rockefeller Foundation. In 1954 Dean Rusk and Kenneth W. Thompson organized a group of American journalists, academics, and Department of State officials to investigate theoretical aspects of international relations. In 1958, at the suggestion of Thompson, a similar committee of British scholars and commentators on world affairs was funded and organized. Unlike its U.S. predecessor, it successfully launched a tradition of inquiry that attracted younger scholars not only from Britam but also from Australia, Europe, South Africa, Canada, and Japan, among other places. That traditional interdisciplinary approach inquires into the principles of foreign policy, the practices of statecraft and diplomacy, the ethics of war, the place of human rights in a world of sovereign states, and so on. It calls upon the knowledge of political and moral philosophy, international law, and diplomatic and military history.

Although the American committee also took something of

a traditional approach, it published an edited volume and disbanded (W. T. R. Fox, ed., *Theoretical Aspects of International Relations*, 1959). Its legacy was soon swept away by the rising tide of social science positivism that drew its inspiration from sociology, psychology, economics, and the philosophy of science and that went on to dominate the study of international relations in the United States over the next four decades.

Tim Dunne has written a highly readable history about development of the English School and the work of some of its leading representatives. In an opening chapter he outlines the approach they take, stressing, quite rightly, their emphasis on a "tradition of inquiry," their "interpretive approach," and the importance they assign to normative theory. He devotes two chapters to the Rockefeller-funded British Committee, which left a deposit of significant writings, the most important being a volume edited by Herbert Butterfield and Martin Wight, (Diplomatic Investigations, 1968), which contains seminal contributions not only by the editors but also by Hedley Bull, Michael Howard, and several other scholars. There are separate and insightful chapters on Wight, Butterfield, Bull, and John Vincent. All of that, and more, will enlighten and inform anyone who may be interested in learning about the classical approach that survived and even flourished on the other side of the Atlantic during the same period that the social sciences colonized the study of international relations in North America.

Unfortunately, Dunne was not content merely to present and assess the academic contribution of the English School. He tried to incorporate into it something alien, namely, the work of E. H. Carr and of critical IR theorists (and constructivists) more generally. Carr was never a member of the British Committee. He did not write in the same vein as Butterfield, Wight, Bull, or Vincent. His work does not strive to enter into the political uncertainties and moral dilemmas of statesmen, which the English scholars felt obliged to do in order to understand the profoundly human character of diplomacy, foreign policy, war, and international relations more generally. Carr had very different scholarly inclinations. He downplayed the situational ethics of statecraft and understood power in an altogether more sociological and materialist way. Power was a determining force in world affairs, not merely an agency or apparatus that statesmen could employ for good or ill. He was, as Dunne rightly notes, a prototypical critical theorist. "Power for Carr, unlike Bull or Vincent, goes all the way down" (p. 36). That is far removed from the classical humanism of Wight, Butterfield, or Bull.

Presumably Dunne assigned Carr to the English School to buttress his claim that a new version of international society theory has been perfected that escapes the state-centric predilections and politically agnostic detachment of Butterfield, Wight, Bull, and Vincent. These progressive international society theorists, led by Ken Booth and Andrew Linklater, are ambitiously seeking to provide intellectual direction that will enable humanity to realize its destiny of freedom and justice by breaking the chains of political nationality and state citizenship. But that purported fraternity between the classical international society theorists and the critical theorists, which Dunne captures using the elastic expression "critical international society theory," is two different schools of scholarship, not one. The classical theorists and the critical theorists approach the study of international society in ways that are as unlike as, shall we say, Grotius and Burke, on the one hand, and Marx and Engels, on the other. That is the only major flaw of this otherwise informative and useful study.

Poststructuralism and International Relations: Bringing the Political Back In. By Jenny Edkins. Boulder, CO: Lynne Rienner, 1999. 163p. \$49.95.

Navigating Modernity: Postcolonialism, Identity, and International Relations. By Albert J. Paolini. Boulder, CO: Lynne Rienner, 1999. 227p. \$52.00.

Jutta Weldes, University of Bristol

In an age of rampant liberal triumphalism, Jenny Edkins and Albert Paolini seek to reclaim the political for an IR discipline that is increasingly apolitical or even antipolitical. Edkins demonstrates that poststructuralism broadly defined offers a "route to repoliticization" (p. 16) for IR by drawing our attention back to the artificial and contingent character of the taken-for-granted of the social order. Paolini examines "the political implications of the perspectives of postcolonialism in order to make sense of what postcolonialism offers in its account of the place of the marginalized and the dispossessed in the contemporary world" (p. 49). Read together, these two volumes generate a fruitful debate: Edkins straightforwardly lays out some of the major poststructural arguments about subjectivity, while Paolini challenges them to be thought about more reflexively and subtly under conditions of postcoloniality.

In a slim but by no means slight volume, Edkins challenges IR to take more seriously the notions of subjectivity and ideology, which can reinvest the discipline with a more overtly political sensibility. She offers a convincing refutation of the standard charge that poststructural arguments disable political engagement; in fact, as she shows, poststructuralism allows us to bring the political back into the study of politics. She offers a remarkably lucid treatment of a series of complex theoretical issues, which renders them accessible to a wide audience. In successive chapters she defines politics, depoliticization, and subjectivity and then examines the decentering of the Enlightenment subject. This is followed by four admirably clear chapters that elucidate the arguments of Michel Foucault on discourse, disciplinary practices, and power/knowledge; Jacques Derrida on logocentrism, the supplement, and deconstruction; Jacques Lacan on the mirror stage and ideology; and Slavoj Zizek on subjectivity and ideology. Through this exegesis, Edkins attempts to shift our thinking from a depoliticized notion of politics as "rational calculability and planning" (p. 9) to a notion of "the political," by focusing our attention on truly political moments. Such moments are obscured or concealed by the notion of politics dominant in IR, which is in important ways "depoliticized" or "technologized" (pp. 9-14). Politics in this narrower sense is equated with "that sphere of social life commonly called politics: elections, political parties, the doings of parliaments and governments, the state apparatus, and in the case of international politics, treaties, international agreements, diplomacy, wars, institutions of which states are members (such as the United Nations), and the actions of statesmen and women" (p. 2).

The truly political moments that Edkins labels "the political," in contrast, are about "the establishment of that very social order which sets out a particular, historically specific account of what counts as politics and defines other areas of social life as not politics" (p. 2, emphasis in original). There are two aspects to this political. The first is "the founding moment of a social totality in revolution" (p. 136) when a new social order is created, such as "the move to totalitarianism in the USSR" (p. 3) or the "events of 1989 in Europe" (pp. 125-6).). As feminists remind us, however, "the political is not limited to grand moments of openness or undecidability"; rather, it compasses "every moment of decision" (p. 5). The

second aspect of the political is thus "the more prosaic moment of the endless process of decisioning" (p. 136). Edkins quite convincingly argues that the poststructural concepts of subjectivity and ideology can contribute to the investigation of such political moments.

Paolini's volume, which is less sharp and so a more difficult read, also tackles questions of politics, subjectivity, and agency, specifically with respect to postcoloniality and the possibility of resistance to domination. As does Edkins, Paolini offers a widely ranging criticism of IR, especially its statism and the fact that it is "excessively Western in sensibility and orientation" (p. 30). But Paolini is more skeptical than Edkins about the usefulness of a postmodern reading of the Third World and of postcoloniality. He recognizes that such an approach is in many ways "apt and useful" (p. 98), but he also notes that it needs ridding of 'the Western baggage that accompanies [it] into the postcolonial" (p. 98); including problems with its notion of identity. He argues that a postmodern reading imports Western disillusionment inappropriately to the Third World, where there might instead be attractions to modernity (pp. 98-9); that it homogenizes and flattens the Third World and its heterogeneity (pp. 100-1); and that its notions of subjectivity, agency, and resistance actually undermine prospects for agency and thus for resistance (pp. 101-2). The poststructural dichotomy of essentialism versus death of the subject, he argues, in fact disempowers people who are just finally finding a voice. As Paolini points out, "the balancing between a postmodern reading and the need to carve out a space for enabling action on the part of postcolonial subjects is an inherently awkward one" (p. 58). His analysis thus questions Edkins's claim that poststructuralism easily offers a political strategy, and Paolini provides a more nuanced and critical analysis of the relationship of postmodernism to agency.

Despite their merits, these volumes have limitations. Both, for example, reproduce the contemporary imbalance between theory and analysis in IR. I repeatedly found myself asking: Where is the stuff of world politics? With the exception of a few examples much too few and much too sketchy—there is no empirical material in Poststructuralism in International Relations (in fairness to Edkins, she has carried out such research elsewhere). Although the book is undoubtedly useful to IR scholars and students, it is not actually about world politics at all and could more appropriately have been called "Poststructuralism and the Political." Navigating Modernity suffers from a similar weakness. Paolini repeatedly promises us that the abstract investigation of postcolonialism and identity will be applied to Africa, but the empirical analysis recedes like a mirage as we advance through the book. It resolves in the end into a tiny oasis, made up of little more than a few anecdotes and vignettes derived from others' writings. In both volumes, the empirical material is too little and comes too late to be convincing. From Edkins I wished for some extended examples to show how these wonderfully elucidated theoretical concepts might help me with concrete problems of world politics. From Paolini I wished for the theory to be pulled together, as promised, in the African case.

Each volume also has at least one major theoretical drawback. Although Edkins recognizes that the political includes both the founding moment of a social totality and the more prosaic moment of decisioning, she neglects the latter in favor of the former. That is, she formally recognizes (e.g., p. 128) but practically ignores the political nature of the constant reproduction of and resistance to elements of the social order. In the end, and I am sure it is not what she intended, her defense of the utility of poststructuralism actually seems to limit what poststructuralism can contribute

to political and international analysis. She wants it to encompass the political defined broadly as all constitutive moments and exercises of power, but she really focuses on and seems to privilege the narrower notion. I would argue that post-structuralism is more widely useful than this book implies.

Similarly, while I sympathize with Paolini's emphasis on subjectivity and identity, I am increasingly uncomfortable with the tendency in some poststructural analysis to substitute these concepts for material structures of oppression associated with race, class, and gender. Although Paolini mentions materialist critiques of postcolonialism's culturalism, he effectively dismisses these interventions as subject to the danger of "excessive structuralism" (p. 119). Ironically, this has the effect of displacing the everyday experience of gender oppression, for example, with analysis only of the discursive construction of subjectivity. Something is lost in that translation.

Despite these shortcomings, each volume more than lives up to the promise of the Lynne Rienner series to offer critical perspective on world politics. Both are excellent teaching tools for graduate and perhaps even undergraduate courses. Edkins offers an outstanding introduction to some of the central ideas of poststructuralism; Paolini offers a useful introduction, among other things, to the concept of postcoloniality and the relationship of globalization to the Third World. These volumes, in short, are useful in complementary ways: Edkins provides a crisp exposition of several important theoretical arguments; Paolini's detailed analysis makes possible a sophisticated integration of the Third World into IR, and hence enables an IR worthy of the name.

Elections and War: The Electoral Incentive in the Democratic Politics of War and Peace. By Kurt Taylor Gaubatz. Stanford, CA: Stanford University Press, 1999. 208p. \$45.00.

Zeev Maoz, Tel-Aviv University

Kurt Gaubatz explores the relations among public opinion, electoral politics, and peace and war decisions in democratic states. The study is an outgrowth of the author's doctoral dissertation, but it reads like a coherent and mature book and does not have the standard and technical features often associated with doctoral theses. Gaubatz's starting point is that, despite the large body of literature on the democratic peace phenomenon, there is a "surprising lack of effective work drawing connections between the internal political processes of democratic states and [war and peace] outcomes at the international level" (p. 3). Accordingly, this book explores how democratic politics work to affect national security issues.

The author develops a framework that envisions three major domestic inputs to foreign policy choices: government leaders, opposition elites, and mass publics. The structure and workings of domestic institutions mediate the link between these inputs and foreign policy choices. The focus is on the effects of domestic politics on foreign policy outcomes, but the framework also discusses how foreign governments and the choices of the democratic state interact to produce international outcomes.

The book is organized around this framework, and each chapter focuses on one of its elements. Gaubatz starts by discussing various notions about the relations among public attitudes, elite attitudes, and peace and war decisions. He first dismisses the realist notion that domestic processes have little or no effect on peace and war decisions. At the same time, he discounts naive notions about the "nature" of

democratic publics. His argument, which he labels "protean public," is that democratic publics are neither inherently pacifist nor easily swayed by inflammatory rhetonic. They respond to international events and to their own leaders, and at the same time their attitudes have an effect on leaders' choices.

Leaders interact not only with the mass public in general but also with opposition elites. This interaction sometimes produces outcomes that are opposed to leaders' own preferences with regard to foreign policy. In some cases, leaders who opt for war may refrain from it, even if a small but significant public minority opposes it. Gaubatz's analysis of public opinion and leaders' preferences during the interwar years in Europe, the United States, and Canada provide interesting illustrations of these arguments. The three nineteenth-century cases in which democratic publics displayed prowar attitudes (the Crimean War, the Boer War, and the Spanish-American War) are less convincing. Gaubatz does not succeed in documenting whether leaders entered these wars because of public pressures or because public pressures coincided with leaders' preferences for war.

Gaubatz focuses on elections and electoral politics to analyze the influence of democratic institutions on foreign policy choices. He examines these politics in terms of "election cycles," that is, the potential relationship between the distance from the last election and the proximity to the next election and conflict decisions. Using both the militarized interstate dispute (MID) and interstate war data, he tests for the effect of election cycles in dispute and war behavior. The most significant finding is that "democratic states have entered significantly more wars in the early stages of their electoral cycles and significantly fewer wars in the latter stages" (p. 142).

The general conclusions of the study suggest that democratic politics make a difference in peace and war decisions, but their workings are more complex than presumed by various theoretical approaches. Realist notions of international behavior, other naive notions about pacifist or militant publics, and simple-minded election cycle effects do not seem to account for the nature or the scope of the linkages between democratic politics and war. Gaubatz advocates a contingent approach that takes into account not only the changing moods about wars within democratic publics but also—and this is an important contribution—the ways in which the transparency of democratic politics affects actual and potential opponents. Overall, Gaubatz cautions against sweeping generalizations about democratic politics and war, but he argues that certain limited, although relatively robust, patterns do exist.

The book makes several important contributions to knowledge about democratic politics and war. It offers an interesting framework for studying these relationships. Some may object to certain elements of this framework, but overall it is a good starting point from which one may develop theoretical propositions. It also serves as a primary guide for empirical research. Gaubatz is not wedded to his framework and is willing to reassess some of its elements (e.g., the discussion on the effect of democratic politics on opponents of democracies, pp. 122-4). The book covers a relatively large array of cases, which are generally insightful and well documented. Gaubatz offers a balanced argument about democratic politics and war that rejects naive views of such politics, on the one hand, or a dismissive perspective of democratic effects on foreign policy, on the other. Finally, the book is well written, and the arguments and empirical discussions—especially the historical case studies—flow smoothly and convincingly. This makes the volume potentially appealing to both practitioners and the educated public beyond the specialized scholarly community of political scientists.

The book has some weaknesses. Perhaps the chief among them is methodological. The choice of cases appears fairly arbitrary, although the cases themselves are quite interesting. For example, virtually all involve conflicts between democracies and nondemocratic states (or against rebels in a colonial setting), whereas proponents of the democratic peace proposition often argue that democratic politics matter most (or exclusively) when democracies confront one another. There is no clear structure to the case studies themselves. Each is conducted in relatively free form, although it is set to confirm or disconfirm a theoretical argument. Furthermore, the quantitative analysis is problematic in certain respects. The most important problem concerns the failure to control for other internal or international factors often mentioned in the literature as facilitators or inhibitors of conflict and war behavior. In addition, the failure to consider "opportunities" for conflict makes the interpretation of the findings on election cycles and conflict behavior tenuous due to potential selection bias.

Despite these critical aspects, the balance is quite positive. This is an interesting and insightful start on a very important subject. It will make a very useful addition to the burgeoning literature on the relationship between domestic and international processes.

At Arm's Length: The European Union and Europe's Defence Industry. By Terrence R. Guay. New York: St. Martin's, 1998. 219p. \$59.95.

Anand Menon, University of Oxford

The author sets out to provide a theoretically grounded explanation of the development of European Union (EU) influence over defense industry policy. His core argument is that, despite clear limits, this influence has grown significantly since the end of the Cold War. He employs a "reactive spillover" approach to explain such developments. This displays several obvious similarities with neofunctionalism—a theory of integration that has been around for half a century. First, it "accepts that sub- and supra-state actors are the main agents of integration" (known in neofunctionalist jargon as political and cultivated spillover, respectively). Second, reactive spillover uses the concept of "functional spillover" to explain how integration spreads from one functional sector to another: "Economic integration in other sectors has pulled in (or spilled over into) the defense sector" (p. 179). In addition, however, reactive spillover stresses the role of the international environment—both the changes under way in the international economy and more political factors, such as the end of the Cold War-in determining the pace and scope of spillover in this policy sector.

The book is well researched and provides a host of interesting empirical information concerning the changes that have occurred in European defense industries since the early 1990s. The arguments presented in chapter 2—that shifts in the global economy and the tremendously rapid consolidation of the American defense industry that accompanied it necessitate fundamental changes in the European defense industry—are compelling. More generally, it is refreshing to see the relationship between European integration and defense issues being treated at all in a book-length study, as security issues in general are often ignored by students of European integration.

The book is flawed in several ways. First, it is far from clear that the author marshals sufficient empirical evidence to

justify either his empirical or theoretical claims. Guay did not convince me of his central empirical argument, that is, the EU, and particularly the supranational institutions, exerts significant influence over defense industry policies. Certainly, the EU has started to intrude at the margins, but this is a far cry from the claim that "the ability of the EU institutions to push the integration process . . . is very evident in this area" (p. 176). He cites as evidence the fact that "Commission officials made numerous speeches challenging the existence of Article 223" (the Treaty of Rome article that grants a derogation from the laws governing the common then single market to defense-related products) (p. 176). Yet, Article 223 still exists. This is not "agenda setting" (p. 176) but, rather, an illustration of the continuing ability of the member states, and particularly in this case the larger ones, to block integration when it suits their interests.

The treatment of interest groups also is not convincing. As Guay himself admits (p. 161), it is impossible to generalize from the limited number of interviews carried out with defense industry officials. Moreover, the evidence presented has more to do with the attitudes of these officials toward a putative EU role in defense industry policy than with the influence they have managed to wield over developments in this area. Stating preferences is not the same as ascertaining influence, hence using the claim (possibly empirically unsustainable) that "most defense firms regard a European armaments procurement agency as a desirable objective" (preferences) to illustrate the presence of some degree of political spillover (influence) is simply misleading.

Indeed, so keen is the author to put forward his point of view that one is sometimes drawn to the conclusion that he is pursuing an ideological agenda. The notion that the "economic pressures exerted by a push to form a single European market will eventually engulf the armaments sector" (p. 72) is far from proven. Indeed, one could argue precisely the contrary. The tremendous pressures working in favor of increased consolidation of the European defense industry—American restructuring, increasingly aggressive American competition for export markets, decreasing defense budgets, and more expensive unit costs—have so far led to little integration. What chance is there that this will ever occur?

The book leads one to wonder whether the traditional theories of integration deployed and tested by Guay really serve much purpose in treating the relationship between the EU and this particular policy sector. Numerous detailed case studies have found that elements of both are useful in explaining the development of the EU. Yet, few if any provide theoretically useful indications as to under what conditions this may be the case. Guay's book falls into this precise trap. Reactive spillover seems little more than a statement that international factors matter. How, when, and in what ways are questions that are not systematically addressed. Moreover, traditional theories of integration do not consider in any detail the role of institutions other than the EC/EU in shaping policy outcomes. This may not be of significance in some policy areas, but it certainly is in the sphere of defense. Strikingly, NATO does not figure as an explanatory variable accounting for the absence of a meaningful defense role for the EU.

In sum, the book is a worthy undertaking but fails to convince. We must await a more satisfactory explanation of the relationship between integration and this often overlooked yet both economically and politically crucial policy sector.

American Foreign Environmental Policy and the Power of the State. By Stephen Hopgood. New York: Oxford University Press, 1998. 262p. \$65.00.

Lynton K. Caldwell, Indiana University

Stephen Hopgood argues for the continued relevance of the state in a world of increasing transnational relations and redefined sovereignty. The case in point is the foreign environmental policy of the United States, particularly the politics involved in the United Nations Conference on the Human Environment (Stockholm, 1972) and the United Nations Conferences on Environment and Development (Rio de Janeiro, 1992). The argument is that the state remains the principal and indispensable actor in a world of global interactions among nations.

Hopgood writes: "As will be painfully obvious to the initiated, the book which lies before you began life as a doctoral thesis. Although extensively revised and updated, it has been an impossible task to rid the text completely of that lack of sophistication so characteristic of graduate work" (p. v). I agree with this acknowledgment. The author's style is dense and detailed. The audience for the book must be almost wholly academic—graduate students and university faculty interested in international relations and environmental policy. The substance is limited to interagency politics in the U.S. government with respect to the UN conferences of 1972 and 1992. A detailed account is given of the interactions of the principal persons involved. Personal interviews, position papers, memoranda, and reports are meticulously recorded.

The subject, the state, has multiple meanings, which allows ambiguity. The most straightforward and simple definition is the statement attributed to Louis XIV: "L'état, c'est moi." In chapter 1 Hopgood takes the reader through the various ways in which the state is conceived and interpreted by political scientists. Distinctions and relationships among state, nation, country, and peoples have been drawn differently by historians, geographers, and political scientists over the years. Among the many designations are nation-states, multinational states, city-states, federal states, and unitary states. There are nations without states, but can there be a political state without territory or people (governments in exile)? Even in fiction, imaginary states must have location and population.

The author explains his interpretation of the state in institutional terms as an actor linking domestic and foreign policies. But he observes that these two areas of policy are dealt with very differently, with "foreign issues empowering the core state to the detriment of peripheral state actors and societal and international pressures" (p. 56). Readers from outside the domains of political theory and international relations will likely find Hopgood's designations abstract and theoretical. I interpret his thesis to relate primarily to the core state, which comprises those agencies and officials who are designated policymakers in international environmental affairs. The principal actor in international policymaking in the United States is, of course, the Department of State. Other agencies have inputs in their special areas. Some, notably the Department of Defense, are able to carry on their own international relations in matters specific to their mission. But in dealing with the United Nations, its specialized agencies, and notably international UN conferences such as those in Stockholm and Rio de Janeiro, the Department of State is the state.

The contribution of this book to American international policy lies primarily in the detail it provides of the maneuvering of personal actors within the core state. The book is replete with who said what to whom. Readers primarily interested in the policies actually adopted may be impatient with the plethora of personal specifics and may have difficulty remembering the bureaucratic status of actors when their name pops up many pages after their first identification. The same holds for acronyms. Editorial policy may determine how often the full name of agencies and offices should appear, but the writing style of this book makes for slow reading and too often necessitates flipping back a number of pages to tie together the course of action.

Some of the errors of fact do not damage the scholarly merits of the book but misinform the reader. For example, the National Environmental Policy Act of 1969 did not establish a Council on Environmental Quality (CEQ) in the White House (p. 64). The CEQ was placed by statute in the Executive Office of the President (EOP), which assists the president in his managerial responsibilities and was established on recommendation of the President's Committee on Administrative Management, its officers subject to Senate confirmation. The act that created the EOP also created the White House staff, whose members are not subject to congressional confirmation and are regarded as personal political assistants to the president. Contrary to the text, the CEQ is not "empowered to demand environmental impact statements from other government agencies and departments on major federal projects" (p. 64). That requirement, which I introduced into the drafting of the act in 1969, is mandatory in Section 102(2)c of the act itself and is enforceable in the courts. The CEQ reviews impact statements but is not empowered to demand that the agencies prepare them—the law does that.

Another misstatement on page 64 implies that the Environmental Protection Agency (EPA) was established "under the terms of the Act," an error repeated on page 139. The EPA is a nonstatutory agency created in 1970 by an Executive Order. A British author may be forgiven for misunderstanding the constitutional and political differences between the EPA and the CEQ and between the CEQ and the White House staff. The New York Times and Washington Post regularly misstate the status and relationships of these agencies. Also, a minor editorial lapse is the misspelling of Audubon throughout the text as Audobon.

Despite the detail-burdened text and some avoidable errors, the book has an informative value for anyone interested in the two UN environmental conferences. It identifies source material upon which other investigators may draw. It is not a history of the conferences but is a significant contribution concerning the role of the United States.

Governing the Frozen Commons: The Antarctic Regime and Environmental Protection. By Christopher C. Joyner. Columbia: University of South Carolina Press, 1998. 363p. \$49.95 cloth, \$24.95 paper.

Creating Regimes: Arctic Accords and International Governance. By Oran R. Young. Ithaca, NY, and London: Cornell University Press, 1998. 230p. \$35.00.

Robert Friedheim, University of Southern California

The Arctic and Antarctica are cold, remote, and lightly touched by human use but important to the fate of the world, especially in terms of the effect of possible weather and climate alterations and increased resource use. Those concerned with international institutional creation often cite the polar regimes as examples of successful international resource and environmental regimes. In short, they are "models." The similarities can be pursued for analytic purposes;

for example, both can be characterized as "clubs." Sandler and Tschirhart ("The Economic Theory of Clubs: An Evaluative Survey," *Journal of Economic Literature* 18 [December 1980]: 1482–1521) define a club as "a voluntary group deriving mutual benefits from sharing one or more of the following: production costs, the members' characteristics, or a good characterized by excludable benefits" (p. 1482). The polar regions share the last two attributes. But there are also important differences that other scholars, more commonly non-American scholars, would emphasize as the more important features.

Both Joyner and Young are concerned with the creation, maintenance, and implementation of international polar institutions, but they go about their analytic efforts in very different ways. Joyner is a professor of international law and, not surprisingly, pursues his analysis using the tools of law, history, and international relations theory. Young, a major figure in the new institutionalism, is both theorist and empiricist, a valuable combination. Both have produced books that should be read by scholars concerned with international environmental and resource institutions.

Joyner provides a full and rich description of the development and operation of the Antarctic Treaty System (ATS)the Antarctic Treaty itself (1959), the Agreed Measures for the Conservation of the Antarctic Fauna and Flora (1964), the Convention for the Conservation of Antarctic Seals (1972), the Convention for the Conservation of Antarctic Living Marine Resources (CCALMR, 1980), the abortive Convention on the Regulation of Antarctic Mineral Resource Activities (CRAMRA, 1988), and the protocol to the Antarctic Treaty on Environmental Protection (1991). He shows how states that have made territorial claims and those that have not were able to put aside their differences and "freeze" claims so that agreement could be reached; how opposing sides in the Cold War also put aside differences and cooperated in managing the "frozen commons," including agreeing to inspection schemes and arms control.

Joyner traces the development and operation of a two-tiered governance system that differentiated between the first-class members (my description) (consultative parties, or ATCPs), who have year-round scientific stations in the Antarctic region, and second-class members (nonconsultative parties), who only have "substantial scientific activity" in the region. He also describes efforts to patch the weaknesses in the original treaty, which failed to provide regime capabilities to manage the region's natural resources. The Agreed Measures, the Seal Treaty, CCALMR, and CRAMRA are direct results of that effort. The development in CCAMLR of a secretariat provided for better management capabilities than allowed in the original Antarctic Treaty.

Joyner views the Antarctic as an open-access commons (mixing legal and economic constructs) and its resources as available in a common pool. He argues it must be treated as a commons because claimants fail the test of effective occupation (p. 81). This despite the fact that he provides all the arguments, made primarily by states in the Third World, that the ATCPs exclude them from their commons rights and arrogate to themselves the right to make regime rules. In short, the ATCPs created a club for themselves. He is especially insistent that "no two commons areas are exactly alike" (p. 44), and he tailors his arguments to the characteristics of Antarctica. Joyner describes in great detail the effort under CCAMLR to impose living resource use regulations in the region's maritime space (the Antarctic Convergence), the work to develop environmental regulations and protection rules related to minerals exploitation through the failed effort to promulgate CRAMRA, and the successful effort to replace

it with the Environmental Protocol. He reviews efforts to regulate science and tourism in Antarctica. He is especially helpful in explaining the significant role played by international environmental nongovernmental organizations (NGOs) in recent regime activities. Although he shows many weaknesses in the details of the regime, Joyner clearly believes that the ATS is a significant development. It does a respectable job of keeping or restoring the region to near pristine condition; it is a model for regime-making.

Governing the Frozen Commons is a big, sprawling book that covers its subject in great descriptive detail. At times I disagreed with the author's interpretations on details, but Joyner includes arguments that allow the reader to disagree. He covers the arguments fully and fairly, and for that reason the book is likely to become a standard reference for those interested in the ATS.

By contrast, Creating Regimes is very focused and tightly written. Young brings together two streams of his work: his theorizing about the nature and functioning of international regimes and his special concern with the Arctic. He tells the "creation stories" of two Arctic accords, the Arctic Environmental Protection Strategy and the Barents Euro-Arctic Region. Both are soft law, both are focused upon creating programmatic rather than regulatory institutions. Young achieves something that is relatively rare: He posits a theory about a political process that especially interests him—creating constitutive international environmental regimes—and tests his analytic construct with data in order to reach empirically validated conclusions. As a result, within the caveats he provides, he makes a wholly convincing case that the two Arctic regimes can be explained using his framework.

Young argues that "conventional" international bargaining analytic tools are inadequate for understanding his stories of the successful formation and implementation of the two regimes. He believes efforts to analyze the regime formation process as a seamless whole are doomed to failure. Instead, he claims what is needed is multivariate analysis. He proposes assessing the two regimes by breaking down the process into three phases: agenda formation, negotiation, and operationalization. Each has different political dynamics, they sometimes overlap, and successful movement from one phase to another is not guaranteed. Using these tools allows Young to show that ideas are important in the agenda formation stage and sometimes acquire a momentum of their own; in the negotiation stage, building consensus is more important than putting together a winning coalition; and in the operationalization phase, which is also a political process, the problem is usually a struggle to obtain the material resources to fulfill the constitutive contract, but the effort is bifurcated into domestic and international levels. For each stage he examines driving forces, players, collective-action problems, context, tactics, and design perspectives.

The way in which Young fits his evidence to his theory is a model of social science analysis. His analytic framework allows him to refine notions for which he is already well known, such as the role of leadership and entrepreneurship in regime formation, issue framing for collective choice, and many others. He adds to it an emphasis on the importance of context for explaining why some ideas get onto the international agenda and others do not. Finally, his work on operationalization has made a significant advance in answering the question of why some agreements succeed and others do not, with its emphasis on understanding both domestic and international aspects of operationalization.

Is Young's latest work a useful guide to further advances in the field? He is appropriately modest and does not make exaggerated claims. Despite his caveats, I am concerned that his cases are a bit too perfect, too similar to prove that his analytic construct will fit other types of regime constitutive processes. First, as he acknowledges, only Arctic states attempted to play a role in these regimes; NGOs played a minor role, especially as compared to their role in the Antarctic. The Arctic is a "club" composed only of members who wanted to get in. One can argue that, owing to Arctic Ocean circulation patterns that distribute persistent organic pollutants (POPS), all states and peoples have a stake in Arctic environmental health and that the regime should not be a club, but only those who share close characteristics or expected excludable benefits were involved in creating the Arctic regimes.

Second, context heavily affects Young's cases. The end of the Cold War made both Arctic regimes possible. Can we expect events as meaningful as this to happen often enough to facilitate creating international regimes?

Third, the regimes in question created programs of action, which are inherently less conflictual than regulatory or allocative regimes. The main problem involved resources; the Russians had trouble coming up with the resources to follow through, and the Americans were reluctant to do so.

Finally, I am skeptical that others will make similar findings using Young's analytical construct applied to more conflictual regime formation constitutive processes. Two cases come to mind—the law of the sea and the whaling regime. In both cases the effort to build a winning coalition rather than an effort to build consensus is more likely to capture their core political processes.

But being the fine scholar he is, Young makes his claims in a manner that is testable. It will be up to another generation of scholars to validate them.

Zones of Peace in the Third World: South America and West Africa in Comparative Perspective. By Aric M. Kacowicz. Albany: State University of New York Press, 1998. 267p. \$21.95.

Edmond J. Keller, University of California, Los Angeles

This exercise in comparative international relations analysis takes a regional perspective rather than a dyadic, diachronic, and evolutionary approach. Kacowicz conscientiously attempts to stand apart from traditional approaches to international relations by endeavoring to explain prolonged periods of interstate peace as opposed to war. He argues that, because conventional theories of international relations (i.e., realism, liberalism) ignore the "phenomenon of zones of peace," there is a need for studies such as this one, which provide "a more complex, eclectic, and original explanation that links international and domestic politics" (p. 2).

Kacowicz defines a zone of peace as "a discrete geographical region of the world in which a group of states have maintained peaceful relations among themselves for a period of at least thirty years—a generation span—though civil wars, domestic unrest, and violence might still occur within their borders, as well as international conflicts and crises between them" (p. 9). He differentiates among three gradations of peace: (1) negative peace, or the mere absence of war; (2) stable peace, that is, no expectations of interstate violence in the region; and (3) a pluralistic security community of nations, in which member states share common norms and are deeply interdependent. Rather than deal with regional relations throughout the world, Kacowicz concentrates on the Third World, in particular South America and West Africa. He suggests, however, that his analytical model can be used to study other regions as well.

The book is divided into five chapters. The introduction deals with some definitional and conceptual issues. Chapter 2 lays out the basic arguments and critiques relevant theoretical literature on the conditions for peace and the causes of war at the state-state, regional, and international relations levels. The author presents a series of competing explanations (realist, liberal, and national territorial satisfaction) for the maintenance of zones of peace by assessing several necessary, sufficient, and favorable conditions for the persistence of different gradations of regional peace. The chapter concludes with a sketch of the theoretical model around which the rest of the discussion is organized. Kacowicz argues that war and peace are overdetermined, and there is no one explanation for either. He suggests that there are myriad alternative explanations to account for these highly complex phenomena (p. 63). He goes on to formulate nine explanations for the maintenance of regional peace, grouped into three basic categories: realist, liberal, and domestic territorial satisfaction. In the process of applying the model, the author indirectly attempts to join the debate over the causes of the "democratic peace." He argues, and presents evidence to suggest, that although democracy may be a sufficient condition for explaining the causes of a zone of peace, it is not a necessary condition.

Chapters 3 and 4 are cogent case studies that employ a structured focused comparative method similar to that used by Alexander George ("Case Studies and Theory Develop-ment: The Method of Structured Focused Comparison," in Paul G. Lauren, ed., Diplomacy: New Approaches in History, Theory and Policy, 1979, pp. 43-68). În the case of South America, Kacowicz attempts to understand why that region has been a zone of peace for such a long time (1883-1996). For West Africa he asks: "Why have there been no wars between states in the region, while so much domestic violence has taken place within them?" The author tediously applies his theory, frequently reminding us that multiple competing explanations might explain this phenomenon (e.g., existence of a regional hegemon; level of economic development; incidence of democratic regimes; geopolitical considerations; strength/weakness of the state; compatibility of major values).

In the final chapter, the author turns to cross-regional comparisons of Latin and Central America; West Africa and other African regions; South America and West Africa; and the regions of South America and West Africa with the Asian zones of peace (northeastern and southeastern Asia). Here, Kacowicz's reach exceeds his grasp. The author does a fairly decent job of structuring the comparison between West Africa and Latin America; he had more empirical data with which to work. He finds that by the end of the period under study, Latin America had entered a condition of widespread stable peace, and it was on the brink of evolving into two pluralistic security communities (MERCOSUR and the Andean Pact). In contrast, West Africa was still at the stage of negative peace. The explanation for the zones of peace in both regions is found to be the satisfaction with the territorial status quo. Also, in neither region was there found to be a balance of power. Regarding the other comparisons in the last chapter, the author can only be suggestive. The argument is logically constructed, but Kacowicz is not able to bring to bear sufficient empirical evidence for the reader to evaluate the effectiveness of applying his model elsewhere. Perhaps he will address this problem in a later research project.

Although this book does not represent a major contribution to the study of international relations, it would serve as a nice device for provoking discussion and debate in courses on comparative international relations. The style is occasionally somewhat cumbersome and the presentation tedious, but there is valuable information throughout and a certain amount of original thinking. Perhaps the greatest limitation is that, by trying to explain extended periods of peace, the author is unable to deal with the implications of the regionalization of international relations following the Cold War. As David Lake and Patrick Morgan show in their edited volume (Regional Orders: Building Security in a New World, 1997), with the end of the Cold War the way has been open for the development of broader and deeper interstate relations in various regions of the world. The tendency now is to enter into regional security pacts not only for political purposes but also for military, economic, and developmental purposes.

International Management of the Environment: Pollution Control in North America. By Emmett N. Lombard. Westport, CT: Praeger, 1999. 198p. \$59.95.

Robert Paehlke, Trent University

Lombard presents a timely and generally competent review of post-NAFTA environmental policy and administration in North America. An assessment of the complex changes in pollution control within and among the United States, Canada, and Mexico since NAFTA is an ambitious undertaking, and this book is a useful beginning. Broadly speaking, the treatment is better with regard to the United States and Mexico than with regard to Canada or at the level of integrated administration, but even where it is less thorough than it might have been, the book is reasonably effective.

One strength is that the post-NAFTA changes in environmental policy in North America are generally treated within the broader context of economic globalization. The recent trend to voluntary controls within all three nations has serious shortcomings but is nearly irresistible in a global context of accelerated product development and just-in-time manufacturing that create a "pace of change . . . that is much faster than the processes of regulatory agencies" (p. 9). Lombard also briefly discusses recent public doubts in all three nations regarding any positive role for government and the power this places in the hands of organized interests.

The book is also well structured and quite comprehensive. It considers national environmental protection efforts in each of the three nations (chap. 2) as well as the responsibilities and actions of provincial, state, and local governments within each jurisdiction (chap. 3). Chapter 4 discusses public participation and nongovernmental organizations, and chapter 5 considers several examples of regionally based environmental controls, such as the Great Lakes Regional Toxic Emissions Inventory and the Border XXI Project. The latter is directed at the notorious environmental problems especially but not exclusively on the Mexican side of the U.S. border. Lombard, writing in 1998, is unable to draw firm conclusions about the effectiveness of the Border XXI project, but he does note that pollution control expenditures within Mexican border states have increased significantly in recent years.

Nearly absent from the discussion of regional initiatives, however, is the North American Commission for Environmental Cooperation (NACEC), the multilateral agency created by the North American Agreement on Environmental Cooperation, the so-called NAFTA Side Agreement initiated by President Clinton in 1993. Lombard argues that such "consensus-seeking" entities, this one composed of the environment ministers of the three nations, "have only limited usefulness as dispute mechanisms" (p. 8). He notes that public (as distinct from governmental) initiatives are very

limited within the rules of NACEC, but he does not examine in detail the several cases in which this body has failed to act when it might have. He also does not discuss the ways in which NACEC has at times perhaps had a positive effect (as from the publicity obtained for its ranking of total pollution emissions within subnational jurisdictions).

The book might have been improved by more detailed information on recent reductions in environmental protection expenditures by the Ontario and Canadian governments. Lombard notes (p. 168) the importance of accurate emissions inventories and other forms of environmental information in an era of voluntary reductions and decentralized enforcement, but he fails to comment on the scale of post-1995 radical cutbacks to environmental protection agencies within Canada's industrial heartland. These were not unrelated to declines in provincial and federal revenue in the early 1990s, which in turn could be linked to the process of economic rationalization accelerated by NAFTA. Despite the recent return of Ontario to prosperity and the achievement of a federal budget surplus, the cuts have not been restored due to the struggle to harmonize Canadian tax rates with those south of the border.

Aside from these relatively minor limitations, this book makes a very important contribution. It provides one of the closest comparative examinations of environmental protection in all three nations, and through that window can be seen many of the issues that the world will face as economic integration and trade regimes advance. Lombard provides a balanced view that assumes neither a runaway race to the bottom nor a smooth and automatic advance of environmental protection, proceeding in lockstep with economic growth. He documents a general retreat in public participation opportunities, but he sees as well that some new regional and voluntary approaches to environmental protection are not devoid of opportunities for improvement, especially perhaps in Mexico, where there is a very long way to go. Overall, this book provides us with a better sense of what works and what does not in terms of offsetting some of the possible negative environmental pressures inherent in global and regional economic integration.

Justice, the State, and International Relations. By Leo McCarthy. New York: St. Martin's, 1998. 277p. \$65.00.
Justice among Nations: On the Moral Basis of Power and Peace. By Thomas L. Pangle and Peter J. Ahrensdorf.
Lawrence: University Press of Kansas, 1999. 362p. \$45.00.

Louis Rene Beres, Purdue University

If justice is to be found anywhere in global society, it coexists with mountains and mountains of corpses. A terrible narrative without evident closure, the human record of war and atrocity is now so enduring and overwhelming that even a brief mention of justice seems out of place. Yet, strange juxtapositions need not blot out all hope, and there is ample reason to recall and reaffirm a line by the Greek poet Odysseus Elytis: "Those who committed evil have been taken up by a black cloud" (Odysseus Elytis, Selected Poems, 1981, p. 42).

Thomas Pangle and Peter Ahrensdorf are committed to the imperative centrality of justice in global society and to the associated relevance of political thought. In a very carefully constructed and systematic assessment, *Justice among Nations* self-consciously seeks a proper reconciliation of political theory with the discipline of international relations. Moreover, they seek to provide "a synoptic, critical introduction that would clarify the distinctive core of each of the most important philosophers' conceptions of international justice, that would indicate the historical relationship among these alternatives, and that would bring them into mutually illuminating juxtaposition and contrast" (p. 1).

The effort succeeds admirably, combining a graceful look at the contemporary forces of globalization with in-depth consideration of pertinent intellectual history, from Thucy-dides and Plato to Morgenthau and Waltz. As the discipline of international relations has regrettably distanced itself from the great works of political theory, Pangle and Ahrensdorf make a strong case for narrowing this distance and returning to an inquiry with moral compass. The chapter entitled "Classical Cosmopolitanism: The Stoics and Cicero" is especially useful, as it brings the reader back to an important discourse that is too often ignored by current students of international relations.

Justice among Nations deals extensively with ideas of natural law, ideas that are at the very heart of Leo McCarthy's elegant work. Similar in theme and substance to the Pangle and Ahrensdorf work, Justice, the State and International Relations offers a defense of natural law tradition, attacking both realism and cosmopolitanism as theories of international justice. McCarthy is most compelling and original in his conclusion that "the fundamental principles of international obligation and the fundamental principles of the legitimacy of the state are one and the same" (p. 242). Rejecting the pessimism of the realists, McCarthy offers as best justification for the state one that is essentially pragmatic: It is better positioned than any other political institution to uphold rights and justice.

Both books display substantial lucidity and erudition. Both cover very much the same intellectual terrain, and both accomplish this task in a comprehensive and aptly analytic fashion. Justice among Nations is a more sensible choice as a course text because it brings together a broader array of writers and topics. Furthermore, it looks closely at just war doctrine, considering even Islamic and Jewish perspectives.

In the chapter entitled "The Cosmopolitan Theory of International Justice," McCarthy considers briefly the World Order Models Project (WOMP). I was an early participant in this ambitious academic effort in the late 1960s and early 1970s, and while reading Justice, the State and International Relations I recalled the allegedly "pragmatic idealism" of WOMP, the persistent denunciations of Westphalian international relations, and the more or less insistent calls for global "central guidance mechanisms." The proposed mechanisms of central authority were pretty much the old world government arguments clad in new garments, and in the final analysis, WOMP left the fundamental element of world order reform unreformed, that is, the individual human being. "Out of timber so crooked as that from which man is made, nothing entirely straight can be built" (Immanuel Kant, "Idee zu einer allgemeinen Geschichte in weltburgerlicher Absicht," 1784). Kant's wise remark must be the starting point of all purposeful enquiries into justice and world politics. The world order can never be improved by structural alterations alone; rather, the key imperative of those who seek justice must be to change people.

Both of these books would have benefited from more explicit investigations of human nature and its potential mutability. A principal motif in both is that world politics is always about the struggle for power. Although such thinking is assuredly correct, it also misses a far more important point. The struggle for power in world politics is always epiphenomenal. It is what underlies this struggle, what animates competition between states and human rights violations within states, that represents the problem of world politics. I refer to

the individual's private apprehensions, needs, and terrors, including the desperate need to belong to groups and the often all-consuming (and related) fear of death.

"Everything in this world exudes crime," said the French poet Baudelaire, "the newspapers, the walls and the face of man." Yet, this "face" does not belong solely to what Grotius called "Men of deplorable wickedness" (Hugo Grotius, De iure praedae commentarius, Commentary on the Law of Prize and Booty [1604], trans. G.L. Williams, 1964, p. 90). The real problem of world politics and justice is not the hostes humani geners, or "common enemy of mankind," but the "normal" human being, the one who adheres closely to societal expectations while secretly dreaming of corpses. It was this human being, not the wicked monster of traditional scholarship, who made possible the previous century's many wars and genocides.

Both of these books represent very capable and carefully researched inquiries into justice and world politics. Both, however, could have benefited from a closer look at the essential human underpinnings of world order, the more or less remediable behavioral bases of international relations. Cicero set forth the classic statement of reason and natural law, but it was Nietzsche and Dostoyevsky who recognized that justice must always coexist with unreason and lawlessness.

Risk-Taking in International Politics: Prospect Theory in American Foreign Policy. By Rose McDermott. Ann Arbor: University of Michigan Press, 1998. 242p. \$39.95.

Donald A. Sylvan, Ohio State University

Rose McDermott addresses a significant topic in her work on the applicability of prospect theory to the study of American foreign policy. The book examines such important issues that it should be recommended to students of foreign policy decision making, political psychology, and international relations. It makes an important contribution to our understanding of risk taking in international politics. Greater precision in applying many of the concepts of prospect theory would have made the book even more useful.

To her substantial credit, McDermott offers a context for efforts to understand and evaluate prospect theory. She carefully presents overviews of the Iranian hostage rescue mission, the decision to admit the shah to the United States, and the U-2 and Suez Crises. McDermott is on track in her implicit assumption that for prospect theory to be useful to students of political science and international relations, it must be interpreted and tested with cases such as those she examines. The four cases offer a good range of variation on some important variables, including the president in office. Ideally, cases that could be classified as noncrisis would have been included as well.

In the concluding chapter, McDermott raises the issue of operationalization. To her credit, she acknowledges that this is a troubling issue for the application of prospect theory to the study of foreign policy. Yet, that does not excuse her problems in this area. Specifically, two of the scope conditions of the initial statements of prospect theory by Daniel Kahneman and Amos Tversky ("Prospect Theory: An Analysis of Decision under Risk," Econometrica 47 [1979]: 263–291) are that choices must be seen to be in the same domain and must be seen as close. McDermott does not establish this in all her cases. In fact, it was not clear in all the cases exactly what specific choice was being examined. Therefore, she may not be testing prospect theory per se.

As to the role of risk in prospect theory, McDermott and I

again interpret the theory a bit differently. She assumes that the assessment of the riskiness of a choice is a major part of prospect theory. I interpret the theory as stating that risk influences how individuals weight options. The difference may be subtle, but one would look at different evidence to evaluate the theory's performance depending upon which interpretation one chooses.

McDermott makes a good point in arguing that risk can be seen as partly in the payoffs and partly in the probabilities. A crisper specification of that point in the analysis of the cases would have improved the book. Also, McDermott's treatment of prospect theory as rather deterministic is problematic. In the initial specification of prospect theory, it is set forth much more probabilistically.

In the concluding chapter, McDermott tells us that, "in many ways, prospect theory does not so much simplify political analysis as complicate it" (p. 184). I wholeheartedly agree. Prospect theory should be employed as an adjunct to other important theories and variables that help us understand and explain decisions. When we invoke "prospect theory" in our explanations, however, it should not be in a simplified form. It should be the more subtle version set forth by Kahneman and Tversky, and we should only accept those portions of the theory for which we have empirical verification in the domain of our investigation.

A comprehensive treatment of prospect theory in the context of foreign policy and international politics is long overdue. Risk-Taking in International Politics makes important strides in that direction. This is a good book that could have been even better if it had been somewhat more precise in its application of prospect theory.

Leading Questions: How Hegemony Affects the International Political Economy. By Robert Pahre. Ann Arbor: University of Michigan Press, 1999. 277p. \$49.50.

Brian Pollins, Ohio State University

For ten years following the publication of Robert Keohane's After Hegemony (1984), the subject of system leadership was central to the research agenda in our field. But like a declining hegemon, this once dominant topic faded from our literature for reasons not well understood, despite the fact that the question of order (in the global system as well as in our field) remains unanswered in the post-Cold War era. Another look at the question of international system leadership is warranted, and Robert Pahre offers a game-theoretic analysis of hegemony more detailed than we have seen thus far. Propositions from the model are examined in light of four historical case studies (two concentrating on the United States, two on Great Britam), and selected claims are tested statistically against time-series data. Pahre's chosen subject as well as his blending of formal analysis, statistics, and historical cases is reminiscent of David Lake's Power, Protection and Free Trade (1988). Pahre's game-theoretic analysis is much deeper, but Lake's book remains the best in this genre for its clarity and accessibility to a wider audience.

Leading Questions begins with a very broad one: "What does a leader do when it leads" (p. 8)? Others have addressed this question with the breadth it demands, examining the consequences of leadership (and its disappearance) not only for the global economy but also for interstate conflict. Robert Gilpin's War and Change in World Politics (1981) and George Modelski and William Thompson's Leading Sectors and World Powers (1996) are noteworthy, although the latter is not discussed or cited in Leading Questions. Pahre instead centers his attention on one important aspect of system

leadership: the provision of (quasi-) public goods supporting the governance and operation of the global economy. This book is best appreciated if the reader looks for insights specific to the public goods question, rather than the broader sweep offered by Gilpin and others.

Pahre brings us up to speed quickly with the relevant ideas and claims surrounding hegemony and collective goods provision, beginning with Charles Kindleberger's World in Depression, 1929-1939 (1973). We learn that scholars have considered more than ten public or quasipublic goods that might be provided to the global economy by a hegemon (Kindleberger alone treats five of these), including the countercyclical balancing of capital flows, liquidity provision, and currency stability. We are well into Leading Questions, however, before we realize that Pahre's model and empirical analysis center very much on only one of these goods: the provision of a distress market for the economies of other nations during slack times. There is much to be learned on this question alone, but the author should have made this clear and provided some discussion of how the analysis of this one good might be generalizable to others. As written, the argument slides between broad general claims and specific applications and pieces of evidence without adequate foundation. The insights and novel findings that might have been gained from this study are much less clear as a result.

Pahre presents his basic model in chapters 2 and 3. Thereafter, in chapters that alternate between historical narratives (interspersed with occasional statistical tests) and further elaborations of the basic model (to explore security concerns and the implications of repeated play), the argument unfolds, revealing many surprises. Foremost among these is Pahre's claim that Charles Kindleberger and Mancur Olson are wrong to associate the quantity of public goods provided with the concentration of resources in the hands of one or a few (p. 29). The very simple formal model generates what Pahre calls the "nonconcentration hypothesis": The distribution of resources among actors is irrelevant to the quantity of public goods provided to system members. This result flies so directly in the face of so much work on collective goods that it deserves more explanation-particularly for the nonspecialist in formal models—than one finds in Leading Questions. We are told that Olson's model was not so explicitly game theoretic since it assumed independence among the choices made by actors, and this "probably accounts for the difference between [Pahre's] claim and many scholars' intuition about the public goods problem" (p. 30). Probably? Fuller exploration of this and other points is

Might the dramatic difference in the results of Olson and Pahre instead be due to other simplifying assumptions made by Pahre? Since his analysis looks only at the "distress market" aspect of public goods, he elects to use the volume of imports as the lone indicator of public goods provision (by any one nation as well as in the system overall). Since the willingness to import by any one nation does us as much good as any other (e.g., why should we care whether it is the Americans, the Germans, or the Canadians who are willing to import our goods?), Pahre tells us that these national markets are perfect substitutes for one another. This might be useful when it comes to distress goods (formal modelers should be granted a fair degree of latitude in their assumptions, for they do offer a unique clarity in their logic), but it is less clear, for example, that the Canadians are just as capable of contributing to global currency stability as the Americans or Germans. Concentration of resources might matter after all.

Other aspects of the book's presentation get in the way of

our understanding. For example, "formalization... requires an explicit definition of hegemony" (p. 14), yet the reader is hard-pressed to find one. Soon thereafter, Pahre states that he assumes a hegemon has always been present in the system since 1815, so there is "no variation in the independent variable for the key hypotheses of the theory." But whether or not one expects variation in any indicator depends first on one's operational definition of that variable. Hence, the missing definition of hegemony leaves us handicapped from the outset of this study. Pahre shows that falsifiable second-order predictions can still be generated by the model, and he argues that when they are not falsified, we may infer that first-order claims and characteristics of the model are themselves "not false" (p. 15). Many readers will find this means of testing to be overly tortured.

If the model's presentation and the marshaling of evidence are unconvincing, the book remains intriguing. Pahre draws us back to a subject eminently worthy of further study. His application of game theory to this subject is more extensive than we find in this literature, and the conventional wisdom on the subject deserves a challenge as stiff as he attempts. From this challenge come gems for future research. How does the security environment alter incentives for hegemons to provide collective goods? Might some goods be more readily provided to the system than others? Pahre began by asking what a leader does when it leads. This book gives us glimpses of a broader question: When, if ever, does the system require leadership for collective goods provision? Without the model offered here, the question would not be framed this way.

International Order and the Future of World Politics. Edited by T. V. Paul and John A. Hall. Cambridge: Cambridge University Press, 1999. 421p. \$64.95.

Donald J. Puchala, University of South Carolina

This book was inspired by an interdisciplinary dialogue about contemporary international relations between the two editors, T. V. Paul, a political scientist, and John A. Hall, a sociologist. At a conference at McGill University in 1997, they elaborated this dialogue into a conversation among a number of well-known scholars, whose respective contributions became the volume under review. The object of the project was to determine what social science may have to offer to the better understanding of international order after the Cold War.

The book has some notable strengths, although these are largely to be found in the insights of several of the individual contributors rather than in the volume as a whole. There is, for example, a remarkable essay by T. J. Pempel, in which the author extracts conclusions about the future of Japanese foreign policy from his deep understanding of Japanese domestic politics. Japan not only will continue to shrink from leadership in world affairs, but also, Pempel reckons, it will experience intensifying strains from forces of globalization. In his imaginative contribution, Jack Snyder identifies Russia as a "declining power." He asks analogically whether Russia will decline in one of the ways that major powers have declined in the past, and he plausibly concludes that growing marginalization will render Russia increasingly irrelevant on the world stage. Michael Mann's essay on globalization and the fate of the nation-state is a tour de force both analytically and conceptually. Globalization, Mann points out, is best envisaged as adding new networks of human interaction that supplement but do not replace already existing national and local ones. Also impressive is Jack Goldstone's thorough,

plausible, and very discomforting analysis of the effects of demographic trends on the politics of the twenty-first century. Goldstone's essay could well turn out to be the most prescient in this collection.

Readers seeking edification from *International Order* are advised to read selectively. For one thing, the parts do not hang together. This appears to be partly due to the well-intentioned but unsuccessful attempt to bridge disciplines, which results here in juxtaposed discourses instead of productive interdisciplinary communication. Also, there is a rather notable lack of conceptual consistency. Even the definition of "international order" varies from author to author, with some avoiding the idea altogether, and there are differing renditions of such concepts as "state," "nation-state," or "security."

There is also a disjuncture between the theoretical first part of the book and most of the remainder. After the theorists have their say in the opening chapters, what they have said is largely ignored by the rest of the authors. This could well be because the theoretical offerings are, in this collection at least, of little relevance. In the esoteric, elaborate, elegant, formalistic, and abstract ways of international relations theory today, the contributing theorists tell us many things that we already know or that common sense readily recommends. For example, we are told by Michael Mastanduno and others that we live in a unipolar system in which the United States is dominant. Military conflict among the democracies of the "northwestern" quadrant of world affairs is improbable, Michael Doyle explains, because democracies do not fight one another, and among these countries there is precious little to fight about. In their own chapter, the editors rediscover that prudential statesmanship contributes to international order, but exactly how this truism integrates realism and liberalism, as Paul and Hall attest, escapes the reader. Lisa Martin assures us that international institutions matter, although how they matter remains puzzling to her, admittedly, and to others expectantly wanting to know. By taking a constructivist and mildly postmodern stance, Steve Smith contributes the only genuinely imaginative essay to the theoretical section. International orders, as Smith points out, are indeed constructs that can and ought to be deconstructed down to their normative cores.

The fundamental flaw in both the project that produced International Order and in the book itself is that the editors apparently had a position regarding international politics after the Cold War that they wished the conferees to confirm. Clearly, the editors (as well as several of the authors) accept as real that the post-Cold War world is not qualitatively different from the universalized Westphalian system, Bullian "international society," that preceded it. That nation-states remain the principal agents of international politics and that the causes of war and conditions of peace among states must still head the research agenda is the message that the editors wanted to deliver, and their edited volume is structurally biased to deliver it. Why devote nearly one-third of the text to discussing the future of the major powers and almost no space to international organizations, nongovernmental organizations, or international social forces? Why dwell theoretically and substantively upon international politics, largely depicted as geopolitics, and neglect international political economy? Why push ideological, cultural, ethical, and legal issues to the margins of concern, when among many of the world's peoples today these are matters of central concern? Why recruit a corps composed of North Americans onlywell credentialed as they are—to ponder issues of international order, when analyzing the workings of the post-Cold

War world is today a preoccupation of capable thinkers on all continents?

Despite the biases of the Paul and Hall design, glimpses of a very different kind of world politics are detectable in some of the essays. This is a world politics of emergent disorder and institutional and environmental collapse; international agendas are set not by geopolitical aspirations and calculations but by imperatives to respond to internally driven events. It is also a world in which North and South are highly polarized, the distribution of well-being is a primary issue, culture is a sanctuary, ideologies mobilize discontent, and power is diffused among a variety of agents. Of course, states represented by national governments will continue to strut and fret about as they conduct twenty-first-century international politics, but the significance of international politics per se is likely to be much reduced. Some of the contributors to the Paul and Hall volume say this. They are right.

In the Shadow of Power: States and Strategies in International Politics. By Robert Powell. Princeton, NJ: Princeton University Press, 1999. 310p. \$55.00 cloth, \$19.95 paper.

R. Harrison Wagner, University of Texas, Austin

A person who buys this book will get not one book but two. One is a major contribution to our understanding of some important issues that have been debated by students of international politics. At the center of these debates has been Kenneth Waltz's claim that to understand international politics one must understand the implications of the anarchic environment in which it takes place. The other book is an intellectually distinguished contribution to recent debates about the merits and demerits of formal rational choice theory in political science. At the center of these controversies have been Donald P. Green and Ian Shapiro's Pathologies of Rational Choice Theory (1994) and a recent article on the role of formal models in security studies by Stephen Walt ("Rigor or Mortis? Rational Choice and Security Studies," International Security 23 [Spring 1999]: 5-48).

It is hard to deny that states exist in an anarchic environment, which implies that every state must confront the possibility that other states will use force against it. It is also plausible to assume that the leaders of states prefer not to be attacked and defeated by other states, and that they think carefully about what they should do to avoid such an outcome. The neorealist school of international politics, which is largely but not entirely based on the work of Waltz, has claimed to derive strong conclusions about the behavior of states from weak assumptions such as these. Thus, neorealism has always been a theory of rational behavior (although one that is ignored by critics of rational choice theory, such as Green and Shapiro), and Waltz claimed to be emulating microeconomics. Yet, scholars disagree about what conclusions, if any, such assumptions imply. The result is a variety of schools of thought within the neorealist camp as well as competing doctrines, such as neoliberalism and constructivism. The main message of Powell's book is that many of the arguments advanced by participants in these controversies are not valid, and constructing valid arguments about these issues is harder than is commonly assumed.

The book is organized around three ways a state can try to cope with external threats: It can mobilize its own resources, try to compromise, or rely on other states for assistance. An analysis of the first response leads to the question of whether states will want to maximize their power and will be concerned with only relative rather than absolute gains in dealing with other states. Prominent scholars claim that we should expect both to be true. Powell's analysis implies that they will do neither and that a peaceful equilibrium is possible. An analysis of bargaining over the terms of a negotiated settle-

ment as an alternative to fighting leads to an examination of the relation between the distribution of power and the probability of war, the conclusion of which is that neither the balance-of-power nor the preponderance-of-power schools is right. And an analysis of the choice of allies leads to the conclusion that we should not expect states to "balance" against other states, as Waltz and others claim.

Much of this analysis has been published in journal articles. Moreover, the three responses to external threats are discussed in isolation from one another even within this book, since bargaining or alignment are not options in the discussion of mobilization, mobilization or alignment are not options in the analysis of bargaining, and bargaining and mobilization are not options in the discussion of alignment decisions. What unites the analysis of these issues is instead a discussion of the role of modeling in the study of politics, in which these analyses serve as examples. Powell concentrates on making the reasoning in each of these analyses as easy to follow as possible, including the choices that were made in setting up the models. An integrated analysis of how states respond to external threats is left to future work (or, perhaps, as an exercise for the reader).

Powell's discussion of "the modeling enterprise" makes clear that what divides formal modelers from their critics is mainly the emphasis the modelers place on constructing logically valid arguments. In his critique of formal modeling, Walt obscures this issue by focusing instead on "logical consistency and precision" and "empirical validity" as criteria for evaluating scholarship. On the one hand, a logically consistent argument is not self-contradictory. A valid argument, on the other hand, is "so constructed that if the premises are jointly asserted the conclusion cannot be denied without a contradiction" (Random House Dictionary of the English Language, 1987). Arguments can be consistent but not valid, in which case they do not work, since even if one accepts the premises, one is free to deny the conclusion. Walt claims that it is relatively easy to avoid contradictions and points to several nonformal works in the social sciences as examples. No one who reads Powell's book could possibly think that it is easy to construct interesting valid arguments about international politics or overlook the fact that the literature is full of invalid ones.

· One might think that in evaluating explanations logical validity is less important than factual evidence. Yet, only if observed facts can be derived from an explanation can they be explained by it, and only if we have some prior reason to believe the explanation might be true can the facts provide evidence that the explanation is true. Often a deductive argument is the only way these conditions can be met.

Logical validity is not the only desirable property of an argument, but it is a condition that must be met before other criteria become relevant. Of course, before an argument can be shown to be valid, someone must first guess that it is, and therefore the greatest scholars are also the greatest guessers. Not all guesses are correct, however, and, contrary to Walt, expertise in modeling need not interfere with one's ability to make inspired guesses. No one knows for sure how to encourage inspired guesses, of course, but this book is one of the best guides available to what must be done if one wants to determine how inspired any particular guess about international politics really is.

Alliance Politics. By Glenn H. Snyder. Ithaca, NY: Cornell University Press, 1997. 414p. \$39.95.

John Conybeare, University of Iowa

This book is based on the premise that "neorealism does not say much about what causes states in a multipolar system to identify each other as friends or enemies or about the

consequences of such identification" (p. 32). Snyder synthesizes his considerable stock of accumulated wisdom on alliances in easily accessible game theory, illustrated with his impressive historical knowledge of alignments before World War I. He sets out a large array of factors that affect alliance politics, and he shows many permutations of alignment payoffs using simple variable-sum games. He is at his best in offering insights into the diplomatic tactics and motives of the

Overall, however, the book does not offer a great deal that is new. Parts of it are reprints of earlier work. The historical content, apart from Snyder's speculations about tactics and motives, is easily available from the many diplomatic histories of the period. Considered as a work of theory, the book is less satisfying than one might hope. There is the familiar expected utility equation with the usual variables (p. 47) but (unlike, say, the work of Bueno de Mesquita) there is no indication of how such an equation might be tested. Once into the case studies, Snyder assigns payoffs to alliance behaviors, but these numbers appear to be rather arbitrary and are not

obviously tests of the expected utility equation.

Snyder offers general hypotheses on alliances (p. 74), from which are derived the hypotheses that he applies to his cases. The problem is that these hypotheses are not particularly remarkable: "The least dependent partner will enjoy the most influence," "allies with the most interest at stake in bargaining encounters will have the most influence," and "the more firmly a state is committed to defend its ally, the less influence it will have in intra-alliance bargaining" (p. 199). These theories are rather close to tautologies of the following type: In the absence of imperfections in alliance markets, efficient allocations of resources will ensue (i.e., resources will go to those who value them the most). Tautologies are not necessarily useless, since they may direct us to the most interesting aspects of a phenomenon (e.g., if we know that banks would not exist in the absence of costly information, this tells us to look at exactly how banks overcome gaps in information). Nevertheless, the hypotheses that Snyder tests with his intuitive measurements are likely to disappoint some

This book exhibits a deep appreciation of diplomatic history, and it is a splendid work of historical commentary that offers many interesting inductive generalizations. It is also a gold mine for scholars of alliances who are looking for issues worthy of further explication, either historically or theoretically. Students will also find it valuable because it pulls together many of the recent developments in the alliance literature (e.g., security and autonomy tradeoffs), although some of this contemporary work is accepted rather uncritically. In discussing "balancing" and "bandwagoning," for example, Snyder seems to assume that one knows which of these motives one is observing, but frequently this may be unclear. When Rumania joined the Tripartite Pact in 1940, was it bandwagoning with Germany or balancing against a Soviet Union that had already devoured parts of it neighbors?

Regional Orders at Century's Dawn: Global and Domestic Influences on Grand Strategy. By Etel Solingen. Princeton, NJ: Princeton University Press, 1998. 334p. \$72.50 cloth, \$20.95 paper.

Richard Rosecrance, University of California, Los Angeles

Solingen has written an extraordinarily interesting book. Rarely are we offered new independent variables that help explain or even determine typical international relations outcomes war, militarized disputes, peace, or economic relationships—between states. Solingen has done so. Her

"Internationalist coalitions" are an important arrow in the quiver of causation as applied to grand strategy and regional conflicts. That domestic variables are important influences on grand strategy has been shown previously. Stein and Rosecrance, Kupchan, and Snyder have already made this point. What Solingen does is focus on internationalist or liberalizing coalitions in the explanatory process. In her analysis these turn out to be more important than democracy, institutionalism, or realist factors in determining grand strategy. Rather than concentrate on a "large n" analysis that might apply historically, Solingen seeks to explain conflict and cooperation within regional orders under conditions in which there is (1) an integrated global economy, (2) a web of international and regional institutions, and (3) a revival of nationalist and confessional influences. In other words, she attempts to account for the international system at the onset of the twenty-first century. Her focus is the southern cone in Latin America, the Middle East, the Korean peninsula, and by application South Asia, Russia, Taiwan, and China. Her argument is that internationalist coalitions are not the necessary condition for peace among regional-neighbors, but they are the sufficient condition.

Democracy, institutionalism, and realism do not meet this standard. Democracy and institutionalism are supposed to act to prevent war among regional neighbors, but war can nonetheless occur between weak or unstable democracies (India and Pakistan), and conflict can take place in the Middle East despite the heavy network of institutionalism of the Gulf Cooperation Council and the Arab League. Realism is supposed to expect conflict unless there is a local nuclear balance. Yet, conflict has not occurred between North and South Korea, even though the South Koreans gave up nuclear ambitions in 1975. Conflict has not taken place between Taiwan and China, even though Taiwan got out of the nuclear business in the 1970s. If restraint still exists, it is because of the development of internationalizing coalitions, inchoate on one side and well established on the other, that link the contending parties.

Solingen accepts the conclusions of Mansfield and Snyder that democratization is a conflictual process, with a plethora of log-rolling coalitions domestically. Stable authoritarian nations inhabited by liberalizing or internationalizing groups, however, may be much more peaceful. From her standpoint, the American pressure for democratization and human rights is a mistake if there is no internationalizing coalition in place. One worries about Russia because a quasi-democracy has been established without a powerful liberalizing coalition. This is not true for most of Eastern Europe, although it does apply to Serbia.

Logrolling emerges in a cartelized polity in which there is no solid, largely unchanging moderate majority for policy. Extremist outcomes can then emerge that are favored by no particular faction in the logrolled coalition. Under presentday circumstances, statists and confessionals may establish a joint log roll, but agreements with liberalizers are more difficult to form. Sometimes they can agree, however, as in the case of Pinochet's Chile, where the military associated with economic liberalism. De Gaulle's France could liberalize economically because the defeated military in Algeria were paid off with higher arms spending. A somewhat similar logroll occurred in Indonesia. Such coalitions are not likely to endure, however, because liberalizers seek ultimately to cut the military budget and to persuade the armed forces that a stronger economic mobilization base is more important than more military spending. Solingen makes a persuasive case that the instances of denuclearization detailed in Mitchell Reiss's Bridled Ambition (1995) are largely to be accounted

for by liberalizing coalitions in place or influencing those in power.

There seems little doubt that internationalist coalitions exert an important effect upon the possibility of peace. But what explains the origin of such coalitions? On this point Solingen is less clear. In the case of Taiwan and South Korea, it was the sudden check administered by the United States to a nuclear and statist strategy. In other cases it was the reinforcement or insistence by important international agencies, such as the IMF and the World Bank, that countries internationalize their economy if they wished to gain external financial support. In some instances "emulation effects" may have been dominant, in that countries observed the success of such nations as Japan, Singapore, and Germany and wished to follow in their wake. In still others, the possibility of becoming a "virtual state" with high-level services concentrated at home and production abroad may have attracted attention as the rewards of such status grew. In certain cases an economic crisis like that of 1982 in Latin America and 1997-98 in East Asia stimulated liberalization to surmount

Solingen, however, is in no doubt that states follow a risky course as they enter the "valley of transition" from a statist/ confessional coalition to an internationalist one. Military spending will be cut, and state industry will be put out of business, with attendant unemployment. Until foreign investment enters and new export industries develop, political and economic uncertainty will govern the outcome. As one paramount example of Solingen's point, China is now entering a "valley of transition" in both Przeworskil's and Solingen's terms. Despite the recent crisis, China did not suffer a major economic reverse, which definitively moved Chinese politics in a liberalizing direction. Beijing possesses both a statist coalition and a liberalizing one. In the Korean case, the United States administered a check to the statist group in the mid-1970s, but it can hardly do this in China. Such an attempt would more likely guarantee the success of statism and the defeat of liberalization. More important is that other countries help the Chinese leadership develop incentives (Ilqui bonoll gains), which can convince statists and the unemployed that their longer term benefit resides on the side of further economic liberalization. Pressing for political liberalization and human rights, however, despite the democratic peace hypothesis, can only be counterproductive. Solingen has written a book that not only is generally correct but also has implications that can inform policymakers in many parts of the globe. Economics first, politics second.

In the Hurricane's Eye: The Troubled Prospects of Multinational Enterprises. By Raymond Vernon. Cambridge, MA: Harvard University Press, 1998. 262p. \$35.00.

Randall J. Jones, Jr., University of Central Oklahoma

This is Raymond Vernon's last book, published the year before his death, and it is a fitting literary conclusion to a long and distinguished career. It demonstrates once again the insightful, balanced analysis to which students of multinational corporations have become accustomed in Vernon's writings. For more than a quarter-century Vernon focused on the uneasy relationships between multinational enterprises (MNEs) and national governments. He recognized that tension is inherent in these relationships but argued that it must be kept to manageable levels. That could best be achieved, he believed, by maintaining balance—equilibrium—in patterns of reciprocal influence between firms and governments. That goal was always elusive, however, and there likely was never

a time when he thought that such equilibrium existed. In his 1971 classic, Sovereignty at Bay, Vernon documented the growing power of American multinationals and was concerned that the scales were weighted too much in their favor. This theme continued in later work as well.

By 1998, however, Vernon's primary concern seemed to shift. In the Hurricane's Eye highlights his belief that the balance may be tilting too far against MNEs. In fact, his main argument is that MNEs are threatened by unprecedented tensions and pressures—primarily from governments—that may impede their effective functioning. Vernon sees this as a particularly troubling prospect because MNEs have been, and continue to be, the engine of world economic growth.

Vernon's earlier work emphasized problems in dealing with host governments, but today the greatest source of tension for MNEs is the relationship with their home government. Vernon sees negative home-government policies toward MNEs as being largely driven by unions, mobilized in response to the loss of jobs to foreign workers and the perceived threat to the labor safety net. He also observes a more fundamental demographic problem, the aging population in many industrialized countries in which MNEs are based. Jobs for working-age people are critically needed to support nonproductive elements of the population through old-age insurance programs. Growing awareness of this consequence of jobs lost to foreign investments has meant that labor's concerns find support among other key groups in industrialized countries. From this perspective Vernon is pessimistic as to the outlook for MNE-government relations, for the problem is endemic in society and may be beyond the control of either.

Vernon also stresses the negative consequences for MNEs of conflicts among governments seeking to exercise jurisdiction over them. What are companies to do when the home government attempts to impose jurisdiction over their foreign subsidiaries, especially to promote unrelated foreign policy goals, which then cause the subsidiaries to run afoul of

host government policies? A persistent, if less dramatic, jurisdictional issue concerns taxation. Progress has been made through international agreements to avoid double taxation, but Vernon notes that issues related to the tax consequences of intrafirm transfer pricing remain.

Vernon addressed similar jurisdictional issues in previous work. By the late 1990s, however, he believed the problem was being compounded by additional conflicting demands made on firms by regulatory entities other than national governments—subnational state and local governments, international regional organizations (notably the European Union), and global organizations.

Vernon observes that structural changes in the international environment have been especially challenging for MNEs, notably the collapse of the Soviet Union and the rise of independent, chaotic Russia and CIS states; the wave of democratization throughout much of the world; and the further integration of Europe. One consequence of democratization that Vernon stresses is privatization of government-owned firms. Privatization has provided MNEs significant opportunities but at considerable risk in sensitive sectors. Vernon clearly is concerned that MNEs investing in petroleum, utilities, and banking may have forgotten the lessons of the 1970s: Nationalization is always a possibility if nationalism is rekindled. This eventuality cannot be ruled out, given the fragile economies and equally fragile democratic institutions in many countries.

The bottom line of Vernon's analysis is that MNEs are vulnerable, and a well-functioning international economy requires that tensions with governments and other political entities must be effectively managed and, it is hoped, reduced. This book's emphasis contrasts with Vernon's earlier stress on the power of MNEs, but *In the Hurricane's Eye* underscores that Vernon was always a pragmatist, not committed to preconceived notions, and willing to reinterpret concepts in the light of changed circumstances.



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Editor's Notes

By now most readers will know of the New York Times article on November 4, 2000, reporting a protest against the American Political Science Association in general and the American Political Science Review in particular. The protestors generated a letter signed by 124 scholars, which they sent to the Times and circulated to many colleagues.1 A week later, 98 more signatories were added. The letter raises questions about APSA governance and complains about APSR articles and the equity of our review process. As I have not been included in the protestors' discussions, there may be points of which I am unaware and which might have affected my thinking on the issues. The letter, however, mischaracterizes articles in the APSR and contains substantial misunderstandings about Review procedures and outcomes.

The major question asked in the letter is: "Why does the APSR and why do other prominent professional fora seem so intensively focused on technical methods, at the expense of the great, substantive political questions that actually intrigue many APSA members, as well as broader intellectual audiences?"

Speaking only about the APSR, I would make several points.

Yes, many of our articles use advanced statistical techniques and mathematical modeling, but not "at the expense" of dealing with the great political issues. Rather, the intent is to obtain more systematic and reliable knowledge about them.

In the past five years, we have published articles on human rights; international conflict and war; the tendency of democracies to be at peace with one another; the conflict in Bosnia; public attitudes toward war; international trade; political inclusion and democratization; interethnic cooperation; political processes in the European Union; the conditions of stability in parliamentary systems; political participation and the beginnings of a competitive electoral system in China; democratization in Mexico, Africa, Eastern Europe, and the former Soviet Union; political change in Japan, Latin America, and Israel; the interrelationship of international crises and domestic policies; how different political institutions affect satisfaction with democracy; electoral systems and how they develop; the number of parties under various electoral systems; minority voting rights and majority welfare; minority representation in the U.S. Congress and how best to achieve it; voting rights enforcement; attitudes toward environmental issues; attitudes toward welfare; school choice and its effects on social capital; the effect of jury size on convictions; campaign strategy and voter turnout; the effect of mass media on political attitudes and behavior; public reaction to Supreme Court decisions; political corruption; electoral accountability; organized interests and lobbying; critiques of capitalism; the conditions of

collective action; deliberative democracy; legislative bargaining and coalition formation; strategic positiontaking among elected representatives; how children develop political attitudes; the effect of family life on citizen participation; civil liberties and tolerance; political trust; civic voluntarism; the conditions for self-government; Christianity and republicanism; numerous articles analyzing the positions of both classical and contemporary political theorists on such issues as justice, tyranny, gender equality, the common good, value pluralism, liberalism, and liberty; and many others.

Although many of the articles incorporate technical materials, their topics are the contemporary expressions of the "great, substantive political questions." It is mistaken to conclude only on the basis of numbers or mathematical symbols contained in an article that, ipso facto, the analysis cannot deal with great political issues.

The letter also asks: "Why do the APSA Council and APSR Editorial Board seem to be chosen essentially by their predecessors?" It is hard to guess why things seem as they do to different viewers with varied amounts of information, and evidence bearing on this question should be brought forward by those doing the speculating, not those who are speculated about. The APSR Editorial Board during my tenure was chosen entirely by me, based on my own knowledge of the discipline and on both solicited and unsolicited recommendations received from hundreds of colleagues (some known to me personally and others only professionally) and groups in the discipline. The choice of the editorial board by the editor is the method anticipated by the APSA Constitution and bylaws. One person was selected because he wrote to me saying he had published several articles in the Review and would like to increase his participation by serving on the board. Among my editorial board, none was on the previous board, and only one of the 33 had ever served on an earlier APSR editorial board. During my editorship, the board has been and remains completely independent of any previous editorial board or any council.2

The letter also asks: "Why do so many leaders of our profession not even read, much less submit, to the APSR?" This question needs to be answered by the leaders who do not read or submit, not by the leaders and nonleaders who do. This is a diverse discipline with numerous "leaders" in each of its many subfields. It reflects the fairness and impartiality of our review process that we publish large numbers of articles by young scholars, some of whom are still "nonleaders" but many of whom already can be considered or are rapidly becoming leaders. Furthermore, it would be difficult to pick up at random an issue of the APSR that does not have at least one or two articles by people who

¹ This letter and other related materials are published in the December 2000 issue of PS. PS has invited further comments for future issues.

² At its April 2000 meeting, the APSA Council voted to require council approval of future editorial boards. In view of the suggestion in the protest letter that there is too much concentration of APSA leadership, it is ironic that this change in the bylaws was presented as a reform.

would be recognized as leaders by scholars in more than one subfield. And new leaders are emerging every day. Those who do not read the *Review* may not know who they are.

I have made the point many times over the past five years that APSR articles reflect very closely the types of manuscripts submitted (see, e.g., "APSR Editor's Report for 1999-2000," PS 33 [December 2000]: tables 4, 5, and 7). Unless we adopt a quota system for types of manuscripts, the primary way to achieve greater diversity in what is published is to increase the diversity of submissions. Perhaps the most appropriate question then is why some people, whether leaders or nonleaders, do not submit. The ostensible reason is some variant of "you wouldn't accept it anyway." The letter signed by 222 scholars, for example, proposes that "the current condition of the APSR" could be improved by "revising the APSR reviewing process to seek both to insure that some methodologies are not automatically vetoed and that most articles are of interest to a broad scholarly audience."

As for the second of these standards, since scholars tend to read and write in their own areas of expertise, this may not be a realistic goal. After reviewing about 2,500 papers submitted over the past five years, my own judgment is that very few would appeal across a "broad scholarly audience." In all scholarly disciplines, as knowledge accumulates research tends to narrow rather than broaden, and the focus moves toward analysis of "smaller" issues using finer and more appropriate tools. When the discipline ranges from classical political philosophy to mathematical models of war and peace, the article that appeals to a broad scholarly audience of political scientists may be a chimera. That one could expect to find twenty-five or thirty of them each year to meet the criterion that "most articles [be] of interest to a broad scholarly audience" seems unrealistic in the extreme, and the assumption that such work is more important than detailed scholarship on narrower aspects of the great issues is debatable.

I would suggest that the goal cannot be for each article, or even most articles, to appeal broadly. Rather, the total collection of articles published over a year or two not only should appeal broadly but also should represent the very best work being done in the discipline. Given the APSR citation rate, a widely accepted measure of quality, it seems that we achieve these goals. A recent study of comparative citation rates among ten political science and related journals by the Institute for Scientific Information (publisher of the citation indexes for the sciences and social sciences) found that the APSR ranked first by far on all three criteria of influence used: "impact," the frequency with which the average article in the journal is cited in a given year; "immediacy," how quickly the average article is cited after publication; and "total impact," the number of citations to articles in the journal by all other journals in the ISI database.3 What journal would the critics propose that over time has published a more important collection of articles about government, political processes, and political science than the APSR, and what is the evidence for the claim?

The allegation that some methods are "automatically vetoed" as part of the APSR review process is false. I have described the review process in great detail in my annual reports (published in the December issue of PS each year) and in editor's notes in the Review itself (e.g., the June 1996 issue). In brief, using key words drawn from the submitted manuscript, the APSR interns (advanced graduate students or new Ph.D.s) do in-house research using a database that contains the names and key word fields of approximately 15,000 scholars, online bibliographic databases that permit us to search through publications in thousands of journals, and a variety of search tools on the Internet. We then consult with at least three and sometimes six or seven members of the editorial board about appropriate reviewers for each and every paper. They may choose from among our list of potentials or make additional suggestions from their own knowledge of scholarship in the field of the manuscript and using similar methods. Only when all this input is gathered and organized do I choose reviewers.

The APSR editor is faced with a never ending flow of manuscripts, reviewer choices, and publication decisions for papers that mostly are in fields outside his or her areas of expertise. Why would the editor try to impose an "automatic veto" on papers that would be of interest to readers, especially if their publication would appease the coalition of minorities unhappy with the APSR? There is no such motive; this simply does not happen.

If the review process is fair, if reviewers are chosen from scholars who do similar work with similar methods, why is there a perceived imbalance in our published articles? The letter provides its own answer: Some people in some fields have decided not to submit to the APSR. For example, since August 1995, the Review has received only 33 new manuscripts from 29 individuals among the 222 signatories of the protest letter. 4 Six were accepted, an acceptance rate of 18.2%, almost twice the 9.7% acceptance rate for all other authors, and another six are still pending (including manuscripts still under review and "revise and resubmits" not returned).5 Where is the case for unfairly negative treatment? If the protestors are complaining on behalf of an even larger group of others, how many manuscripts did that group submit, and what was their acceptance rate?

It is difficult to say how many manuscripts the APSR

³ Reported in Table 1 of Lee Epstein, Gregory Caldeira, Micheal Giles, and Gary King, "Report of the Committee on the Puture of

the American Journal of Political Science," April 14, 2000. http://www.indiana.edu/~mpsa/resources/JournalReport.pdf. Accessed November 4, 2000 The ISI ranking presented includes the ten journals with the highest "impact" rates.

⁴ Thirty-two unsolicited manuscripts were received from 28 individuals; an additional manuscript, invited by the editor as a response to a critique, was submitted by another individual.

⁵ The overall acceptance rate was recently reported as 10.1% ("American Political Science Review Editor's Report for 1999-2000," PS 33 [December 2000]: Table 7). Taking into account the new manuscripts received since that report was prepared, our current overall acceptance rate is 9.9%.

should expect from a random group of 222 scholars, but 33 submissions over five years from a group of this size containing as many distinguished academics as the letter signers seems like a rather small number.6 Many scholars who are oriented toward publication in the Review submit quite frequently. The four scholars who have submitted most often in the last five years have submitted a total of 40 new manuscripts, 7 more than all 222 signatories to the letter. The 16 who submitted most frequently submitted a total of 126 new manuscripts and 11 of them had at least one manuscript accepted. Two hundred and twenty-one scholars submitted three or more manuscripts over the last five years and 566 submitted at least two. Given an overall acceptance rate of only 10%, for any author to conclude after one or two rejections that the process is not fair or that it would be hopeless to submit again is unwarranted. In any case, to the extent that the letter signers choose other journals over the APSR, the data refute their claim that unfair treatment is the reason. Any lack of representation of particular subfields or methods results overwhelmingly, if not entirely, from self-selection by authors of where to submit their articles.

Far from wanting to veto any paper, the editor needs to accept a paper every single week of the year to fill the journal, which goes to press on a fixed schedule. It is in the editor's interest to accept as many articles as possible to shake the haunting specter of a blank issue. The editor wants to accept papers. This may be a strange notion to anyone who has not been an editor, especially of a high-quality journal whose reviewers apply very rigorous standards. Even if an editor wanted to impose an unfair review process, the demands of going to press each quarter would not allow it. For every paper on every subject from every author, whether graduate student or endowed chair, the goal is always to structure the fairest possible review process, calling on reviewers who do similar research, so that each paper will have as good a chance to succeed on its merits as any other.

Although the protest group's relatively small number of submissions and higher than normal acceptance rate seem sufficient to refute the charge of bias, one also should consider the extent to which the signatories have been willing to devote their time to evaluating manuscripts of the type they would prefer to see published. The APSR enjoys enormous cooperation from a very large number of reviewers. The large majority of reviewers never turn down requests, even several times per year. Not counting members of the editorial board, over the last five years requests to review have been sent to 2,284 individuals, of which 1,577 (69%) have provided between 1 and 29 reviews without ever declining a request. These are our most dependable reviewers, and they have an important effect on the publication decision of every single manuscript they review. During the last five years, we sent 171 review requests to 57 of the signers of the protest

letter.⁷ Only 52.6% could be similarly categorized as "dependable." Most interesting, the younger scholars in the group (assistant and associate professors) were much more cooperative; 70% of them were dependable, the same as our general reviewer base. Among the "leaders" (full professors and holders of endowed chairs), however, only 41.6% were "dependable" reviewers; the rest declined to review once or more (some repeatedly) or did not respond at all.⁸

If the protesters submit relatively few manuscripts to the *Review* (even though there is no evidence that their acceptance rate is lower than for other authors), and the senior faculty and other leaders among them are likelier than others to decline to provide reviews for papers in their specialties, thus delaying the process and potentially exposing the submissions to less sympathetic reviewers, on what basis can it be expected that there should be more of their preferred kind of research in the *Review*?

The APSR has a fair and transparent editorial process that is at least equal to the best of any other scholarly journal. There are four rules that guide Review procedures.

- A necessary condition for publication of an article is that it be submitted. An automatic veto is imposed on all nonsubmitted manuscripts.
- 2. The APSR editor tries hard to communicate in the most efficient way about the journal's policies and strong interest in publishing a wide variety of research, in editor's notes, annual reports, panels and networking at conferences, and invited talks at universities. The APSR does not, however, commission articles, and no potential author should expect a handwritten personal invitation to submit.
- Each manuscript is put through the same rigorous review process as all others and is evaluated by a panel of reviewers who do similar research, i.e., the author's scholarly peers.
- 4. All authors are treated with scrupulous fairness by the editor, and decisions are guided by the reviews. Unfortunately, the odds of rejection are 9 out of 10. Indeed, unless the number of APSR pages is increased, the odds will become even less favorable as the editor succeeds in attracting more papers from a greater variety of scholars. Nevertheless, regardless of the field of research, there is every reason to assume that a persistent author who writes well, can persuade readers of the importance of the problem studied, and does high-quality work using methods appropriate to the research problem, has as good a chance as anyone else of being successful over the long term.

Given that the *Review* is received by all members of the association, any editor will want it not only to publish outstanding research but also to appeal and be useful to as many scholars as possible. Ultimately, however, the editor can only work with manuscripts

Of the 222 individuals who signed the letter, 66 are full professors, 24 are holders of endowed chairs, and 8 hold high administrative appointments in research institutes or other academic organizations.

⁷ The figures exclude manuscripts sent to the one signatory to the letter who is also a member of the APSR Editorial Board

Nevertheless, several signatories to the letter have been extremely cooperative reviewers, reviewing multiple manuscripts without ever declining.

that are submitted. Our goal is to publish the most outstanding of the manuscripts received. The journal's worldwide reputation, the respect it is accorded throughout political science and allied disciplines, and our citation rates all suggest that we succeed to a very great extent. The diversity of the discipline and the specialized reading habits of scholars mean, however, that most will find relatively few articles of special interest in any given issue. This is not a failing of the Review but is in the essential nature of any disciplinewide journal. Political science has room for many different types of research. It does none of us any good to demean the work of others, and least of all should such disparagement come from those who tell us publicly that they do "not even read, much less submit, to the APSR."

The APSR now receives approximately 450 new papers every year and can publish fewer than 50. The editor has no control over the size of the journal. After communication with hundreds of authors over the past five years, and especially with those close to their tenure deadline who want to speed up the review process, no one understands better than I the frustration of many scholars with our very low acceptance rate and the limited range of APSA publications. I have urged an increase in the size of the APSA's publication program in several venues, including meetings of the Publications Committee and the APSA Council (see my "Annual Report," PS 32 [December 1999]: 809-10). Based on the very strong recommendation of the Publications Committee, the APSA Council at its August 2000 meeting passed a resolution to explore ways to increase publication space, and a committee is now working on that issue. Options will include adding pages to the APSR or to PS, or introducing a new journal.9 Until something like that happens, some of the problems addressed by the protestors cannot be fully resolved.

In the long run, the discipline may fractionate by subfield and/or method, and the problems of difficult communication and lack of mutual appreciation for one another's research may be resolved by an agreement to disagree and the formation of additional associations, which has already happened in several subfields. In the short run the solution is not to attack and attempt to delegitimize the one journal common to all APSA members but to increase the journal space available for our growing number of active scholars and to exhort all interested members of the discipline in all subfields to submit early and often.

As always, I will be happy to clarify anything about our submission and review process that remains unclear. Please contact me at apsr@ssc.msu.edu.

It is time once again to thank all the scholars who reviewed for the APSR this past year. The quality of our articles is improved very significantly by their efforts. In "revise and resubmit" decision letters, authors are told to which reviewers I will return and are

invited to write memos that specify changes made in response to these reviewers' comments or that explain their disagreements. Virtually all authors submitting revisions do this. The responses to the revision memos come, of course, in the subsequent reviews. This process frequently encourages a very fruitful dialogue between author and reviewer. Although there is not always a meeting of the minds, this system encourages very serious consideration of suggestions for revision. The depth of the reviews, many of which are multiple pages, and the willingness of reviewers to consider revisions (sometimes more than once) are measures of their commitment to the process.

Approximately 2,000 scholars have reviewed for the APSR over the past five years, making ours one of the largest and broadest reviewer pools of any scholarly journal. The editorial board joins me in expressing gratitude to all of them. The approximately 700 scholars who participated this year are listed at the back of this issue. If anyone we have not yet called upon has an interest in reviewing manuscripts, or knowledge of other scholars who can discern good work in their field of expertise, please let me know. As large as our reviewer pool is, increasing its size and diversity is a constant goal.

Readers will also find the annual index for 2000 at the back of this issue. A comprehensive index of all articles and book reviews published since 1996 can be found at our web site: http://www.ssc.msu.edu/~apsr/cumindex.htm

Authors are reminded that no new submissions will be accepted by this office during August 2001. Invited revisions and reviews may be sent in at any time. Please refer to the June 2001 issue for information about the address change for the editorial office, as well as the dates for sending manuscripts and reviews to the new editorial office.

Ada W. Finifter Editor

INSTRUCTIONS TO CONTRIBUTORS

General Considerations

The American Political Science Review strives to publish scholarly research of exceptional merit, demonstrating the highest standards of excellence in conceptualization, exposition, methodology, and craftsmanship. Because the Review reaches a diverse audience of scholars and practitioners, contributors should demonstrate how their analysis illuminates a significant research problem, or answers an important research question, of general interest in political science. For the same reason, authors should strive for a presentation that will be understandable to as many scholars as possible, consistent with the nature of their material.

Since the *Review* publishes original research, authors should not submit articles containing tables, figures, or substantial amounts of text already published or forthcoming in other places, or that are included in other manuscripts submitted for review, whether to book publishers or periodicals (including online journals), or are otherwise committed. In many such cases, subse-

⁹ These initiatives have been in play for well over a year, and various reports and council actions were posted at the APSA web site long before the 124 sent their letter of protest.

quent publication of this material would violate the copyright of the other publisher. The APSR also does not consider papers currently under review at other journals or that duplicate or overlap with parts of larger manuscripts that have been submitted to other publishers (including publishers of both books and periodicals). Since space is at a premium, submission of articles substantially similar to manuscripts submitted or published elsewhere, or to part of a book or other larger work, is also strongly discouraged. If there is any doubt about whether these policies would apply to a particular case, the author should discuss any such publications related to a submission in a cover letter to the Editor. The Editor should also be notified of any related submissions to other publishers, whether for book or periodical publication, that occur while an article is under review at the APSR and that would fall within the scope of this policy. Copies of related publications may be requested.

For manuscripts containing quantitative evidence and analysis, authors should describe their empirical procedures in sufficient detail to permit reviewers to understand and evaluate what has been done and, in the event the article is accepted for publication, to permit other scholars to carry out similar analyses on other data sets. For example, for surveys, at the least, sampling procedures, response rates, and question wordings should be given; authors are encouraged to calculate response rates according to one of the standard formulas given in The American Association for Public Opinion Research, Standard Definitions: Final Dispositions of Case Codes and Outcome Rates for RDD Telephone Surveys and In-Person Househould Surveys (Ann Arbor, MI: AAPOR, 1998). This document is available on the Internet at http://www.aapor.org/ ethics/stddef.html>. For experiments, provide full descriptions of experimental protocols, methods of subject recruitment and selection, information about any payments and debriefing procedures, and other relevant details. It is desirable for articles to be selfcontained, and authors should not refer readers to other publications for descriptions of these basic research procedures.

Please indicate variables included in empirical analyses by capitalizing the first word in the variable name and italicizing the entire variable name the *first* time each is mentioned in the text. Authors should also be consistent in using the same names for variables in text and tables, and avoid overuse of acronyms and computer abbreviations when discussing variables in the text. All variables appearing in tables should have been mentioned in the text and the reason for their inclusion in the analysis discussed.

As part of the review process, authors may be asked to submit additional documentation if procedures are not sufficiently clear; the review process works most efficiently if such information is given in the initial submission. If readers are advised that additional information is available from the author, printed copies of that information should be submitted with the manuscript. If the amount of this supplementary information is extensive, please inquire about alternate procedures.

The APSR uses a double-blind review process. Authors should follow the guidelines for preparing anonymous copies in the "Specific Procedures" section below.

Manuscripts that are largely or entirely critiques or commentaries on previously published articles will be reviewed using the same general procedures as for regular articles with one exception. In addition to the usual number of reviewers, such manuscripts will also be sent to the scholar(s) whose work is being critiqued, in the same anonymous form as they are sent to other reviewers. Comments from the original author(s) to the editor will be invited as a supplement to the advice of regular reviewers. There are several purposes for this notice to the original author(s): (1) to encourage review of the details of analyses or research procedures that might occasionally escape the notice of disinterested reviewers; (2) to enable prompt publication of critiques by supplying critiqued authors with early notice of their existence and, therefore, more adequate time to reply; (3) as a courtesy to critiqued authors. Authors of critiques should therefore send as many additional copies of their manuscripts as will be required for this purpose.

Manuscripts being submitted for publication should be sent to Professor Ada W. Finifter, Editor, American Political Science Review, Department of Political Science, Michigan State University, 303 South Kedzie Hall, East Lansing, MI 48824-1032. Correspondence concerning manuscripts under review may be sent to the same address or, preferably, by e-mail to apsr@ssc.msu.edu. Please note that the APSR does not process new manuscripts during August; new manuscripts should be mailed to arrive either before or after. Invited revisions and reviews may be submitted at any time and will be processed upon arrival.

Manuscript Formatting

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International Peacebuilding: A Theoretical and Quantitative Analysis

MICHAEL W. DOYLE Princeton University NICHOLAS SAMBANIS The World Bank

International peacebuilding can improve the prospects that a civil war will be resolved. Although peacebuilding strategies must be designed to address particular conflicts, broad parameters that fit most conflicts can be identified. Strategies should address the local roots of hostility, the local capacities for change, and the (net) specific degree of international commitment available to assist sustainable peace. One can conceive of these as the three dimensions of a triangle whose area is the "political space"—or effective capacity—for building peace. We test these propositions with an extensive data set of 124 post—World War II civil wars and find that multilateral, United Nations peace operations make a positive difference. UN peacekeeping is positively correlated with democratization processes after civil war, and multilateral enforcement operations are usually successful in ending the violence. Our study provides broad guidelines for designing the appropriate peacebuilding strategy, given the mix of hostility, local capacities, and international capacities.

The collapse of state institutions in Somalia, a coup in Haiti, and civil wars in Bosnia, Cambodia, El Salvador, Guatemala, and other countries have marked the distinctive contours of civil strife in the past decade. The international community's responses to these emergencies have, despite sometimes major efforts, been mixed at best: Occasional successes in restoring a legitimate and effective government are matched by striking failures to do so. The United States and the United Nations intervened in Somalia, but their effort appeared to lack direction. In Cambodia the UN undertook a multidimensional operation that left behind in 1993 a partial peace; there was a coup in 1997, and the country now struggles ahead with a renewed coalition government. In El Salvador, Guatemala, Namibia, Eastern Slavonia (Croatia), and Mozambique peace is firmer, but the long-run prospects of social integration remain problematic. In Bosnia, de facto partition still holds sway in most of the country, and current stability is a direct function of NATO peacekeeping. The international community has assumed temporary sovereignty in Kosovo and East Timor, and the task of developing viable polities there has barely begun.

One of the most important challenges for the international community is how to rebuild stable polities in the aftermath of civil war. How can it help prevent renewed hostility? What role should it play to ensure that failed states do not relapse into chaos as soon as the peacekeepers leave? The UN and various regional

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organizations, including NATO, have accepted the responsibility for postconflict peacebuilding and have authorized their member states to undertake extensive intrusions into the domestic affairs of legally sovereign states. What guidelines should be developed to help steer these ambitious mandates?

In current usage in the UN and among private voluntary organizations, peacebuilding is an attempt, after a peace has been negotiated or imposed, to address the sources of current hostility and build local capacities for conflict resolution. Stronger state institutions, broader political participation, land reform, a deepening of civil society, and respect for ethnic identities are all seen as ways to improve the prospects for peaceful governance.² In plural societies, conflicts are inevitable. The aim of peacebuilding is to foster the social, economic, and political institutions and attitudes that will prevent these conflicts from turning violent.³ In effect, peacebuilding is the front line of preventive action.

PEACEBUILDING STRATEGIES

The political strategy of a peacebuilding mandate is the concept of operations embodied in its design. Just as civil wars are usually about failures of legitimate state authority, sustainable civil peace relies on its successful reconstruction. Peacebuilding is about what needs to happen in between. Civil wars arise when individuals, groups, and factions discover that a policeman, judge, soldier, or politician no longer speaks and acts for them. The local cop becomes the Croatian, Serb, or

¹ For a discussion of the concept, see Cousens, Kumar, and Wermester 2000; Franck 1998; Goulding 1993. The UN's own views can be found in Annan 1998 and Boutros-Ghali 1992.

² For a discussion of comprehensive peace, see Boulding 1964 and Kacowicz 1994, chap. 1. For a valuable collection of articles on peacebuilding, see Cousens, Kumar, and Wermester 2000; Evans 1993; UN Department for Development Support and Management Services and UN Industrial Development Organization 1995.

³ No peace is perfect. Isaah (11:6) prophesied that we shall know peace when we see the lamb lie down with the lion. Comedian Woody Allen (1976, 28) adds a valuable warning for our world: One of the two might not get much sleep.

Muslim cop. When the disaffected mobilize, acquire the resources needed to risk an armed contest, meet resistance, and judge that they can win, civil war follows.⁴

Although we can imagine purely cooperative solutions to domestic peace,5 the confusion, "noise," violence, and changing identification that characterize the onslaught and conduct of civil war do not seem to be promising circumstances for rational cooperation among factions. Instead, the establishment of civil peace seems to require addressing directly both the defensive and aggressive incentives that motivate faction leaders (and sometimes their followers). Defensive incentives arise in the domestic security dilemma. As anarchy emerges (due to the collapse of central authority), each group/faction seeks to arm itself for protection; as in interstate anarchy, however, each defensive armament constitutes a threat to other factions (Posen 1993). Offensive incentives arise because factions and their leaders want to impose their ideology or culture, reap the spoils of state power, seize the property of rivals, exploit public resources for private gain, or all of the above. Peace thus requires the elimination, management, or control of "spoilers" (Stedman 1997) or war entrepreneurs (DeFigueiredo and Weingast 1999).

Conquest by one faction can solve the problem (but even in this case political and social reconstruction can be vital for longer term legitimacy and stability). Peace through agreement can employ the separation of populations and territorial partition to address war-prone incentives (Kaufmann 1996). Civil wars can be turned into international conflicts, as between Eritrea and Ethiopia, or stable and relatively secure international or intercommunal balances of power, as in Cyprus or Somalia (Herbst 1996/97). To each spoiler, his or her separate pile of spoils. But in many civil wars the contest is over who or what ideology controls a single polity. Moreover, in some ethnic wars the costs of "cleansing" will seem too high, or a common basis for overarching civic citizenship exists or can be created. Combatants in these circumstances still have continuing disputes over material interests, who or what rules, and safety. Each has experienced devastating destruction (although in varying degrees), and both leaders and followers are likely to harbor deep resentment for losses sustained, particularly to family and friends. They also experience the costs of war and may come to a "hurting stalemate," in which no faction sees that it can win and each perceives the high costs of continuing strife (Zartman 1985). In these latter circumstances, sustainable peace needs state authority as a starting point to overcome security concerns. Hobbes's Leviathan—state sovereignty, or authority—fills that role, restoring legitimate power.6

The rational choice theory of civil war is relevant to the calculations parties make to support or reject peace after the fighting ends. Simply put, war will recur if the expected utility of war is greater than the expected utility of peace. Such a model underpins recent work on civil war occurrence and termination (Azam 1995; Collier and Hoeffler 1998, 2000; Hirschleifer 1987; Mason and Fett 1996). These studies assume that the warring parties are rational but not infallible; war generates private and public gains and losses that are unevenly distributed; private gains explain why war may be rational for some groups; and because war is collectively suboptimal, it is also collectively irrational. These assumptions allow analysts to make a series of hypotheses regarding the likelihood of war, but the results of such a decision-making model clearly depend heavily on a further set of detailed assumptions.

The specific motivations that shape the behavior of combatants are thus complex and varied. The Thucydidean and Hobbesian trinity of motives (fear, honor, interest) are present in modern variations—security dilemmas, ethnic identity and/or ideological fervor, and loot seeking—and each of them is complicated by potential differences between leaders and followers and factions and patrons. The decision to organize or participate in a rebellion and then attempt to achieve a viable peace is not a straightforward matter and may differ greatly across actors. What each shares, however, is a political environment in which success in achieving peace depends on the degree of harm sustained, the resources available for development, and the international assistance to overcome gaps. Low levels of economic development and other deficiencies in local capacities may motivate actors to violence, due to the low opportunity cost of war and the opportunities for private gains from violence (Collier and Hoeffler 2000). Increased hostility due to the experience of war makes reconciliation more difficult. To achieve peace and reconciliation under these circumstances, Zartman (1995) argues that we need to (1) reconcentrate central power (the powerful must be recognized as legitimate; or the legitimate made powerful), (2) increase state legitimacy through participation (elections, power sharing), and (3) raise and allocate economic resources in support of peace. To this he adds that, given the devastation of civil war, all three generally require (4)

⁴ The literature is extensive. We have especially benefited from David 1997; DeNardo 1985, Eckstein 1964, Gurr 1993, Lake and Rothchild 1996.

⁵ See, for example, Fearon and Laitin 1996, but note that the authors do not claim to explain the empirical record of domestic peace. They acknowledge (p. 731) that state power and domestic authority are alternative explanations.

⁶ The Oxford English Dictionary defines authority: "right to command," "power to influence action," "power over the opmions of others." An enlightening essay is "What Is Authority" (Arendt 1961), and an mughtful treatment of the Hobbeauan problem applied to economic development is the concept of the "stationary bandit" (Olson 1993).

⁷ A decision-making model of peacebuilding failure would require limiting assumptions about the interaction among rebel groups and between them and the government as well as about the rebels' motivation, their relative size, and their strength, by estimating either a Cournot-Nash equilibrium (Collier and Hoeffler 2000) or Stackelberg (leadership) equilibrium (Azam 1995). The parties' decision to support peace or return to war could then be derived by maximizing their utility functions with respect to each other's expected reaction functions.

external, international assistance or authority in a transitional period.8

This last dimension is our particular focus here. We do not intend to model a specific decision-making framework nor to predict where the UN will choose to become involved; rather we will explore the correlates of successful and unsuccessful peacebuilding after civil war. What role does international assistance play in the peace process? How much and of what kind is required? We will argue that the levels of war-related hostility and the pre- and postwar levels of local capacities interact with present international capacities to deliver specific postconflict outcomes. For given levels of local capacity and hostility, we will identify the right form of international assistance to maximize the available space for peace.

A PEACEBUILDING TRIANGLE

International peacebuilding strategies—concepts of operations—should be "strategic" in the ordinary sense of that term, matching means to ends. Although the strategy must be designed to address a particular situation, broad parameters that fit most conflicts can be identified. All strategies should address the local sources of hostility, the local capacities for change, and the (net) specific degree of international commitment available to assist change. One can conceive of these as the three dimensions of a triangle whose area is the "political space," or effective capacity, for building peace. This suggests that the dimensions substitute for one another, that is, more of one substitutes for less of another; less extreme hostilities substitute for weak local capacity or minor international commitment.

International commitment (or lack thereof) interacts with local capacities and factional hostility to shape the triangular space; few peacebuilding plans work unless regional neighbors and other significant international actors desist from supporting war and begin supporting peace. The end of Cold War competition was an important precondition for the bloom of peacebuilding operations in the early 1990s. Beyond that minimum, international peacebuilding, from monitoring to enforcement, also makes a difference. We divide international peacebuilding into four types of mandated operations.

First, a monitoring or observer mission is an interim arrangement used in violent conflicts with the consent of the host government. In these conflicts, there is no formal determination of aggression. The purpose is to monitor a truce and help negotiate a peace through the presence of military and civilian observers.

⁸ Not every country would benefit from external mediation or mtervention in its civil war. Some wars, we could argue with hindsight, are more likely to promote stable and just government if they are fought to a conclusion and the just side wins. Such an argument might be made for the U.S. Crvil War.

Second, traditional peacekeeping involves the deployment of military units and civilian officials in order to facilitate the negotiated settlement of a conflict. It is based on the consent of the parties (normally authorized under Chapter VI of the UN Charter). Traditional peacekeeping operations (PKOs) typically establish and police a buffer zone and assist the demobilization and disarmament of military forces.

Third, multidimensional peacekeeping is also consent based and is designed to implement a comprehensive negotiated peace agreement. It includes a mix of strategies to build a self-sustaining peace, ranging from those of traditional PKOs to more multidimensional strategies for capacity expansion (e.g., economic reconstruction) and institutional transformation (e.g., reform of the police, army, and judicial system; elections; civil society rebuilding).

Fourth, peace enforcement is a (usually multilateral) military intervention, authorized under Chapter VII of the UN Charter. It is designed to impose public order by force, if needed, with or without host government consent.

International peacebuilding mandates must take into account the characteristics of the factions. Peacebuilding operates not upon stable states but upon unstable factions. These factions (to simplify) come in varying numbers and in various dimensions of hostility based upon how much damage each has done to the other. The more hostile and numerous the factions, the more difficult is the peace process, and the more international assistance/authority is needed to establish peace.

In less hostile circumstances (few factions, a hurting stalemate, or less harm done) international monitoring may be sufficient to establish transparent trust and self-enforcing peace. Monitoring helps create transparency among partners who lack trust but have compatible incentives favoring peace. Traditional peacekeeping assistance also can reduce tradeoffs (e.g., help fund and certify the cantonment, demobilization, and reintegration of former combatants). In these circumstances—few players, substantially compatible incentives, some reconciliation, less damage—international coordination and assistance may be sufficient to overcome hostility and solve implementation problems. An international presence itself can deter defections from the peace treaty, due to the possible costs of violating international agreements and triggering further external involvement in domestic conflict.

In more hostile circumstances, international enforcement can help solve commitment and cooperation problems by imposing order or by directly implementing peace agreements or raising the costs of defection from them. Long-term trusteeship may be required to overcome deep distrust and powerful incentives to defect from the peace. As in prisoner's dilemma and mixed motive games (Axelrod and Keohane 1985; Oye 1985), deep hostility, multiple factions, or lack of coherent leadership may complicate the achievement of self-enforcing cooperation. Conscious direction by an impartial agent to guarantee the functions of effective sovereignty become necessary, and includes such activities as demobilizing the armies of the factions,

⁹ In Cambodia, for example, it was quite important for the prospects of peace that, together, China cease military support for the Khmer Rouge, the Soviet Union and Vietnam for Hun Sen's regime, and the West for the royalist forces. Lacking external support, each faction found negotiations for peace more attractive.

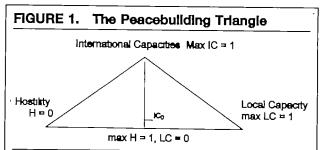
arresting war criminals, and policing and administering a collapsed state.

War-torn countries also vary in economic and social capacity. Some start out with considerable economic development (the former Yugoslavia) and a high level of social capacity in an educated population. Others begin poor and are further impoverished by war (Angola, Sudan, Cambodia). In both cases reconstruction is vital; the greater the social and economic devastation, the larger the multidimensional international role must be, whether consent-based multidimensional peacekeeping or nonconsent enforcement followed by and including multidimensional peacekeeping. International economic relief and productive jobs are the first signs of peace that can persuade rival factions to disarm and take a chance on peaceful politics. Institutions need to be rebuilt, including a unified army and police force and the even more challenging development of a school system that can assist the reconciliation of future generations.¹⁰

In sum, there should be a relation between the depth of hostility, the number and character of the factions, and the level of economic development, on the one hand, and the extent of international assistance and effective authority, whether monitoring or enforcement, needed to build peace, on the other. In a world in which each dimension is finite, we can expect, first, that compromises will be necessary to achieve peace-building success and, second, that the international role will be significant in general and successful when it is designed to fit the case.

Research Design

We will argue that the probability of successful peacebuilding is a function of a country's capacities, the available international assistance, and the depth of war-related hostility. The relations among these are complicated. The availability and prospect of international assistance and extensive local capacities, for example, can both raise the gains from victory (spoils of war and rebuilding assistance) and reduce the costs of fighting (as the assistance serves to sustain the fighting). So, too, deep war-related hostilities can have dual effects. We will argue more specifically that (1) the decision to support peacebuilding (PB) is enhanced by both local and international capacities for peace; (2) net local capacities (NLC) are given by the difference between local capacities (LC) or developmental potential minus war-generated hostility (H); and (3) international capacities (IC) can substitute for deficiencies in local capacities to compensate for the depth of hostility. Thus, we theorize that the PB process is captured by: PB = IC × NLC. That relationship is



Note. The triangle is a metaphor for the peacebuilding space after civil war. Available space is determined by the interaction of the triangle's three eldes: Local Capacities (LC), International Capacities (IC), and Hostility (H) level. The greater local and international capacities and the amailler the hostility level, the greater the space for peace. We assume a strictly positive level of IC, given the support and legitimacy offered sovereign states by international law and norms. This positive level of international support is denoted by the constant lob, which ensures that IC cannot be zero. All three variables, LC, IC, and H, can be measured as indices, ranging from 0 to 1 (maximum). We estimate a model of peacebuilding using these indices as regressors later in the paper.

loosely reflected in the shape of a peacebuilding triangle, as shown in Figure 1.11

This convenient shape makes it possible to visualize our key hypotheses, since the three sets of variables interact competitively (H vs. IC and LC) and cooperatively (LC and IC) to produce a space for peace. ¹² Specifically, this interactive model posits: (1) The larger is IC, the higher is the probability of PB success, given H and LC; (2) the greater (deeper) the hostility, the lower the probability of PB success, given LC and IC; and (3) the larger is LC, the higher is the probability of PB success, given H and IC.

We will test our interactive model by identifying and measuring proxy variables for *Hostility, Local Capacities*, and *International Capacities* and by computing the relative significance of each of these determinants for peacebuilding success. This is the first quantitative analysis of the correlates of successful peacebuilding and of the contribution of UN operations to peacebuilding outcomes.¹³ We map the strategic environment within which actors make their decisions to

¹⁰ We have observed peace operations in El Salvador, Cambodia, Eastern Slavonia (Croatia), Breko (Bosnia), and Cyprus, and it is our opinion that establishing a unified army or multiethnic police force, although difficult, is easy compared to agreeing on an elementary school curriculum.

 $^{^{11}}$ H, LC, and IC are indices ranging from 0 to 1 (maximum). IC = $ic_0 + IC_1$, where IC_1 is the amount of international assistance after the war and ic_0 is a positive constant $(0 < ic_0 < 1)$ that represents the lowest level of international and available ex ante to sovereign states, as provided by international laws ensuring that if LC – H does not equal zero, the probability of peacebuilding success can be defined as the area of the triangle.

¹² Note that we use the triangle to visualize the interaction of the three core variables, and we will be testing that interaction without assuming a functional form for it, as we would, for example, by assuming that the peacebuilding space is only given by the area of the triangle.

¹³ There are many informative and comparative case studies of peacebuilding success and failure. For a valuable critical assessment and bibliography see Collins and Weiss 1997. Among the many works we have found especially helpful are Brown 1996; Chopra 1999; Doyle, Johnstone, and Orr 1997; Durch 1993, 1997; Fetherston 1994; Hampson 1996; Lickhider 1993; Paris 1997; Ratner 1995; and Walter and Snyder 1999 Diehl, Reifschneider, and Hensel 1996 and Haas 1986 analyze the effect of UN missions on conflict recurrence, with a focus on interstate conflicts of varying intensity. A classic piece on interstate conflict is by Haas, Butterworth, and Nye (1972), who argue that the UN works best when elaborate (e.g., multidimensional) peace operations are used.

support peace or war and we explain how best to use UN peace operations to prevent civil wars from recurring.

The Data

To test our hypotheses, we constructed a new crosssectional data set of all civil wars since 1944. We focus on those that ended by 1997, so that we can measure peacebuilding outcomes at least two years after the end of the conflict. A few cases of ongoing wars are included if a peace operation has been initiated, since our interest is in the effectiveness of such operations in ending war and restoring peace. Appendix A provides further discussion of case selection.

We define a civil war as an armed conflict that meets all the following conditions: causes more than 1,000 deaths overall and in at least a single year; challenges the sovereignty of an internationally recognized state; occurs within the recognized boundary of that state; involves the state as a principal combatant; includes rebels with the ability to mount organized armed opposition to the state; and has parties concerned with the prospect of living together in the same political unit after the end of the war. This definition allows us to combine wars from several data sets.

The Explanatory Variables

We selected proxy variables that are theoretically consistent with the interactive model presented above and that forge links with the civil war literature. We proxy the level of hostility by the log of deaths and displacements (LOGDEAD), the type of conflict (WARTYPE), the number of hostile factions (FACTNUM), the level of ethnic division (EH, ELF), and the outcomes of the war (MILOUT, TREATY) (see Appendix B for summary statistics and a list of sources for key variables). We proxy local capacities by a set of socioeconomic measures of development, such as real per capita GDP, energy consumption (DEVELOP), and natural resource dependence (EXP). International capacities are provided by the strength and mandate of peace operations (UNMANDATE, UNOP), if any were used, and by the amount of economic assistance available to the country at war (TRANSFCAP). We control for systemic constraints, such as the Cold War (COLDWAR) or the decade during which the war started (DECADE).

Our main concern is with how international capacities, UN peace operations in particular, influence the probability of peacebuilding success. We collected data on all UN operations and classified them into the four types described above: 15 monitoring or observer mis-

sion (UNOP2), traditional peacekeeping (UNOP3), multidimensional peacekeeping (UNOP4), and peace enforcement (UNOP5).

The Dependent Variable

The main version of our PB dependent variable is PBS2—peacebuilding success or failure two years after the end of the war. We also measure PB outcomes five and ten years after the war (Appendix C explains our coding method). PBS2 is a binary variable, coded 1 for PB success, 0 otherwise. We used a lenient (PBS2L) and a strict (PBS2S3) version of the PB variable (as well as different thresholds for the strict version) to test the robustness of our findings. The lenient version implies an end to the war and to residual lower-level violence and uncontested sovereignty. It represents a minimum (or negative) measure of peace, focused on the absence of violence. There are 71 (57.26%) lenient PB failures and 53 successes (42.74%).

The stricter version of PB also requires a minimum standard of democratization. We prefer this version because it reflects a higher order of peace but requires only a minimum standard of political openness. There are 81 (65.32%) strict PB failures and 43 (34.68%) successes. Table 1 lists all civil war events since 1944 and presents two-year PB outcomes for all these events. 17

TEN POLICY-RELEVANT HYPOTHESES

To achieve easily interpretable results with straightforward policy implications, we interpret our interactive model of PB as a set of associations between PB outcomes and our proxies for hostility, local capacities, and international capacities. We use these proxies to make ten policy-relevant hypotheses.

HYPOTHESIS 1. The probability of peacebuilding (PB) success should be lower in identity wars (i.e., ethnic and religious wars).

Hostility is easily channeled across ethnic lines and several other scholars have identified the ease with which ethnic passions can be mobilized into support for ethnic war (DeFiguereido and Weingast 1999; Lake and Rothschild 1998). Furthermore, the ease of ethnic identification makes it harder to reconcile differences among combatants after civil war (Kaufman 1996).

meduding such regional ones as the NATO operation in Bosnia. We also consider other forms of UN involvement (UNINV) and diplomacy (UNOP1) conducted before attempts at peace implementation.

Briefly, we coded a peacebuilding fallure if the country was at war, if it had large-scale political violence short of war, if it had divided sovereignty, or if it did not meet a minimum standard of political openness according to Gurr's democracy scores (Polity98 project). The data set, information on our coding, sources for all variables, and a discussion of all war events can be downloaded at: http://www.worldbank.org/research/conflict/papers/peacebuilding.

17 We dropped from our analysis wars that ended less than two years before the time of writing (see Appendix A for a full explanation).

¹⁴ Dropping those cases did not change the results presented in later sections. These cases are Angola (1992-ongoing), Burundi (1991-ongoing), Colombia (post-1978), Srl Lanka (Tamil insurrection), and Somalia (after 1992).

¹⁵ We are examining UN cases because they are the predominant form of multilateral peace operation in the entire period, but the conclusions are likely to apply to other multilateral initiatives,

Country	Start	End	Lenlent PB	Strict PB	Country	Start	End	Lenlent PB	Strict P
Afghanistan	78	92	Fallure	Fallure	iraq/Kurds	61	75	Fallure	Fallure
Afghanistan	93	_	Fallure	Failure	Iraq/Kurds	88	94	Fallure	Fallure
Algeria	62	63	Success	Fallure	Iraq/Shiltes	91	94	Fallure	Fallure
Algeria	92	97	Fallure	Failure	Israel/Palestine*	47	97	Success	Succes
Angola	75	91	Fallure	Fallure	Jordan	· 71	71	Success	Fallure
Angola	92	-	Fallure	Fallure	Kenya*	91	93	Fallure	Fallure
Argentina	55	55	Success	Success	Koréa	50	53	Success	Fallure
Azerbaijan	88	96	Fallure	Fallure	Laos	60	75	Fallure	Fallure
Bangladesh	73	94	Success	Success	Lebanon	58	58	Success	Succes
Bolivia	52	52	Success	Success	Lebanon	75	78	Fallure	Fallure
3uma	48	51	Fallure	Fallure	Lebanon	82	92	Fallure	Fallure
Burma	68	82	Failure	Fallure	Liberia	89	92	Fallure	Failure
Burma	83	95	Fallure	Fallure	Liberia	93	96	Fallure	Failure
Burundl	65	69	Fallure	Fallure	Malaysia	48	59	Success	Succes
Burundl	72	73	Success	Fallure	Mall	90	95	Success	Succes
Burundl	88	88	Fallure	Fallure	Mexico*	92	94	Success	Succes
Burundl	91	-	Fallure	Fallure	Moldova	92	94	Fallure	Fallure
Cambodla	70	75	Fallure	Fallure	Mor./W. Sahara	75	89	Fallure	Fallure
Cambodla	79 25	91	Success	Success	Mozamblque	79	92	Success	Succes
Central Africa	95 05	97	Success	Success	Namibla*	65	89	Success	Succes
Chad Chad	65 80	79 94	Fallure	Fallure	Nicaragua	78	70	Fallure	Fallure
China/Talwan	80		Success	Success	Nicaraguq	81	89	Success	Succee
	47 50	47	Fallure	Fallure	Nigeria	67	70	Success	Fallure
China/Tibet China	50	51 eo	Fallure	Fallure	Nigeria	80	84	Fallure	Fallure
Colombia	67 48	68 62	Fallure Success	Fallure	Northern Ireland*	68	94	Success	Succes
Colombia	46 78	02 _	Fallure	Success Follows	Pakistan/Bangid.	71 70	71	Success	Succes
Congo-Brazzaville	92	96	Fallure	Fallure Fallure	Pakistan/Blch.	73	77 21	Fallure	Fallure
Congo/Zalre	60	65	Fallure	Fallure	Papua N. Gulnea	88	91	Failure	Fallure
Congo/Kisangani	67	67	Success	Fallure	Paraguay Peru*	47 80	47 96	Success	Succes
Congo/Shabba*	75	79	Fallure	Failure	Philippines	50	52	Fallure	Fallure
Congo/Zalre	96	97	Fallure	Fallure	Philippines	72	92	Success Fallure	Succes
Costa Rica	48	48	Success	Success	Philippines	72 72	96	Failure	Fallure Fallure
Cuba	5 8	59	Fallure	Fallure	Romania	89	89	Success	Succes
Cyprus	63	64	Fallure	Fallure	Russla/Chechnya	94	96	Fallure	Fallure
Cyprus	74	74	Fallure	Fallure	Rwanda	63	64	Fallure	Fallure
Ollbout	91	95	Success	Success	Rwanda	90	94	Success	Succes
Dominican Rep.	65	65	Success	Success	Slerra Leone	91	96	Fallure	Fallure
3 Salvador	79	92	Success	Success	Somalia	88	91	Fallure	Fallure
thiopla/Erttrea	74	91	Success	Success	Somalla	92	_	Fallure	Fallure
thiopla/Ogaden	77	85	Failure	Fallure	South Afrida*	76	94	Success	Succes
thlopla	74	91	Success	Success	Srl Lanka/JVP	71	71	Success	Succes
Georgia/Abkhazia	91	93	Fallure	Fallure	Srl Lanka/tamil	83	_	Fallure	Failure
Georgia/Ossetia	92	94	Fallure	Fallure	Srl Lanka/JVP	87	89	Success	Succes
ireece	44	49	Success	Success	Sudan	63	72	Success	Fallure
iuatemala	54	54	Success	Success	Sudan	83	_	Fallure	Fallure
luatemala	66	72	Fallure	Fallure	Tajlkistan	92	94	Fallure	Fallure
iuatemala	74	94	Success	Success	Thalland*	67	85	Success	Succes
lalti	91	94	Fallure	Fallure	Turkey	84	_	Fallure	Fallure
lalti	95	96	Success	Success	Uganda	66	66	Success	Succes
ndla/Partition	46	48	Success	Success	Uganda	78	79	Fallure	Fallure
ndia/Kashmir	65	65	Fallure	Fallure	Uganda	80	86	Fallure	Failure
ndla/Kashmir	89	94	Fallure	Fallure	Vletnam	60	75	Success	Fallure
ndle/Slkh	84	94	Success	Success	Yemen	48	48	Success	Succes
ndonesla/Mol.	50	50	Fallure	Fallure	Yemen	94	94	Success	Succes
ndonesia/Dar.	53	53	Fallure	Fallure	Yemen, North	62	69	Success	Succes
ndonesia	56	60	Fallure	Fallure	Yemen, South	86	87	Success	Fallure
ndonesla/E. Timor	75 20	82	Fallure	Fallure	Yug./Bosnia	92	95	Fallure	Fallure
ndonesia	86	86	Success	Fallure	Yug./Croatla	91	91	Fallure	Fallure
an/Revolution	78 81	79 82	Fallure Fallure	Fallure Fallure	Yug./Croatla Zimbabwe	95	95	Success	Succes
an						72	80	Fallure	Fallure

Note: Astensks denote cases that may not have caused 1,000 deaths for every year of the war but have produced 1,000 deaths in at least one year during the war. In total, there are 71 failures and 53 successes of lement peacebuilding and 81 failures and 43 successes of strict peacebuilding. The sources for this table are listed in Appendix A

Hypothesis 2. The probability of PB success should be lower, the greater are the human costs of the war (deaths and displacements). 18

Human costs also proxy the level of war-generated hostility. We measure total deaths—including civilian casualties—and displacements (refugees and internally displaced persons) that result from the war. The greater the human costs, the lower is a society's remaining stock of social and human capital, and the lower its ability to rebound after civil war. Furthermore, the greater these costs, the deeper are the social-psychological barriers to building peace.

HYPOTHESIS 3. Partly in contradiction to the above, the probability of PB success is higher the longer the war. 19

This may seem counterintuitive. It can be argued that longer wars should increase hostility because they create more casualties (other things being equal). Yet, the parties grow tired of fighting, and any prewar uncertainty about the probability of military victory or the parties' relative resolve is removed.²⁰ Longer wars offer a chance for the parties to learn by reflecting on the benefits of peace and by controlling war-related hostility.

HYPOTHESIS 4. The probability of PB success should be lower when the number of factions is larger.

More factions imply a larger pool of potentially divergent preferences, which makes it harder to negotiate a cooperative equilibrium (Oye 1985). As discussed previously, many factions tend to increase the level of hostility. We also hypothesize, however, that the relationship between the number of factions and PB outcomes is nonmonotonic. The initial effect of multiple factions is negative, but at very large numbers, as cross-cutting coalitions emerge, the probability of PB success may rise. An intermediate number should make peace bargaining harder because it is easier to forge cross-cutting coalitions among larger groups of factions than among a polarized few.²¹

HYPOTHESIS 5. The probability of PB success decreases with an increase in ethnic heterogeneity.

Although ethnic groups need not be hostile toward one other, we can assume that each will have different

18 Both absolute and per capita measures should be important. One million casualties is 10% of the population in Cambodia, 0.4% in the United States. But with modern communications and threshold effects, the political/psychological shock of one million casualties in the United States is likely to be much more than 1/25th the effect that this would have in Cambodia.

¹⁹ There is a negative partial correlation between deaths and displacements (LOGDEAD) and PB outcomes and a positive partial correlation between PB outcomes and war duration (WARDUR) This means that our different sign hypotheses about deaths and war duration are meaningful.

20 Blainey (1973) and Fearon (1995) make a similar argument: War occurs at least partially as the result of uncertainty about relative capabilities and resolve.

²¹ This hypothesis reflects similar reasoning in the literature on international systems. Three classics are by Deutsch and Singer (1964), for multipolar stability; Waltz (1964), on bipolar stability; and Selten (1973), for nonmonotonic factors.

preferences about the terms of a settlement or other distributive issues. Finding a mutually acceptable peacebuilding equilibrium should be more difficult when ethnic heterogeneity is great. Collier and Hoeffler (2000), Collier, Hoeffler, and Soderbom (1999), and Elbadawi and Sambanis (2000) show that ethnic polarization significantly increases the risk of civil war, but high levels of both ethnic homogeneity and ethnic heterogeneity reduce that risk. Bates (1999) finds a similar relationship with reference to Africa. We can expect a similar effect in peacebuilding for the same reasons: Ethnically polarized societies should be less able to cooperate in a peace than ethnically homogeneous or very heterogeneous societies.

HYPOTHESIS 6. The probability of PB success is higher, the higher are per-capita income and overall level of economic development.

More developed economies with lower levels of poverty should be both better able to rebuild after war and less susceptible to wars stemming from economic grievance. Many studies show that poverty motivates large-scale violent conflict (e.g., Collier and Hoeffler 2000). Thus, the greater the local capacity, the easier it will be to compensate for war-generated hostility, and the higher is the probability of PB success.

HYPOTHESIS 7. A related hypothesis is that the risk of new war—the probability of PB failure—should be higher in very resource-dependent countries.

Examples of this relationship are the civil wars in Angola, Sierra Leone, and Liberia, where the rebels financed their activities by diamond looting, which itself could have motivated the wars (Collier and Hoeffler 2000). Natural resource dependence further implies an undiversified economy, which is more vulnerable to commodity price shocks and lacks significant manufacturing and service sectors that develop human capital and facilitate economic growth.

HYPOTHESIS 8. The probability of PB success is higher when the war ends with a peace treaty.

Treaties are indicators of relatively low postwar levels of hostility because, at the moment of signing, they typically represent the parties' will to end the violent phase of their conflict. Furthermore, treaties enable international involvement in the form of loans, foreign aid, transfers of goods and services, and the deployment of peace operations. Treaties should be significant factors in peacebuilding, while controlling for related variables.

Hypothesis 9. The probability of PB success is higher if UN peace operations are employed.

UN intervention signals international interest in ending the conflict and offers needed assistance to the parties. Also implied is the transfer of badly needed aid and technical expertise, which can reduce war-related hostility and boost domestic capacities, as outlined in our interactive model.

HYPOTHESIS 10. The probability of PB success is higher, the stronger is the UN peace operation and the more extensive its mandate.

It is important for the UN and other multilateral actors to become involved in a peace process, but the right mandate should be used. We expect monitoring, traditional, and multidimensional PKOs to have a different effect from peace enforcement. All such operations, however, should increase the probability of PB success.

EMPIRICAL FINDINGS AND INTERPRETATION

The hypotheses were tested by estimating logistic models of peacebuilding outcomes with robust standard errors and clustered same-country observations.22 Our models include at least two proxies for each of the three core variables—hostility (H), local capacities (LC), and international capacities (IC).23 We controlled for systemic factors by adding a dummy and interaction variables for Cold War (COLDWAR).24 Since we believe that the Cold War was more relevant in determining violence levels than democratization levels, and since there has been a clear democratization trend over time in most countries, we use the Cold War as a control variable for the lenient (violence-focused) PB model; a variable denoting the Decade in which the War Started (DECADE) is a control variable for the strict (democratization-focused) PB model. Table 2 presents the results of the model, estimated two and five years after the end of the civil war for both strict and lenient PB. We also use two democratization thresholds, low and moderate, to code the strict PB version to test the model's robustness.

Strict Peacebuilding Success (PB\$2\$3)

Our theoretical model is strongly supported by the empirical evidence. In models A-D, international and local capacity and hostility variables are all significant determinants of successful peacebuilding. Local capacities are the least robust, perhaps because they reflect prewar levels, and one would expect wars in different countries to have a differential effect on postwar

development capacity.25 We find that war-related hostility (LOGDEAD) substantially reduces the likelihood of PB success and that greater international capacities increase the probability of PB success. Notice that by signing a treaty and inviting a UN peace operation the parties are several times more likely to achieve peace (see the odds ratios for those two variables). We also find that higher levels of Net Current Transfers per Capita (TRNSFCAP, which includes unilateral transfers, food aid, and so on) substantially increase the probability of PB success, as do higher levels of Economic Development (DEVELOP), whereas that probability is reduced significantly if the country is heavily Resource Dependent (EXP), which suggests a low level of economic development and an undiversified economy. Wars with an ethnic or religious overtone are less likely to be resolved, as are wars with a large number of factions (more on the nonmonotonic effect of factions later). Finally, these relationships become stronger when our definition of peace is stricter (compare models A and B to C and D). For strict PB, local capacities are more significant, and longer wars support the PB process (model D). Our control variable suggests that strict PB has become less likely over time (since the coefficient of decade is negative).

Lenlent Peacebuilding Success (PBS2L)

Our model is slightly less accurate with respect to lenient PB. Whereas models A-D correctly predict on average 85% of the cases in our data set, models E-F correctly predict 80% of cases. LC variables are now not significantly correlated with PB, and only resource dependence continues to have a significant and negative effect among LC variables (one-tailed test). Treaties are also less significant (the odds ratio drops by about half), although hostility variables (deaths, displacements, war type, and number of factions) become much more significant (the odds ratios double in some cases). International capacities are still important, but net current transfers (TRNSFCAP) become more significant, and peace operations become marginally significant. War Duration (WARDUR) is not significant in ending violence, which seems to contradict the warweariness hypothesis (although this effect is also driven by a few outliers; see below). The Cold War is highly significant in the very short run, increasing the likelihood of lenient peace more than fourfold, which suggests that systemic constraints were important in preventing war/violence.

The results for both the strict and lenient model are broadly consistent in the two- and five-year periods, which strengthens our confidence in the model.²⁶ We

²² Clustering allows us to relax the assumption of independence among same-country observations. We continue to assume independence among civil wars that took place in different countries.

²³ For LC variables, we use natural resource dependence (EXP) and economic development (DEVELOP) as proxies, and economic development we proxy by electricity consumption per capita. These data are often not available before 1960, so we have input missing values from other variables that are theoretically and empirically correlated with the LC variable in question. For example, we input missing observations of electricity consumption from data on 1960 GDP per capita, so we do not use GDP in the same regression with electricity consumption per capita.

²⁴ We created interaction terms between the Cold War and UN operation type (UNOPS), economic development, and identity wars, but they were not significant. We report only the results of models in which the Cold War is entered independently.

²⁵ We used prewar measures of LC variables to avoid endogeneity (reverse-causality) problems and to forge a link with the literature, which has identified several LC variables (e.g., economic development levels) as significant deterrents to civil war initiation. We would expect that relationship to hold for the postwar period also, and to apply to peacebuilding processes.

We tested the robustness of our results to different subsamples of the data by dropping the following: highly internationalized cases of civil war; cases that we coded as PB fallures due to divided

anticipate some outliers, given the idiosyncratic nature of many wars, so our classification success of 80–85% is satisfactory.²⁷ Perhaps more important, the model wrongly predicts a PB success for an actual PB failure only 7% of the time (i.e., the model errs on the side of caution), and robustness and diagnostic tests convinced us that the models are well-specified and robust.²⁸

Our main interest is to explain how successful, democratizing PB can be achieved, especially as a result of international efforts. Therefore, we focused closely on model A (strict PB for the short term). Sensitivity analysis as the result of small specification changes is reported in Table 3 (where we give odds ratios and z-values). We do not focus on classification success in Table 3; rather, we test the effect of specification changes on the coefficients and standard errors of key variables. Tables 2 and 3 provide a test of the ten hypotheses.

Policy Hypotheses Tests

In what follows, we mainly discuss the short-term (two-year) strict PB model, and we refer to the lenient model's results, ending the war, only if they differ significantly from the strict model, which also includes promoting democratization.

Hypothesis 1: Accept. Identity Wars (WARTYPE = 1) are highly significant and negatively correlated with peacebuilding success for both the lenient and strict PB models. This is an extremely robust variable across all specifications of the model, but its odds ratio is quite small. In model A, Table 2, and models A1-A8, Table

sovereignty; cases that may be ambiguous (due to the paucity of available data); and all monarchies, since our strict PB variable may be biased against such political systems. The results were generally year robust

The two observations with the largest Pearson's residuals are Pakistan 1 (Bangladesh's war of independence) and Philippines 2 (Moro National Liberation Front). These are also the two most influential cases (in terms of change in deviance) in reducing the model's classification success. When we dropped these two extreme outliers, all explanatory variables were highly significant (at the .05-.01 level), and the model's classification success improved. Other statistically influential cases are the Rwandan war (1990-94), Sri Lanka's ongoing war with the Liberation Tigers of Tamil Belam (LTTE), the Ethiopian-Entrean war (1974-91), Angola's war (1975-91), and the China-Tarwan war (1947).

These tests include: Pearson's chi-square goodness-of-fit; specification link tests for each of the two single-equation models; classification tables with percentages of correctly classified observed positive and negative outcomes; receiver operating characteristics (ROC) curves; the Hosmer-Lemeshow influence statistic, deviance statistics; Pearson's residuals' and graphs identifying outliers and their relative influence on the predicted probabilities. A technical discussion of these tests can be found in Stata 1999.

International peacebuilding is time-sensitive in many ways. Most countries and organizations have tight deadlines and limited horizons when extending military and economic aid to war-torn states. After two to five years, moreover, accidents (hurricanes, droughts) and other factors that have little to do with either the success or failure of peacebuilding strategies enter into the determinants of the stability of a country. Thus, we focus on the short term. In the longer term, at 10 years out, our model loses much of its predictive capacity and most key variables become nonsignificant; the number of cases in our data set also drops to about half the number in our two- and five-year models

3, the odds ratio for war type never rises above .2, which suggests that the influence of this variable is consistently negative, but its overall effect on the probability of PB success is relatively small. Thus, although identity wars have a lower probability of successful PB than nonidentity wars, that difference is not very large. For lenient PB (models E–F), the odds ratio of identity wars is slightly larger, but it is still small relative to other variables. This small increase in the odds ratio suggests that war type is more relevant to ending the violence than to the process of political reform during peacebuilding.

Hypothesis 2: Accept. Human misery created by the war is consistently and negatively associated with PB success, both lenient and strict. This result holds with respect to both the absolute level of Deaths and Displacements (LOGDEAD in all models of Table 2) and for Per Capita Death and Displacements (DEAD-CAP, model A2, Table 3). The odds ratio of deaths and displacements is considerable, and it is larger for the lenient than for the strict version of PB. We find consistent evidence that hostility variables are more significant for ending the violence than for democratization once the conflict is over.³⁰

Hypothesis 3: Accept with Reservations. War duration (WARDUR) is positively correlated to PB success in both lenient and strict models (Table 2), but it is only significant for strict PB. If we drop the two largest outliers, however, WARDUR is highly significant at the 0.001 level.31 We find war duration is not robust to different specifications of the model (see Table 3) due to its correlation with hostility and local capacity variables.32 If we were to drop the deaths and displacements variable, war duration would lose all significance. It is more significant for strict PB with higher democracy thresholds (models C and D, Table 2), which suggests that war weariness may influence the political reform process rather than the decision to stop fighting. Finally, if our observations are clustered by broad geographical region rather than by country (since regional influences are important in peace pro-

³⁰ Walter (1997), who conducted Pearson correlation tests between negotiated settlements in civil wars and different measures of war intensity, including duration and magnitude (deaths per capita) of the conflict, found that these two are both positively associated with negotiation. Our findings are not comparable, since we have added cases and focus on what happens after the war rather than on negotiation. We also use fully specified regression models as opposed to partial correlations.

³¹ Moreover, duration could be endogenous if the previous conflict, or at least its length and intensity, is caused by some failure of the major powers or the international community, which may want to rebuild their reputation. To do this they must "signal to excess," that is, act with such force that no one lacking the full resolve for the future would go that far. (Avinash Dixit points out to us that this phenomenon is neatly illustrated in George V. Higgins's crime novel about the Boston mafia, Cogan's Trade)

²² As we anticipated, war duration is positively correlated with deaths and displacements and with development levels. Its partial correlation with war duration and per-capita deaths (DEADCAP) is 50% higher than its correlation with deaths (LOGDEAD), which makes war duration lose significance in model A2 (Table 3), where we control for DEADCAP. Regan (2000) also finds that civil war duration is greater in more intense wars.

independent Varlables	Low Dem.	Low Dem.		Model D Strict PB, 5-Year Perlod, Moderate Dem.	Model E Lenlent PB, 2-Year Perlod	Model F Lenlent PB, 5-Year Period
Constant	10.31 ** (2.42)	9.62** (2.46)	7.24** (1.91)	7.65** (2.27)	5.40** (1.72)	5.48** (1.81)
WARTYPE (Identity-based war? I.e., ethnic/religious)	−2.08** (.661) .124	-2.19** (.704) .111	-1.94** (.609) .143	-2.14** (.706) .116	-1.65** (.471) .191	-1.77** (.48) .169
LOGDEAD (natural log of deaths and displacements)	534 ** (.147) .585	437** (.149) .645	465** (.146) .627	−.478 ** (.167) .619	337** (.122) .713	- 263* (.129) .768
WARDUR* (duration of the war In months)	.009* (.005) 1.009	.009* (.005) 1.009	.012** (.0056) 1.01	.014** (.006) 1.01	.0017 (.003) 1.00	.0027 (.003) 1.00
FACTNUM (number of major factions)	-1.76** (.685) .171	-1.88** (.673) .151	-1.18 (.649) .304	-1.36 (.739) .254	982 (.576) .374	-1.10* (.577) .331
FACTNUM2 (square of number of factions)	.114** (.056) 1.121	.122* (.055) 1.13	.0709 (.0579) 1.07	.077 (.067) 1.08	.048 (.053) 1.049	.058 (.054) 1.06
TRNSFCAP (net current account transfers per capita, current US\$)	.0038* (.0015) 1.003	.0038* (.0015) 1.003	.0029* (.0012) 1.00	.0028* (.0014) 1.00	.003 (.002) 1.003	.004* (.002) 1.004
UNMANDATE (type of UN operation, by mandate type)	.706** (.271) 2.027	.684* (.282) 1.98	.694** (.261) 2.00	.742* (.313) 2.10	.364 (.211) 1.43	.342 (.211) 1.41
TREATY (partles signed treaty to end the war)	2.08** (.763) 8.017	1.75* (.804) 5.76	1.52* (.783) 4.58	1.60 (.947) 4.97	1.70* (.710) 5.48	1.32 (.749) 3.76
DEVELOP* (prewar per-capita electricity consumption)	.00057* (.00036) 1.00	.0006 (.0004) 1.00	.0008** (.0003) 1.00	.0009** (.0003) 1.00	.00026 (.0002) 1.00	.0002 (.0002) 1.00
EXP [®] (primary resource exports as %GDP)	-5.3* (2.98) .0048	-5.85* (3.33) .0028	-5.68* (3.50) .0033	-7.129* (4.34) .0008	-5.26* (2.91) .005	-5.38* (3.07) .004
DECADE (0-6: decade war started)	349* (.174) .705	289 (.166) .748	222 (.186) .800	167 (.206) .845	_	_
COLDWAR (0 = war started after 1989; 1 = otherwise)	_	_	_	_	1.56** (.582) 4.76	.995 (.553) 2.70
Observations	122	117	122	117	122	117
.og-likelihood	-45.236	-45.357	-45.312	-40.993	-57.879	-57.603
Pseudo-R ²	0.4286	0.4062	0.3947	0.4187	0.3069	0.2833
Correctly classified	85.25%	82.91%	86.07%	88.03%	80.33%	79.49%

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cesses), we find that war duration is significant at the 0.05 level in the core specification of model A of Table 2 (results available from the authors).

Hypothesis 4: Accept. Number of Factions (FACTNUM) is significant and negatively associated with PB success, both strict and lenient (Table 2). The quadratic term (FACTNUM2) is positively associated with PB success, as theorized, but it is only significant for strict PB, for the lower democracy threshold. Thus, we can accept the theorized nonmonotonic relationship between number of factions and PB only for the strict version of PB and only for low levels of democratization. Beyond that, the number of factions has a strictly negative influence on PB.³³

Hypothesis 5: Reject. Ethnic Heterogeneity (EH) is not significantly correlated with PB success (model A1, Table 3). Its mean value is only slightly lower (less than 4 percentage points) in PB successes than in failures (both strict and lenient). More important, the coefficient of EH is positive, which contradicts our hypothesis (although the coefficient changes sign if we drop other variables from the model). We also used an index of Ethnolinguistic Fractionalization (ELF), which is common in the literature (e.g., Mauro 1995) and which measures the probability that any two randomly selected people from different ethnic groups speak a different language. This variable also was not significant (results available from the authors). In addition, we entered ethnic heterogeneity as a quadratic, following some of the literature on civil wars (Elbadawi and Sambanis 2000), but again we found no significance. Furthermore, dropping identity wars and number of factions from the model did not improve the significance of the EH variable.

This is an important result, because several authors have identified ethnolinguistic fractionalization as a key variable in the economic literature on the onset and duration of civil wars (Collier and Hoeffler 2000; Collier, Hoeffler, and Soderbom 1999) and in the literature on economic growth (Alesina, Easterly, and Baquir 1997; Easterly and Levine 1997; Mauro 1995). Our finding could be due to a selection effect, especially if ethnic diversity is a significant determinant of the initiation of civil wars. Yet, Collier and Hoeffler (2000) find that linguistic and religious diversity actually reduces the risk of civil war, which does not suggest that our results are due to selection problems. We are inclined to interpret the difference between the importance of ethnicity for war duration as opposed to peacebuilding as evidence that religious or ethnic wars are difficult to settle, but the mere presence of ethnic diversity does not mean that populations will resume the fight.

Hypothesis 6: Accept Strict PB; Reject Lenient PB. The overall level of economic development (DEVELOP) as

proxied by per-capita consumption of electricity is positively correlated with PB success in both lenient and strict models (Table 2). It is weakly significant for strict PB with a one-tailed test (model A) but not for lenient PB (models E, F). We must reject our hypothesis for the lenient model: Positive local capacities seem consistently irrelevant for lenient PB, that is, for ending the violence. Yet, local capacities facilitate strict peacebuilding (compare the coefficients and significance levels of economic development in models C and D, where we used a higher threshold of democratization for coding a PB success). Finally, clustering observations by geographical region reveals that economic development is significant at the 0.05 level in the core specification of model A of Table 2 (results available from the authors).

Hypothesis 7: Accept. Natural Resource Dependence, proxied by the share of primary exports in GDP (EXP), is significantly and negatively associated with PB success, both lenient and strict (one-tailed test). This finding effectively links our research to the economic literature on the causes of civil wars, which identifies natural resource dependence as a main culprit in loot-seeking civil wars (Collier and Hoeffler 2000). Easily looted resources provide incentives for new wars, which would reduce the probability of PB success. Our finding is relatively robust to different specifications of the strict PB model.

Hypothesis 8: Accept. The hypothesis that treaties are positively correlated with PB success is resoundingly accepted for both the lenient and strict PB models (Table 2). It is notable that other war outcomes are not significant determinants of peacebuilding. For example, Military Victory (MILOUT in model A3, Table 3) is completely nonsignificant, and this finding does not change if we discriminate between government and rebel victory (the latter results are available from the authors). Informal truces are not significantly associated with strict PB, and they have a negative sign.

Hypothesis 9: Accept Strict; Reject Lenient. UN Involvement (UNINV), without specifying the operation's mandate, is positively and significantly correlated with strict PB (model A4, Table 3), but it is not significant for lenient PB.34 Note that there is a high positive correlation between UN involvement and Treaty (TREATY) (38%), since treaties are necessary for many UN PKOs. When we drop treaty from the lenient model, UN involvement becomes significant at the 5% level. It seems that treaties do the heavy lifting, however, and we must acknowledge that simple UN involvement is not enough to strengthen a peaceful transition. The operation's mandate is critical.

Hypothesis 10: Accept. UN peace operations classified according to mandate are highly significant determinants of PB, but this relationship is quite complex.

³³ In the lement model the quadratic term for number of factions (FACTNUM2) loses significance when we control for deaths (LOG-DEAD). Both are proxies for hostility, and they are positively correlated (28%), which may explain the low significance level for FACTNUM2.

^{**} Results are not reported to save space. The coefficient of UN involvement (UNINV) becomes 375, with standard error = 521.

	Model A1	Model A2	Model A3	Model A4	Model A5	Model A6	Model A7	Model A8
WARTYPE (Identity war?)	.109** (-3.07)	.129** (-3.58)	.131 ** (-2.97)	.133 ** (-3.07)	.15** (-3.03)	.150** (-3.03)	.150** (-3.15)	.175** (-2.86)
LOGDEAD (dead/displaced)	.579** (-3.53)	_	.597** (-3.36)	.618 ** (-3.57)	.621 ** (-3.58)	.628** (-3.57)	.631 ** (-3.73)	.641** (-3.54)
DEADCAP (dead/displaced per capita)	_	.011* (-1.93)	_		_		<u> </u>	
WARDUR (duration in months)	1.008 (1.68)	1.00 (1.23)	1.009 (1.71)	1.008 (1.57)	1.00 (1.48)	1.00 (1.46)	1.00 (1.48)	1.00 (1.13)
FACTNUM (number of factions)	.177* (-2.49)	.137 ** (-3.13)	.171* (-2.487)	.209* (-2.41)	.319 (-1.83)	.338 (-1.72)	.351 (-1.69)	.284 (-1.79)
FACTNUM2 (square of FACTNUM)	1.115* (1.89)	1.145* (2.48)	1.12* (1.982)	1.10 (1.84)	1.0 6 (1.22)	1.05 (1.06)	1.05 (1.03)	1.06 (0.94)
EH (ethnic heterogeneity)	1.005 (0.79)	_	—	_		-	, . -	
TRNSFCAP (net current transfers)	1.003* (2.29)	1.00 ** (3.09)	1.003* (2.33)	1.00* (2.41)	1.00* (2.24)	1.003* (2.25)	1.003 (1.79)	1.004** (2.58)
UNMANDATE (type of UN ops.)	2.048** (2.68)	1.72* (2.21)	2.12** (2.55)	<u> </u>			· -	
UNINV (any UN action?)	_	_	_	3.82* (2.13)	· —	.	_	
UNOP2 (UN observer mlsslon)	_	_	_		1.91 (0.94)	·. —		· —
UNOP5 (UN enforcement)	_	_	- .	_	_	2.148 (0.71)	··· —	_
UNOP3 (traditional PKO)	_	_	_	_	_	_	1.287 (0.188)	<u> </u>
UNOP4 (multidimensional PKO)	_	_	_		<i>_</i>	- .	_	22.99** (2.87)
TREATY (treaty signed)	8.09** (2.75)	5.22* (2.18)	12.42* (2.29)	8.94 ** (2.91)	14.17 ** (3.48)	14.74 ** (3.53)	14.45** (3.57)	8.367** (2.58)
MILOUT (military victory)	_	_	2.09 (0.65)	_	_	_	. —	<u> </u>
DEVELOP (electricity cons.)	1.00 (1.64)	1.00* (2.16)	1.00 (1.82)	1.00 (1.87)	1.00 (1.57)	1.00 (1.55)	1.00 (1.58)	1.00 (1.84)
EXP (primary resources)	.0037 * (-2.01)	.087 (-0.74)	.006 (-1.65)	.003 (-1.93)	.001* (-2.36)	.001* (-2.30)	.000* (-2.34)	.002* (-1.96)
DECADE (decade war started)	.701* (-2.03)	.720 <u>*</u> (-1.99)	.708* (-1.93)	.677* (-2.07)	.75 (-1.69)	.750 (-1.68)	.749 (-1.69)	.741 (-1.77)
Observations	122	122	122	122	122	122	122	122
Log-likelihood	-44.994	-51.504	-44.900	-46.473	-48.051	-48.324	-48.424	-45.648
Pseudo- <i>R</i> ²	0.4317	0.3495	0.4329	0.4130	0.3931	0.3896	0.3884	0.4234

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TABLE 4. Logistic Models of Peacebuilding Success Two Years after the War, Using Indices of Hostility, Local Capacities, and International Capacities

Independent Variables	Strict PB,	Lenlent PB,	Strict PB,	Lenlent PB,
	2 years	2 years	5 years	5 years
Constant	1.21**	1.65**	1.32*	1.79**
	(.547)	(.5 9 0)	(.572)	(.624)
Hostility (H) Index	-7.32**	-6.59**	-7.331**	−6.76**
	(1.749)	(1.53)	(1.78)	(1.59)
	.0006	.0013	.0006	.001
Local capacity (LC) Index	3.68**	2.44*	3.73**	2.567*
	(1.359)	(1.21)	(1.44)	1.289)
	39.865	11.580	42.019	13.03
International capacity (IC) index	5.73**	4.42*	5.40**	4.203**
	(2.151)	(1.94)	(2.17)	(1.97)
	308.696	83.507	222.475	66.95
Observations	119	119	114	114
Log-likellhood	-63.626	-71.096	-62.223	-68.739
Pseudo-R ²	0.1765	0.1280	0.1644	0.1231
Correctly classified	73.95%	66.39%	73.68%	67.54%
Proportionate reduction in error	24.88%	21.36%	25.41%	25.72%

Note: Reported are coefficients (robust coefficient standard errors) and odds ratios in that order. $^*p = .05$, $^{**}p = .01$; two-tailed tests. The possible range for the indices is from 0 to 1. Variables are entered in the indices linearly, independently, and without weights. Our results are robust to several different constructions of all indices. We include treaty in the IC index because it is highly correlated with UN operations, and in many cases treatise were eighted as a result of external involvement. The large difference in the odds ratios of the indices is due to the differences in the actual ranges of each index. Soaling each index by its variance and receitmating the model would adjust the odds ratios. For example, with the scaled indices as regressors, the two-year strict peacebuilding model produces odds ratios for the hostlity, local capsorties, and international capacities indices of .8753, 1.087, and 1.095, respectively

Notice that *UN Mandate* (UNMANDATE) is positively and significantly correlated with strict and lenient PB (Table 2), but the coefficient drops by about half in the lenient version (models E and F). What does this suggest? It is not possible to identify the influence of specific operation types unless we unpack it into its components. We did this and focused specifically on *Observer Missions* in model A5, enforcement in model A6, traditional peacekeeping in model A7, and multidimensional PKOs in model A8 of Table 3.

Diplomatic efforts, UN Mediation (UNOP1), alone is an insufficient strategy. The coefficient is not significant and is negative (not presented). But an observer mission (UNOP2) sent to monitor a truce or treaty does make a positive, although not large or significant, difference. These operations offer additional transparency but no commitment of resources (military and economic) to influence the incentives of the parties to resolve a violent conflict.

Model A6 replaces the previous UN variables with a dummy variable for *UN Enforcement* (UNOP5). It is positively associated with strict PB success but is not significant (the same is true for the five-year version of the PB variable). Thus, the international community cannot count on enforcement alone to build lasting peace,³⁵ but it appears to have a positive role in ending violent conflict.

Models A7 and A8 reveal perhaps the most interesting results with respect to UN operations. Traditional

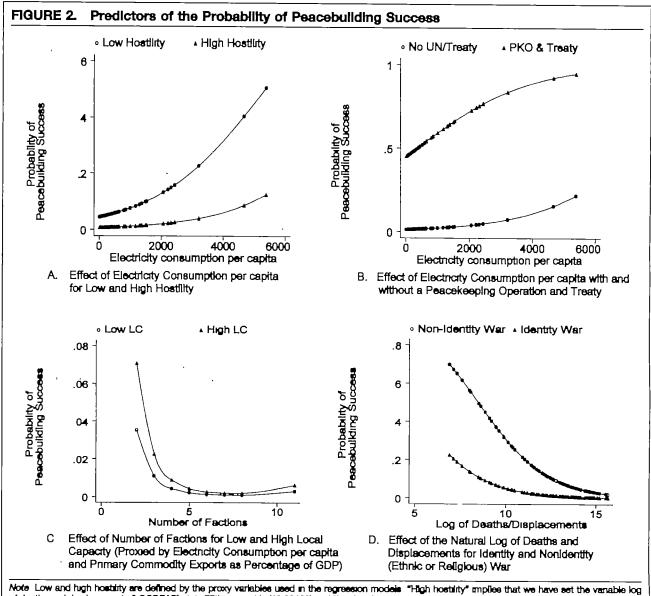
Peacekeeping (UNOP3) is not at all significant in enhancing the prospects for PB success, and it even has a negative sign in the lenient version (results available from the authors). In that case, one observes the syndrome of "peacekeeping without a peace to keep." By contrast, Multidimensional PKOs (UNOP4)—missions with extensive civilian functions, including economic reconstruction, institutional reform, and election oversight—are extremely significant and positively associated with strict PB, a participatory peace. (Notice the high odds ratio of multidimensional PKOs in model A8, Table 3.)

In sum, UN enforcement can help end a war but alone has little effect on institutionalizing participatory peace (our strict PB). By contrast, multidimensional PKOs seem to contribute both to reduced violence (lenient PB; results available from authors) and crucially, to the strict version of PB, which involves institutional and political reform, elections, and democratization. (For further discussion, see Appendix D.)

Index Models of Peacebuilding Success

Having completed our tests of the policy-relevant hypotheses, we return to the core model, aggregate our proxies for hostility, local capacities, and international capacities to three indices, and use these as regressors in estimating the models of strict and lenient peace-building. (The component variables for each index are listed in Appendix B.) We ran many regressions with several combinations of those indices to ensure that the results were not driven by the construction of the index.

²⁵ If enforcement is followed by more diversified missions, however, the results may be different. We only have four cases of enforcement in our data, so these results should be interpreted with caution.



Note Low and high hostility are defined by the proxy variables used in the regression models. "High hostility" implies that we have set the variable log of deaths and displacements (LOGDEAD) at its 75th percentile (13.83109) and "low hostlity" implies setting the variable at the 25th percentile (10.12683). PKO stands for peacekeeping operation, either traditional or multidimensional (for no UN PKO and no treaty, N = 86, for presence of both, N = 15) "Low local capacities" implies that we have set electricity consumption per capita at the 25th percentile (48 kwh) and primary commodity exports as percentage of GDP at the 75th percentile of leathing capacities" are defined in exactly the opposite manner (75th percentile of electricity consumption is 639.0475 kwh and 25th percentile of primary exports as a percentage of GDP is .0880658). Identity wars are ethnic and retigious wars (N = 79). Nonidentity wars are kleology-driven revolutions, loot-driven wars, or other nonethnio, nonreligious wars (N = 45). In Figure 2C, the numbers of factions

Our findings are summarized in Table 4 for a single combination of indices.

We regressed the indices on the two- and five-year versions of lenient and strict PB. We found all three to be highly significant in all models, and their coefficient signs are those predicted by our PB triangle theory. High levels of international and local capacities compensate for high levels of hostility. In varying the composition of the indices, however, we found more significant combinations for the hostility and international capacities indices than for the local capacities index, especially with reference to the lenient PB model. Thus, the results of the index models are in line with the previous discussion of individual proxies.

COMPARATIVE STATICS AND POLICY ANALYSIS

Armed with these findings, we can use the core model to analyze how the interactions between key explanatory variables influence the probability of PB success using conditional effects plots. The four panels of Figure 2 graph the estimated probability of strict PB success when we set a key variable at either the 75th or the 25th percentile of its range while allowing another key regressor to vary throughout its range. Nonvarying regressors are set at their median level.

Figure 2A maps the probability of PB success across all levels of electricity consumption (kwh) per capita for the 75th and 25th percentile of the range of the log

of deaths and displacements (LOGDEAD), which proxies high and low levels of hostility, respectively. We see clearly that not only is PB success much more likely if hostility is low (contrast the circle-studded with the triangle-studded line), but also the rate of increase in the probability of success rises faster as local capacities rise. Local capacity (economic development, measured by electricity consumption) appears to make a reconciliation easier, especially if the two sides have avoided the worst forms of mutual violence.

Figure 2B maps the probability of PB success across all levels of electricity consumption (kwh) per capita (i.e., variable local capacities) with and without a UN peacekeeping operation (PKO) and treaty. The probability of PB success is remarkably higher if a PKO is used. At high levels of local capacity, the probability of success when there is a treaty and a PKO is near unity. The difference is great also at low levels of local capacity: A treaty and PKO substitute for the lack of local capacity (contrast the two lines at very low levels of electricity consumption per capita). The differential effect of a UN PKO and a peace treaty is maximized at middle levels of local capacity.

Figure 2C maps the probability of PB success by number of factions, and for low and high levels of local capacity. Low local capacity is indicated by the values that define the 25th percentile of the range of electricity consumption (kwh) per capita and the 75th percentile of the range of primary exports as percentage of GDP. High local capacities imply values that define the 75th percentile of electricity consumption (kwh) per capita and the 25th percentile of primary exports as a percentage of GDP. The number of factions has a clearly nonmonotonic (U-shaped) effect on the likelihood of peacebuilding. The probability of success is lowest between five and seven factions. It is highest when the number of factions is small, but overall there is not much interaction between the number of factions and local capacity levels, since the difference between the lowest and highest quartiles in the probability of success is quite small.

Figure 2D maps the probability of PB success by level of the log of deaths and displacements (which is the main measure of hostility) for identity and nonidentity wars. There is a huge difference in the probability of PB success of low levels of hostility (deaths and displacements), and, in general, nonidentity wars are most likely to result in a PB success. Ethnic identity wars are four times harder to resolve at extremely low levels of deaths and displacements, but at extremely high levels of deaths and displacements, both types of war are equally unlikely to result in successful peacebuilding. This suggests that war type is overwhelmed by the hostile effects of very large numbers of deaths and displacements, whatever their source. Cambodia, an ideological war with a nonethnic massacre of more than a million people, and Rwanda, an ethnic identity war with a genocide of more than 500,000, are similarly challenging cases in terms of achieving PB success. This plot emphasizes as well the importance of early intervention, before the parties have done extensive killings and especially in a nonidentity war. That is when the probability of PB success is greatest.

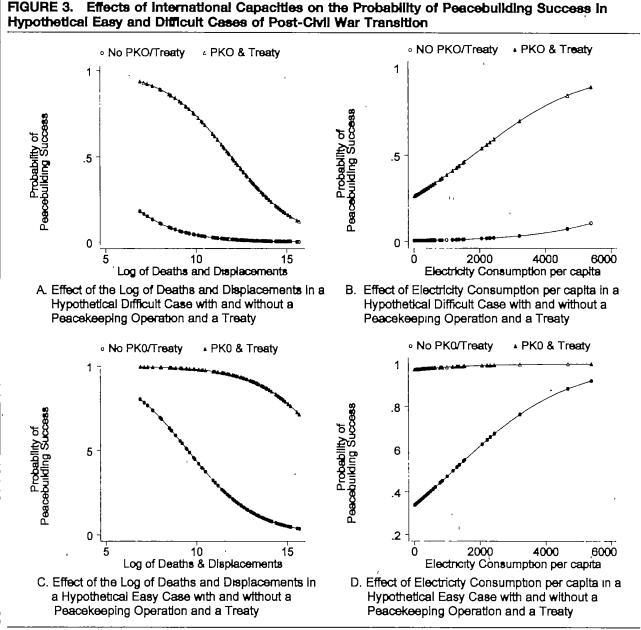
Using model A of Table 2, we can compute the probability of strict PB success for conflicts that have just ended. A perhaps more valuable product of our analysis is that it allows us to assess the contribution of international capacities to the resolution and prevention of future civil war. Using our model, we can identify broad guidelines for PB strategies after civil war, given different levels of local capacities and hostility. Since we focus on the role of UN operations, we draw some broad guidelines for UN involvement using Figure 3.

For simplicity, imagine that peacebuilding processes can be divided into difficult and easy cases. In a hypothetical difficult case, all the variables with a negative coefficient in our model would have high values (we set them at their 75th percentile), and all the variables with positive coefficients would have low values (we set them at their 25th percentile).³⁷ In Figure 3, we create hypothetical difficult and easy cases and explore the effect of international capacities on the probability of peacebuilding success under different combinations of local capacities and hostility levels. Figure 3A and B represent two hypothetical difficult cases, whereas Figure 3C and D represent two easy cases

Figure 3A maps the probability of PB success in a difficult case across all levels of the log of deaths and displacements with and without a UN PKO and peace treaty. This figure represents a hypothetically difficult case because we have set local capacity variables at their 25th percentile. The results are striking: A difficult case without a treaty or PKO, even at the lowest level of hostility (deaths and displacements), has a very low likelihood of PB success, several times lower than with a PKO and a treaty. Peacekeeping does make a positive difference, and early intervention pays. But at very high levels of hostility, after massive civilian slaughter, the two probabilities converge to low levels, although there is still a slightly greater chance of success with a PKO and treaty. For example, a substantial multidimensional PKO made a positive difference in Cambodia, despite the killings and displacements that took place there; an equivalent effort might have been useful in Rwanda.

³⁶ The probability that the religious civil war in Algeria will result in strict peacebuilding success two years after it ends is small (.097), but there is a high standard deviation (.085). Success is even less likely for the Democratic Republic of the Congo (.042). Deploying a PKO in the Congo would increase the probability to an estimated .191, with a standard deviation of .147, that is, the 95% confidence interval for the actual probability would be between 2% and 57%. To obtain these estimates, we used the "Clarify" software (Tomz, Wittenberg, and King 1999).

³⁷ The variables with negative coefficients are WARTYPE, LOGDEAD, FACTNUM, EXP, DECADE. The variables with positive coefficients are UNMANDATE, TRANSFCAP, DEVELOP, WARDUR. Throughout this exercise, we hold WARDUR, FACTNUM, and DECADE constant at their median. Easy cases imply a nonidentity war (WARTYPE = 0), 75th percentile in net transfers per capita (TRANSFCAP) and electricity consumption per capita (DEVELOP), and 25th percentile in primary exports as percentage of GDP (EXP).



Note: To construct easy and difficult cases we use the parameter estimates from model A. For a difficult case, we set regressors with a negative coefficient to the value for the 75th percentile of their range and the regressors with positive coefficients at the value defining the 25th percentile of their range. We do the opposite for an easy case. If we allow local capacity variables to very (e.g., e.g., the log of deaths and deplacementa), then we focus on hostility is high) and easy cases (Figure 3D, where hostility is low) if we allow the hostility variables to very (e.g., the log of deaths and deplacementa), then we focus on capacity variables to create hypothetical difficult cases (Figure 3A, where capacity is low) and easy cases (Figure 3C, where capacity is high). (See note to Figure 2 for definitions of high and low, capacity and hostility.) We keep the following variables at their median: wer duration, number of factions, and decade. This setup allows us to study the effect of International capacities—peacekeeping operations in confunction with a treaty—on the probability of peacebuilding success under different combinations of levels of hostility and local capacity.

These results are almost the mirror opposite of those for an easy case with high local capacities (i.e., electricity consumption at the 75th percentile and natural resource dependence at the 25th percentile) (Figure 3C). Here, the probability of success is quite high at low levels of hostility (deaths and displacements), regardless of whether a PKO is deployed or a treaty signed. The major effect of the treaty and PKO occurs

at high levels of hostility, where they are crucial in maintaining the probability of PB success. Without a treaty and PKO, the likelihood of success drops substantially from an initial value of between 60% and 70% to less than 5% at extreme values of hostility. This appears, for example, to map the situation in Bosnia today, a more developed country that has suffered many casualties and is held together in peace by

NATO, the UN, and a plethora of other international organizations.

Figure 3B maps the probability of PB success for a difficult case across all levels of electricity consumption per capita with and without a UN PKO and peace treaty. This figure represents a hypothetically difficult case because we have set hostility variables at their 75th percentile. We see that a treaty and PKO are even more important for PB success since the slope of the curve with a PKO gets much steeper much sooner than the slope of the curve without a PKO, and the resulting probability of success without a treaty/PKO is minimal even at extremely high levels of economic development.

By contrast, Figure 3D maps the probability of PB success across levels of electricity consumption per capita (i.e., across levels of local capacities) for a hypothetical easy case with and without a UN PKO and peace treaty. This figure represents a hypothetically easy case because we have set hostility variables at the 25th percentile. The effect of a PKO and a treaty is highest at very low levels of development, whereas neither a treaty nor a strong international presence seems necessary for PB success at very high levels of development. Developed countries that experience minor civil violence can put themselves back together. The UN is most needed elsewhere, in the less developed countries that have suffered extensive violence.

CONCLUSION

Our analysis identifies the critical determinants of peacebuilding success. We find that higher order, or democratic, peacebuilding is more successful after nonidentity wars, after long and not very costly wars, in countries with relatively high development levels, and when UN peace operations and substantial financial assistance are available. Lower order peacebuilding—an end to the violence—is more dependent on muscular third-party intervention and on low hostility levels rather than on the breadth of local capacities.

Peacemaking aimed at facilitating a peace treaty is potentially life saving, since we find that treaties are highly correlated with an end to the violence. Moreover, strategically designed peacekeeping and peace enforcement do make a difference. International capacities can foster peace by substituting for limited local capacities and alleviating factors that feed deep hostility. Such intervention improves the prospects for peace, but only if the peace operation is appropriately designed. Enforcement operations can end the violence, but alone they cannot promote durable, democratic peace. By contrast, consent-based peacekeeping with civilian functions (multidimensional PKOs), which ordinarily do not have a mandate to end the violence if parties do not cooperate, usually are not successful in ending violence. With a peace treaty and the cooperation of the parties, however, PKOs can assist with the institutional and political reform that helps secure longer term peace. Truly intractable conflicts, such as those in Bosnia, Kosovo, and East Timor, probably will

require both enforcement and peacebuilding operations, coordinated and in the right order.

APPENDIX A: CODING GUIDELINES FOR CASE SELECTION

The data set consists of the 124 events of civil war since 1944. The analysis is confined to those that terminated before 1997 or were ongoing as of December 1999 but had at least one significant settlement, truce, or third-party peace operation since 1997. This allows an evaluation of peacebuilding outcomes at least two years after either the end of the war or the beginning of a peace operation. Because the latter signals the parties' desire to terminate the war and restore peace at some point, we include these few cases.

Our definition of civil war (see page 783) is nearly identical to that in Singer and Small (1982, 1994) and Licklider (1993, 1995). We define termination as signature of a peace treaty or victory by one side. Our coding of war reflects the 1,000 annual battle deaths standard, although we relaxed that threshold in a few cases, when the overall amount and nature of the violence were substantial (e.g., a state fighting against organized rebel groups who have popular support) and most of our other criteria were satisfied. In fact, the 1,000 deaths criterion seems rather arbitrary, and we could not corroborate that all cases in the Singer and Small (1994) data set reached that annual level during all years of the war. Moreover, the Singer and Small (1994) codebook does not refer to an annual death threshold as a coding condition (it appears in their 1982 book, Resort to Arms), and no annual death data are made available by the Correlates of War Project.

Our definition of a civil war allows us to combine observations from several data sets: Singer and Small (1994); the Uppsala University project on civil wars by Wallensteen and Sollenberg (1997, 1999); Esty et al. (1995, 1998); Licklider (1993, 1995); Mason and Fett (1996); Regan (1996); Walter (1997); and SIPRI yearbooks (1987–98). In addition, secondary texts include case studies and official reports: Doyle, Johnstone, and Orr (1997) on Cambodia and El Salvador, Intrides (1993) on Greece; Rotherg (1998) on Burma; Deng (1999) on the Sudan; Stuart-Fox (1998) on Laos; Human Rights Watch reports on Sierra Leone, Democratic Republic of the Congo, Uganda, Kosovo, Bosnia, and Algeria; and U.S. Department of State reports on Bangladesh, Burma, Cambodia, Chad, Djibouti, Egypt, Guinea-Bissau, Laos, Peru, and the Philippines. Other sources were the CIA World Factbook (various years) and Brogan (1992).

The most important difference between our coding of wars and that by other authors refers to periodization. We tried to apply consistently the following criterion: We code a separate event if war recurs after the parties sign a peace treaty or after a prolonged break in the fighting (at least two years). In some cases this criterion collapses two or more observations in other data sets or, conversely, divides a single observation into two or more war events. For more details on our application of this coding rule, see "Country-Specific Comments" in the online documents (http://www.worldbank.org/research/conflict/papers/peacebuilding).

We adopted this periodization rule because of our focus on peacebuilding efforts. We needed to evaluate the success or failure of every significant attempt at peace, and the criteria for separation suggest that war has subsided, which presents an opportunity for peacebuilding. If a PKO is implemented and the war resumes, that is clear evidence of peacebuilding failure, so we needed to include these cases of ongoing war in the analysis.

Several rules of thumb for coding separate war events also were used. (1) If a different war started while a previous conflict was ongoing in the same country, then we coded separate events (e.g., the Tigrean and Eritrean wars in Ethiopia). (2) If the parties and issues changed dramatically, then we coded a separate event (e.g., the Afghan war before and after Taliban). Also, we collapsed two or more events in

other data sets into one if the parties and issues were the same. (3) If less than two years intervened between the first and second event, then we collapsed observations in other data sets into a single war event. (4) If the war ended officially through an agreement but fighting did not subside, then we considered the war ongoing and coded a single event. Mason and Fett (1996, n. 8) do the same.

Varlable	N	Mean	S.D.	Min.	Max.	Source
Hostlity Proxies Ethnic/Religious War? (WARTYPE)	124	0.64	0.48	0	1	Gurr and Harff 1997; Licklider 1995; various secondary sources (see Appendix A)
Deaths and displacements (natural log) (LOGDEAD)	123	11.91	2.41	6.91	15.7	Brogan 1992; HRW reports; Licklider 1995; Singer and Small 1994; various sources
Deaths and displacements per capita (DEADCAP)	123	0.11	0.21	0.00	0.85	As above; population data from UNHCR various years; U.S. Bureau of the Census; World Bank 2000b
War duration in months (WARDUR)	124	78.73	,92.96	1	600	Lickflder 1995; Singer and Small 1994; Wallensteen an Sollenberg 1997; various sources
Number of factions (FACTNUM)	124	3.32	1.52	2	11	Brown 1996; various secondary sources (see bibliography and online supporting documents)
Ethnolinguistic fractionalization Index (ELF)	117	48.90	30.63	0	93	Mauro 1995
Ethnic heterogeneity Index (EH)	124	56.91	34.04	0	144	Vanhanen 1999
Signed treaty (TREATY)	124	0.28	0.45	0 ,	1	Licklider 1995; Wallensteen and Sollenberg 1997; Walte 1997; secondary sources
Military victory (MILOUT)	124	0.60	0.49	0	1	Licklider 1995; Wallensteen and Sollenberg 1997; Walte 1997; secondary sources
International Capacity Proxies	·					
Net transfers per capita (TRNSFCAP)	123	58.43	171.1	-112	1,272.5	IMF 2000; World Bank 2000a
Any UN involvement (UNINV)	124	0.36	0.48	0	1	United Nations 1996; UN Department of Peacekeepir Operations 2000, including fact-finding and envoys (Brown 1996)
UN operation by mandate: No operation, mediation, observer, PKO, enforcement Note: PKOs are aggregated in a single category (UNMANDATE)	124	0.83	1.23	0	4	United Nations 1996; UN Department of Peacekeepin Operations 2000

APPENDIX B: (Continued	<u></u>		-	N. 41		0.0000
Varlable	<u>, N</u> _	Mean	S.D.	Mln.	Max.	Source
UN operation by mandate: No operation, mediation, observer, traditional PKO, multidimensional PKO, enforcement (UNOPS)	124	0.85	1.45	0	5	United Nations 1996; UN Department of Peacekeeping Operations 2000
UN Mediation (UNOP1)	124	0.07	0.26	0	1	United Nations 1996; UN Department of Peacekeeping Operations 2000
UN Monitoring/Observer Missions (UNOP2)	124	0.07	0.26	0	, 1 ·	United Nations 1996; UN Department of Peacekeeping Operations 2000
Traditional PKO (UNOP3)	124	0.06	0.25	0	1 1	United Nations 1996; UN Department of Peacekeeping Operations 2000
MultIdImensional PKO (UNOP4)	124	0.06	0.23	0 .	1	United Nations 1996; UN Department of Peacekeeping Operations 2000
UN enforcement (UNOP5)	124	0.03	0.18	0	, 1	United Nations 1996; UN Department of Peacekeeping Operations 2000
Local Capacity Proxies Economic development (DEVELOP) measured by electricity consumption (kwh) per capita	124	540.8	851.1	10	5,387	World Bank 2000b
Natural resource dependence (EXP) measured by the percent of primary exports in GDP	124	0.12	0.08	0.01	0.51	Coiller and Hoeffler 2000; World Bank 2000b
Democracy (GURR2) and five-year average prewar democracy Index (GURRLAG5)	120	6.017	5.68	, 0	20	Jaggers and Gurr 1999
Other Control Variables Decade dummy for war start (DECADE)	124	3.45	1.43	1 ' -	6	Based on war-start data from Licklider 1995; Singer and Small 1994; and other sources on periodization of wars listed in Appendix A
Cold War dummy (COLDWAR)	124	0.78	0.41	0	1	Coded 1 for war-starts before 1989, 0 after 1989
nstrumental Variables Europe (EURO)	124	0.08	0.27	. 0	1	CIA World Factbook 2000
Real per capita GDP (GDP)	· 123	697.5	1,632.8	130	10,000	Heston and Summers 1995; World Bank 2000b
Third-party partial Intervention (INTERVEN)	124	62.9	48.5	0	1	Regan 1996; various secondary sources
Non-UN peace operation (NONUNOP)	, 124 ,	0.61	1.31	0	4	Durch 1993, 1996; United Nations 1996; various secondary sources

APPENDIX C: CODING GUIDELINES FOR THE DEPENDENT VARIABLES

The dependent variables are Lenient Peacebuilding (PBS2L) and Strict PB (PBS253). Three intermediate variables were used in their construction: War End (WAREND), No Residual Violence (NOVIOL), and Democracy (GURR). Each intermediate variable has two versions, one for the two-year period after the war and the other for the five-year period. Each version is represented by a numerical suffix after the computer abbreviation of the variable name. The coding process was done in four steps.

First, did the war end (was there no recurrence)? We coded the variable war end two and five years after the war: WAREND2 and WAREND5. These are coded 1 if the war ended, 0 otherwise. We used the same sources and rules as for coding war events.

Second, did low-level violence end after the war? We coded the variable no residual violence two and five years after the war: NOVIOL2 and NOVIOL5. These are coded 1 if there was no residual violence, 0 otherwise. NOVIOL2 and NOVIOL5 are coded 0 if war recurred (i.e., if WAREND2 or WAREND5 = 0); if other data sets (Esty et al. 1995, 1998; Regan 1996; Wallensteen and Sollenberg 1997) code an ongoing low-level or intermediate armed conflict; or if a politicide or genocide occurred after the end of the war (Gurr and Harff 1997, 1994; Licklider 1995).

Third, what was the level of democratization after the end of the war? We coded the variable democracy (GURR2 and GURR5) from the Polity98 (June 1999) data as the sum of [Democracy + (10 - Autocracy)], two and five years after the end of the war. This variable ranges from 0 (extreme autocracy) to 20 (maximum democracy).

Fourth, the dependent variables were then coded as follows. Lenuent PB Success Two Years after the War (PBS2L) and Lenient PB Success Five Years after the War (PBS5L) are coded 1 if there was a success, 0 otherwise. PBS2L = 1 (success) if WAREND2 = 1 and NOVIOL2 = 1, and if state sovereignty is not divided (i.e., state authority can be exercised in the entire territory). Otherwise, PBS2L = 0. We coded PBS5L in a similar fashion, but we use the two-year period in our analysis because at the time of writing many important wars had not yet been over for five years.

Strict PB Success Two Years after the War (PBS2S) and Strict PB Success Five Years after the War (PBS5S) are coded as follows: PBS2S = 1 if PBS2L = 1 and GURR2 > (a democratization threshold; see below). Otherwise, PBS2S = 0. We coded the five-year version analogously. The strict version of PB involves more than an end to the violence and is our preferred measure of PB success or failure. We created two subcategories of strict PB-a low democracy threshold (GURR = 3), which we prefer because it places fewer demands on the country shortly after the end of the war, and a higher threshold (GURR = 6)—to code variables PBS2S3 and PBS2S6, respectively. We coded the five-year versions analogously (PBS5S3, PBS5S6). For ongoing conflicts, we used democracy data for 1998 because those are the most recent available from the Polity98 project. Finally, we coded a score of -77 ("interregnum") in the Polity98 data set as a PB failure.

APPENDIX D: DOES THE UN PICK ITS FIGHTS? TWO TECHNICAL ISSUES WITH SUBSTANTIVE IMPLICATIONS

Our analysis suggests that the UN can play an important role in peacebuilding, but not all types of UN operations are right for all jobs. We were concerned that some of our findings might be due to selection effects or influenced by the potential endogeneity of UN peace operations, so we briefly examined both these problems. We found that neither alters our findings.

First, let us summarize the concern about potential endogeneity. Given the high degree of correlation between the deployment, mandate, and strength of UN operations and variables that determine PB outcomes, it is possible that the UN's decision to intervene may be determined by some of these other variables. For example, perhaps the UN picks the easy fights. Conversely, perhaps it decides heroically to intervene only in the severest situations. To test that possibility, we estimated two-stage models of peacebuilding, instrumenting for the UN involvement. We do not present the statistical results in detail, since they do not alter our findings.

First, we identified a number of good instruments for UN involvement, such as a dummy variable for Europe (EURO), Real GDP per Capita (GDP), Third Party Partial Intervention (INTERVEN), Non-UN Peace Operation (NONUNOP), and Military Outcome (MILOUT). Second, we then estimated a two-stage least squares (2SLS) linear probability model, a two-stage probit model, and a bivariate probit model with a selection, and we found no evidence of endogeneity for UN involvement. Third, a Smith-Blundell test of exogeneity for the core strict PB model A, using the instruments identified above, yields a chi-squared(1) test = .4118, with p = .521, which does not allow us to reject the null hypothesis of exogeneity (Smith and Blundell 1986).38 That test leads us to reject exogeneity only if we use treaty as an instrument and not as an exogenous variable in the structural equation, which would be incorrect given the significant relationship between treaty and strict PB. Furthermore, even if we were to assume that the UN involvement is in fact endogenous, a linear probability model of strict PB (specified as above and using treaty as an instrument) yields a highly significant coefficient estimate for UN involvement (.497), with a robust standard error equal to .152.39 Given that most of the models estimated reveal no evidence of endogeneity, we can rely on our previous inferences.

A second possible complication arises from the difference in the standard error of UN involvement in the strict versus lenient PB model, which raises concerns about possible selection effects. That discrepancy may have been due to a different effect of UN peace operations on the probability of ending the violence as opposed to the level of democratization following the end of the war. Because a democratization process is observed in our data set only after the war ends, there is a legitimate concern about selection effects. We estimated a Heckman selection model, given in Table D-1, to test whether such selection effects have a significant influence on the efficiency of our previous parameter estimates.

We specified two variations of this model to test the

³⁸ On two-stage probit models, we followed Alvarez and Butterfield 2000; Alvarez and Glascow n.d.; Bollen, Guilky, and Mroz 1950; Guilkey, Mroz, and Thomas 1992; Madalla 1983; and Rivers and Vuong 1988. We used the method suggested by Rivers and Vuong to test for exogeneity by performing a t-test on the residual of the first-stage regression, included as an explanatory variable in the structural model. The residual is the fitted value of UN presence (PredUN) minus the actual value of UN involvement (UNINV). This procedure is used with continuous endogenous variables, so we have to assume that some added level of uncertainty is included in the residual in our case.

⁵⁹ All our analysis and do-files testing the potential endogeneity of UN involvement can be accessed at: http://www.worldbank.org/research/conflict/papers peacebuilding. These regressions are not included here since they do not change our previous results.

		Model 1	Model 2	
Dependent Variable and Selection	Explanatory Varlables	(distinguishes between types of PKOs)	(all types of PKOs together)	
BURR2 (Democracy	Constant	8.36**	8.255**	
Index)		(2.09)	(2.05)	
	Multidimensional PKO (nontraditional ops.)	4.00* (1.93)	_	
	UN PKOs (traditional and multidim.)	· ,	3.03 (1.79)	
	Enforcement mission (Chapter VII UN op.)	-3.33 (2.35)	-3.09 (2.47)	
,	Development level (electricity	(Ε.ΟΟ)	Ψ ,	
,	consumption)	.00032	.0002	
•	144	(.0012)	(.001)	
	War duration (in months)	.0147* (.007)	.0176* (.007)	
	Five-year average prewar democracy	.306*	.298*	
	Index	(.125)	(.125)	
election Equation: PBS2L (Lenlent PB)	Multidimensional PKO (nontraditional ops.)	1.66* (.734)		
, ,	UN PKOs (traditional and multidim.)	_	.626 (.404)	
•	Enforcement mission (Chapter VII UN op.)	1.07 (.658)	1.09 (.660)	
	Development level (electricity consumption)	.0002 (.0001)	.00019 (.0002)	
	War duration (measured in months)	.0004 (.002)	.0007 (.0023)	
	War type (Identity war?)	−.9212 ** (.258)	977** (.256)	
	Deaths and displacements (log of dead/displaced)	175** (.0 6 5)	189 ** (.065)	
	Number of factions (major factions)	487 (.372)	531 (.323)	
	Quadratic of number of factions in the war	.012 (.038)	.0243 (.030)	
	Net transfers per capita (current US \$)	.0023* (.001)	.0017 (.001)	
	Was a treaty signed to end the war?	1.01* (.448)	1.107** (.419)	
	Natural resources (primary exports %GDP)	-3.41* (1.76)	-3.64* (1.72)	
	Dummy variable for the Cold War	.794* (.379)	.887* (.364)	
	Athrho:	457 (.47 6)	476 (.442)	
	Lnsigma:	1.63** (.101)	1.651** (.103)	
	Rho:	428 (.38 9)	44 3 (.355)	
	Sigma:	5.12 (.521)	5.21 (.540)	
	Lambda:	-2.19 (2.137)	-2.31 (2.008)	
bservations	Censored/uncensored	51/69	51/69	
	Log-likellhood	-208.7298	-210.8292	
	Goodness of fit	Wald χ^2 (5) = 37.69	Prob > χ^2 (5) = 31	

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significance for democratization of peacekeeping as opposed to enforcement operations. Model 1 distinguishes between traditional and multidimensional PKOs, whereas model 2 combines the two. The estimates in model 1 show that multidimensional PKOs are highly significant and positively correlated with the level of democratization observed after the war ends, and they also help end the violence (see the results in the selection equation). By contrast, UN enforcement operations are significant in ending the violence but are nonsignificant and negatively associated with democratization. In model 2, the variable denoting all types of PKOs combined is not strongly significant, especially for ending the violence but also for democracy. The results with respect to enforcement are comparable to model 1. We control for war duration, which is positive and significant, and for the prewar five-year democracy average (GURRLAG5) in our models. We find that a prewar tradition of democratic institutions is the surest correlate of democratization after a civil war.

Model 1 yields a Wald test of independent equations of $\chi^2(1) = 0.92 \text{ (Pr} > \chi^2 = 0.3368)$ and model 2 yields a $\chi^2(1) =$ 1.16 (Pr $> \chi^2 = 0.2816$). These tests do not allow us to reject the null hypothesis of independent equations, which means that the estimates presented in tables 2 and 3 are more efficient and that we can rely on them for our inferences. The Heckman selection models did not cause us to change our estimation methods, but they amplified our earlier arguments regarding the differential effects of PKOs and enforcement operations, and they helped us disentangle the influence of different types of UN operations on democracy.

We can now argue with greater confidence that, the more stable and democratic the peace, the more we need specialized peace operations with a developed civilian component. Furthermore, enhancing local capacities is more important for more democratic peacebuilding, whereas reducing the depth of hostility is more important for less democratic peacebuilding.

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Cluster-Based Early Warning Indicators for Political Change in the Contemporary Levant

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Event Interaction Survey coded event data generated from Reuters between 1979 and 1998. A new statistical algorithm that uses the correlation between dyadic behaviors at two times identifies clusters of political activity. The transition to a new cluster occurs when a point is closer in distance to subsequent points than to preceding ones. These clusters begin to "stretch" before breaking apart, which serves as an early warning indicator. The clusters correspond well with phases of political behavior identified a priori. A Monte Carlo analysis shows that the clustering and early warning measures are not random; they perform very differently in simulated data sets with similar statistical characteristics. Our study demonstrates that the statistical analysis of newswire reports can yield systematic early warning indicators, and it provides empirical support for the theoretical concept of distinct behavioral phases in political activity.

'n recent years, early warning has emerged as a major concern of national and international agencies that respond to political crises (Boutros-Ghali 1994; Davies and Gurr 1998; Dedring 1994; Esty et al. 1995, 1998; Mizuno 1995; Schmeidl and Adelman 1998) and is a topic of renewed interest in the international relations literature (Gurr and Harff 1994, 1996; Rupesinghe and Kuroda 1992; Schmeidl 1996). The policy community has been inspired by the success of famine forecasting models that have enabled international agencies to reduce fatalities by several orders of magnitude over the past twenty years (Rashid 1998; Whelan 1998). Agencies such as the UN High Commissioner for Refugees and the UN Office for the Coordination of Humanitarian Affairs would like to have comparable models for predicting other catastrophic situations—notably, refugee flows—that require emergency humanitarian response. Academics have contributed to these efforts, as evidenced by the collaboration between operational agencies and university-based researchers in the State Failure Project (Esty et al. 1995, 1998) and the Forum for Early Warning and Emergency Response (http://www.fewer. org); academic scholarship also has been motivated by sobering assessments of the failure of traditional forecasting methods to anticipate the end of the Cold War (Gaddis 1992).

A central focus of event data research has long been the development of tools for the early warning of political change. Much of the initial impetus and, quite critically, early funding for that research came from the U.S. Defense Advanced Research Projects Agency (DARPA). These efforts, including the Early Warning and Monitoring System (EWAMS), were contempora-

neous with the creation of the World Event Interaction Survey (WEIS) and Conflict and Peace Data Bank (COPDAB) event coding frameworks as well as a variety of systematic forecasting techniques (see Andriole and Hopple 1984; Choucri and Robinson 1979; Daly and Andriole 1980; Hopple 1984; Hopple, Andriole, and Freedy 1984; Laurance 1990; Phillips and Rimkunas 1983; Singer and Wallace 1979).

For both institutional and pragmatic reasons, these initial efforts failed to take hold in the policy community (Laurance 1990). By the mid-1980s, interest in and funding for statistical early warning projects had virtually ceased. The policy community continued to spend billions on analytical forecasts by intelligence agencies, but virtually all *political* prediction—as distinct from projecting economic and demographic trends—used traditional rather than statistical techniques.

Because human-coded event data are expensive to generate, the end of policy-oriented event data research meant that new data collection activity declined dramatically, and the academic community was limited to reanalyzing legacy collections that terminated around 1977. The WEIS data set, which was the centerpiece of the DARPA efforts, was maintained through a variety of public and private efforts through the 1980s and 1990s (Tomlinson 1993), but it was not widely available or used. In addition, several small and geographically specific event data sets were created by scholars (e.g., Ashley 1980; van Wyk and Radloff 1993).

Ironically, the emphasis on event-based early warning research ended at a time when two technological changes rendered the approach more feasible. First, the revolution in electronic communications made available a much greater amount of news about political affairs than was used by previous analyses, which usually relied on a small number of elite Western newspapers. By the early 1990s, much of this information was in machine-readable form, initially through such commercial services as NEXIS and Reuters Business Briefing and now through the World Wide Web. Second, the exponential increase in computational power allowed for statistical and coding techniques that were impossible during the earlier period of DARPA research.

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The purpose of this article is to describe several new approaches to early warning that use contemporary technologies and concepts. We begin with a review of the methodological challenges involved in statistical early warning research. We then apply two clustering methods, one based on the distances between points and the other on the changes in the density of the cluster, to an event data set that covers international behavior in the Levant (Egypt, Israel, Jordan, Lebanon, the Palestinians, and Syria) plus the United States and USSR/Russia between April 1979 and December 1998. The Levant has long been regarded as an important example of an international subsystem (Binder 1958; Gause 1999); the relatively large number of actors and varieties of behavior in the region provide for a robust test of any early warning techniques.

Our analyses indicate that events tend to form temporally delineated clusters, and the movement of points in those clusters can be used as an early warning indicator; this finding is consistent with the theories of "crisis phase" that underlie many qualitative approaches to early warning. The clusters we identify generally correspond to demarcations in the time series that we assigned a priori, based on a qualitative assessment of political changes in the region, and differ significantly from clusters found in a set of simulated data with similar statistical characteristics.

STATISTICAL APPROACHES TO EARLY WARNING: A REVIEW

There are some key differences between structural and dynamic models as applied to the early warning problem. We will discuss these and describe the dynamic time-series approach used in our research.

Structural and Dynamic Models

Statistical approaches to early warning can be classified into two broad categories: structural and dynamic.1 The first category includes studies that use events (or more typically, a specific category of events, such as civil or international war) as dependent variables and explain these using a large number of exogenous independent variables. In the domain of domestic instability, this approach is exemplified by the work of Gurr and his associates, most recently in the State Failure Project (SFP; Esty et al. 1995, 1998). Gurr and Harff (1996) and Gurr and Lichbach (1986) describe a number of such research projects. In the field of international instability, the structural approach is illustrated by the work of Bueno de Mesquita and by the Correlates of War project; Gochman and Sabrosky (1990), Midlarsky (2000), and Wayman and Diehl (1994) provide general surveys. Structural approaches have tended to employ multivariate linear regression

models but recently have branched out to other techniques; for example, the SFP uses logistic regression, neural networks, and some elementary time-series methods.

In contrast, dynamic early warning models use event data measures as both the independent and dependent variables. Most projects in the late 1970s employed event-based indicators in dynamic models to predict whether a pair of political actors would become involved in a crisis. For instance, DARPA's EWAMS research evaluated three WEIS-based measures (conflict, tension, and uncertainty) to determine the "alert status" of any dyad. Azar et al. (1977) took a similar approach based on whether behaviors measured with the COPDAB event scale fell outside a range of "normal" interactions for the dyad. More recent efforts have used increasingly advanced econometric timeseries methods that model interval-level indicators of events as an autoregressive time series with disturbances. Goldstein and Freeman (1990) provide a booklength example of this approach; Dixon (1986), Goldstein and Pevehouse (1997), Goldstein et al. (2000), Lebovic (1994), Ward (1982), and Ward and Rajmaira (1992) illustrate the continued development of this type of dynamic model.

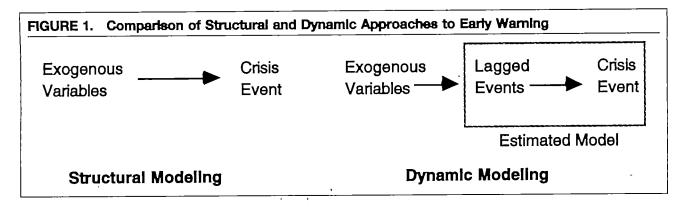
Scholars defend the dynamic approach, which is at odds with most political science statistical modeling in using only lagged endogenous variables, in three ways. According to the first rationale, many of the structural attributes that are theoretically important for determining the likelihood of conflict do not change quickly enough to be used as early warning indicators; in fact, many are virtually static (e.g., ethnic and linguistic heterogeneity, historical frequency of conflict, natural resource base). Data on variables that do change, such as unemployment rates and economic and population growth rates, are often reported only on an annual basis, and the quality of these reports tends to be low in areas under political stress.

The second argument for the dynamic approach is that it allows for greater parsimony in the specification of the forecasting models than a structural approach. The first phase of SFP, for example, collected data on 75 independent variables (Esty et al. 1995, 9), but the final models found that most of the forecasting power resided in only three variables: infant mortality, trade openness, and democracy (Esty et al. 1998, viii). In contrast, the event data collections used in dynamic models allow the researcher to monitor political interactions as reported in public media sources. The focus on events leads to models that contain a relatively small number of independent variables, and the reliance on news sources permits the data to be collected systematically in real time.

Finally, a dynamic modeling approach assumes that the exogenous variables incorporated in the structural models do not need to be explicitly included because their effects will be reflected in the pattern of events preceding a major change in the political system. In

¹ We will not consider the large literature on nonstatistical or qualitative approaches to forecasting, contemporary surveys of which can be found in Davies and Gurr 1998, Rupesinghe and Kuroda 1992, and Schmeidl and Adelman (1998) We also will not deal with long-range forecasting using computer simulation; Ward (1985) and Hughes (1999) summarize that literature.

² Phase II of the SFP involves some limited analysis of dynamic variables and suggests expanding this approach in future studies.



other words, the dynamic approach uses the lagged values of actual events as a substitute for structural variables, as illustrated in Figure 1.

To take a concrete illustration, Gurr (1995, 7) notes that "ethnic heterogeneity probably is most significant for state failure when it coincides with lack of democracy and low regime durability." Consequently, the SFP includes measures for those three variables: ethnolinguistic diversity, regime democracy, and regime durability. A dynamic approach, in contrast, does not measure these aspects of a political system directly; instead, it assumes that each is reflected in the types of events picked up by the international news media. The presence of democracy, for instance, appears not only. in periodic reports of elections but also in a large number of reports discussing disagreements between the government and the elected opposition. A low level of regime durability results in coups and attempted coups. To the extent that ethnicity is an important political factor, there will be ethnically oriented political rallies, outbreaks of violent ethnic conflict, and similar events. A suitably designed event coding scheme should detect the presence or absence of these events and make the appropriate forecast without directly measuring the underlying variables.

At a theoretical level, therefore, dynamic modelers accept the importance of exogenous structural indicators. Ceteris paribus, countries with a high level of ethnic heterogeneity will have a greater propensity for conflict than those with a low level, democracies are likely to be different from autocracies, and so forth. The distinction between the two early warning strategies is one of measurement. Structural modeling seeks to identify and measure the indicators directly, whereas the dynamic approach assumes the effects of structural attributes are indirectly incorporated through the patterns of events they generate.

This is an optimistic but not wholly implausible assumption. For instance, in the Reuters-based domestic data with which we have been working, there is a clear contrast between Israel and Syria with respect to the présence of a democratic opposition. Similarly, ethnoreligious conflict in Lebanon is one of the most conspicuous features of the data set and is in sharp contrast to the relative lack of salience of such conflict in Jordan. Our argument is that an increase in democratization in Syria or a decline in ethnoreligious tensions in Lebanon will be reflected in the political events

reported in Reuters, although we have not analyzed this systematically.

An econometric analogy to the distinction between structural and dynamic political early warning is found in the differences between "technical" and "fundamental" approaches to the analysis of stock prices. A fundamental analysis attempts to predict price changes on the basis of underlying factors, such as marketing, management, raw material prices, and macroeconomic trends. A technical approach assumes that these factors will be reflected in changing stock prices; therefore, analysis of patterns in the movement of those prices will provide sufficient information for forecasting. Fundamental analysis corresponds to the structural approach to modeling political events; technical analysis to the dynamic.³

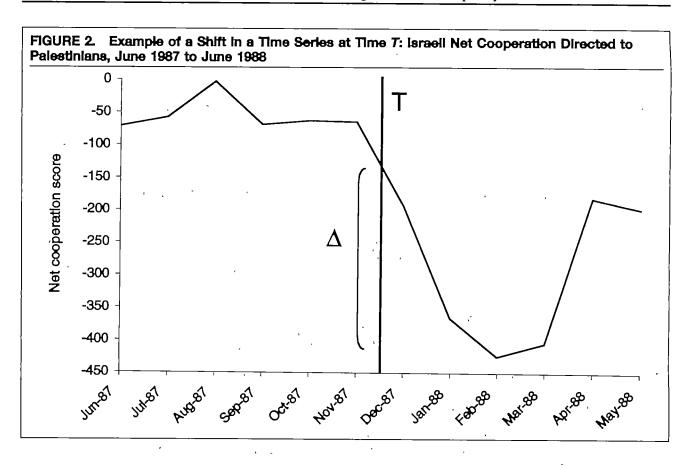
Statistical Characteristics of the Early Warning Problem

The primary goal of time series is to determine the future values of a variable y, given some present and past values of that variable and possibly the present and past values of a set of exogenous variables X. In other words, time-series analysis seeks to determine a function

$$y_{t+k} = f(y_t, y_{t-1}..., X_t, X_{t-1}...)$$

for some k > 0. Due to the importance (and potential financial rewards) of accurate economic forecasts, there is a massive literature on time-series estimation in econometrics (see Hamilton 1994). Standard econometric time-series methods, however, have only limited utility in the problem of early warning, for which the challenge is to identify a time T such that some indicator variable y_t has values consistently different

³ Until relatively recently, technical stock market analysis generally had a bad reputation, due to its use of statistically dubious patterns based on small samples, wishful thinking, and gurus whose fortunes derived more from the sale of books than from trading stock. With the increase in computing power in the 1980s, the situation changed, and "programmed trading systems" can now process sufficiently large amounts of information to generate profits working solely with information endogenous to the market itself. The greater information processing capacity today, in contrast to that available in the 1970s, may have a similar effect on the analysis of political behavior using event data.



(above a predetermined threshold Δ) after time T than before time T:

$$|y_t - y_t'| > \Delta$$
, for all $t > T > t'$.

This is what would occur in aggregated event data following a shift in the type of political behavior in which a dyad is engaged. Figure 2 illustrates this situation for the transition in Israeli-Palestinian interactions that occurred with the outbreak of the Palestinian *intifada* in December 1987.

An additional distinction between early warning research and conventional econometric time-series methods relates to the amount of autoregression in the data. Econometric time series tend to be highly autoregressive: The value at time t is usually close to (or a simple function of) the value at time t-1. Examples of autoregressive series include unemployment figures, prices of consumer goods, and inflation rates. Even series that are potentially less stable, such as stock prices, usually have an autoregressive component combined with generally random noise. For instance, the stock market crash of October 1929 was sudden, whereas the high unemployment rates of the Great Depression required two or three years to develop

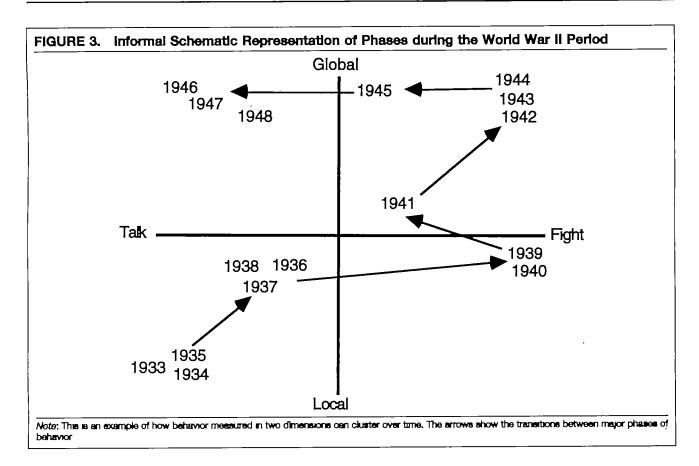
Dynamic early warning models, in contrast, focus on the shifts in a time series that are not autoregressive, even though the series taken as a whole may be autoregressive. An autoregressive model of war and peace will be very accurate, as illustrated by the presumably apocryphal story about the European political analyst who said: "Every day from 1910 to 1970,

I predicted that Europe would remain at peace when at peace, and remain at war when at war, and I was only wrong four times." This type of model is not, however, very useful. It succeeds according to a frequency-based measure but fails according to an entropy-based measure that places higher weight on the prediction of low-probability events (Pierce 1980).

The econometric question most comparable to political early warning is the forecasting of sudden economic shifts, such as those observed in massive exchange rate fluctuations (e.g., the collapse of the Mexican peso or the European Exchange Rate Mechanism). These problems are similar to political early warning in that they are primarily psychological and do not reflect a major change in the underlying physical reality: The economic fundamentals of the Mexican or European economies did not change dramatically during the days of the exchange rate crises, but the perceptions of the future values of the relevant currencies did shift.

Despite these complications, it should be noted that in two very important respects prediction is an easier problem than the typical econometric estimation problem. First, the accuracy of forecasting models can be evaluated probabilistically. Coefficient estimation problems, in contrast, do not have answers: One can always specify an error structure, prior probability, or alternative model that places the estimated emphasis on different variables, and there is no empirical method of deciding among these specifications.

Second, and closely related to the first issue, forecasting problems are not affected by colinearity, which



is the bane of coefficient estimation in the social sciences because every behavior tends to be linked to every other behavior. Coefficient estimates with low standard errors are clearly useful for obtaining a theoretical understanding of a situation, but they are not essential for the pragmatic purposes of forecasting (Wonnacott and Wonnacott 1979, 81). For this reason, it is not surprising that models with very diffuse coefficient structures that do not clearly identify the importance of specific variables—such as neural networks, hidden Markov models, and vector autoregression (VAR)—are found increasingly in early warning research.

CRISIS PHASE AND PREDICTION

The concept of crisis phase is found frequently in the early warning and preventive diplomacy literatures; recent discussions include Alker, Gurr, and Rupesinghe (n.d.), Carnegie Commission for Preventing Deadly Conflict (1997), Gurr and Harff (1994), and Lund (1996). In the empirical literature, crisis phase has been coded explicitly in collections such as the Butterworth international dispute resolution data set (Butterworth 1976), Bloomfield and Moulton's (1989, 1997) Computer-Aided System for Analyzing Conflicts (CASCON; http://web.mit.edu/cascon/), and SHERFACS (http://www.usc.edu/dept/ancntr/Paris-in-LA/Database/Sherfacs.html), developed by Frank Sherman. In a review of these collections, Sherman and Neack (1993, 90) explain:

Conflict is seen "as a sequence of phases." Movement from phase to phase in a conflict occurs as "the factors interact in such a way as to push the conflict ultimately across a series of thresholds toward or away from violence" (Bloomfield and Leiss 1969). Characteristics of disputes can be visualized as the timing and sequencing of movement between and among phases. Processes of escalation of violence, resolution or amelioration of the seriousness (threat of violence-hostilities) and settlement are identifiable through the use of phase structures.

CASCON and SHERFACS, for instance, both code six phases: dispute, conflict, hostilities, post-hostilities conflict, post-hostilities dispute, and settlement. A close analog is found in the DARPA-sponsored work of Phillips and Rimkunas (1983, 181–213), which analyzes WEIS data in a two-dimensional space of threat and uncertainty using Thom's (1975) cusp catastrophe model. Their study successfully locates eight of eighteen crises identified in the WEIS data and produces no false positives.

If the concept of crisis phase is valid, then the international behaviors observed should fall into distinct patterns over time. Figure 3 illustrates this informally for the World War II period, using the two dimensions of talking versus fighting and local versus global involvement. Politics in the years immediately before 1936 were predominantly local and involved little violent interstate conflict. The system shifted to a series of militarized crises between 1936 and 1938, then erupted into a full-scale European war in 1939. After a lull in early 1941, the war spread, first to the USSR and

then to the Pacific; the 1942–44 period was differentiated by a global war. In 1945 this war ended, first in Europe and then in the Pacific, but the postwar politics, rather than return to the unilateralism/isolationism of the prewar period, remained global. The 1946–48 cluster continued to represent the system for most of the Cold War, with occasional departures from that cluster to take in the Korean War, the Suez Crisis, the Cuban Missile Crisis, and similar events.

Figure 3 is obviously idealized. Any analysis using event data will be complicated by the aggregation of dyadic behaviors, the existence of multiple issues determining behaviors, and the fact that real-world political behavior is considerably noisier than the simple summary of international politics in the 1930s and 1940s presented above. Nevertheless, if event data capture the behaviors that characterize a phase typology, then it should be possible to determine those phases using statistical clustering. A cluster occurs whenever the actors in the system respond to each other in a consistent fashion over an extended period (i.e., repeat approximately the same types of actionscooperative, conflictual, or absent-month after month). When the behavior of the dyad changes—for example, from peace to war or vice versa—the system shifts to a new cluster. If the transitions between these phases are gradual, or if interactions that precede a phase shift are distinct from those found when the system is locked in a single phase, then those behavior patterns can be used for early warning of the transition.

A Cluster-Based Approach to Early Warning

To test whether behavioral phase shifts can be used to develop early warning indicators, we analyzed the dyadic behavior of the international actors in the Levant between April 1979 and December 1998. The data were Reuters lead sentences, obtained from the NEXIS and Reuters Business Briefing data services. We coded these leads into WEIS categories using the Kansas Event Data System (KEDS) machine-coding program (Gerner et al. 1994; Schrodt, Davis, and Weddle 1994). We then transformed the individual WEIS-coded events into a monthly net cooperation score for each directed dyad-the actions from a specific source to a specific target—by converting every event to an intensity score, using the numerical scale in Goldstein (1992), and then totaling these numerical values for each of the directed dyads for each month. We examined all the dyads involving interactions among Egypt, Israel, Jordan, Lebanon, the Palestinians, Syria, the United States, and the Soviet Union/ Russia; this gives a total of 56 directed dyads, with 237 monthly totals in each dyad. Appendix A contains additional details on coding and measurement.

TABLE 1. A Priori Phase Assignments						
_ Label	Dates	Months	Defining Characteristic			
Camp Davld	Apr. 1979– May 1982	38	Before Israel's 1982 Invasion of Lebanon			
Lebanon	June 1982- May 1985	36	Israel occupies much of Lebenon			
Taba	June 1985– Nov. 1987	30	Partial Israell withdrawal from Lebanon			
Intifada	Dec. 1987- July 1990	32	Palestinian Intifada			
Kuwalt	Aug. 1990– Oct. 1991	15	Iraq's Invasion of Kuwalt and aftermath			
Madrld	Nov. 1991– Aug. 1993	22	Madrid process: bilateral, multilateral peace talks			
Oslo	Sept. 1993– June 1996	34	Oslo peace process			
Netanyahu	July 1996– Dec. 1998	·30	Netanyahu govern- ment in Israel			

The behavior of this system can be seen by examining the position of the vector

[AB, AC, AD, ..., AH, BA, BC, ..., BH, CA, ..., HF, HG],

where A, B, ..., H are the actors in the system, and each pair of letters represents the total Goldstein-scaled net cooperation for a directed dyad aggregated over one month. The behavior of the system is simply the path that this vector traces over time in a high-dimensional space. In vector terminology, a phase is characterized by a region in the vector space where points cluster over time. If political behavior follows a phase typology, this would be evident by long periods of distinct clusters of behaviors that characterize a phase, with brief transitions between the clusters.

The benchmark for our empirical tests is a set of phases established a priori, based on the dominant political interactions during each period. These phases, shown in Table 1, correspond to the interpretations of "area experts" and are consistent with our own fieldwork in the region. Our discussion will use these a priori clusters as a reference point.

The effectiveness of event-space clustering in early warning depends on whether some measurable characteristic of system behavior changes before the phase transition. In some instances there are few if any precursors to a transition, either because of deliberate concealment by political actors or due to the lack of media interest, as in the examples of Chechnya and Somalia.⁵ Our conjecture is that most political situa-

⁴ We repeated some of our analyses without the USA→USSR and USSR→USA dyads; this change generated only minor differences in the results. (The events involving these dyads include only the USA-USSR interactions reported in the lead sentences of Reuters stories dealing with the Levant, not USA-USSR interactions in general.)

⁵ On the surface, the Rwanda genocide appears to be an ideal case of absent precursors. Yet, in assessing that situation, Ruso (1996) observes that the appropriate facts were available (but incorrectly interpreted) as early as the UN Rapporteur's Report in 1993

tions, however, go through a gradual deterioration (or improvement) over a period of a few weeks or months before a phase transition, rather than manifest a sharp jump. Furthermore, because news-gathering organizations are usually rewarded for correctly anticipating political events, journalists who are present in the region, understand the local politics, and can get their stories accepted by editors and onto the news wires are likely to report the behaviors they perceive to be precursors to any political phase change.

This approach to early warning is similar to the concept of normal relations range proposed by Azar (1972, 184) nearly 30 years ago:

Over a period of time any two nations establish between them an interaction range which they perceive as "normal." This normal relations range (NRR) is an interaction range ... which tends to incorporate most of the signals exchanged between that pair and is bound[ed] by two critical thresholds—an upper threshold and a lower threshold. The upper critical threshold is that level of hostility above which signals exhibited by either member of the interacting dyad are regarded as unacceptable to the other. Interaction above the present upper critical threshold ... for more than a very short time implies that a crisis situation has set in.

The NRR model implies that interactions will cluster, with the diameter of the cluster a function of the upper critical threshold. Consistent with Azar's treatment, we expect that when the system is near the edge of a behavioral cluster, it is in a crisis situation. That crisis will either result in a shift to a new cluster—a phase transition-or be resolved without a transition, in which case the system's interactions will return to the core of the cluster. Unlike Azar, we assume the system is moving away from normal behavior when it consistently nears (or passes) the edge of a behavioral cluster, rather than when it exceeds a single critical threshold. We have generalized Azar's NRR concept by looking at changes in a large number of dyads simultaneously (whereas Azar looked only at one dyad at a time), and we use a standardized metric based on correlation, whereas Azar used a Euclidean metric and established distinct critical ranges for each dyad. Finally, we also look at the density of clusters, defined as the average distance between the points in a cluster, over time. Behavior within the NRR should result in dense clusters, but when a system moves away from one phase/ cluster/NRR and into another, there will generally be a period when the points do not cluster densely.

Detection of Phase Using Clustering over Time

Most widely used clustering algorithms are cross-sectional and do not incorporate the time-series element

(Degni-Ségui 1994). Ruso (1996, 8) concludes. "The authors answer Yes to the question, Did those with the capacity to prevent and mingate the genocide have the information from which such a conclusion might be drawn? In fact, they note that specific information about plans and conspiracles towards this end was picked up by the UN system, most significantly in the notorious Black File of January 1994."

of event data (see Aldenderfer and Blashfield 1984; Bailey 1994; Everitt 1980). A cross-sectional method will not necessarily produce credible time-series clusters. Because the Levantine subsystem as we have defined it does not include all relevant interactions, such as the end of the Cold War, points that are distant in time may still bear a superficial resemblance.

Including time as the dominant dimension simplifies the delineation of clusters. The algorithm we employ in this study is uncomplicated: A new cluster is established at a point x_i , when the distance between x_i and the points before it is greater than the distance between x_i and the points after it. This can be expressed in the equation

$$LML_{t} = \frac{1}{k} \sum_{t=1}^{k} \|x_{t} - x_{t-k}\| - \frac{1}{k} \sum_{t=1}^{k} \|x_{t} - x_{t+k}\| > \Delta.$$

LML is the lagged distance minus leading distance; x_t is the Goldstein-scaled net cooperation score for month t;

$$||x_t - x_{t\pm k}||$$

is the distance between x_t and $x_{t\pm k}$ according to some metric; and Δ is a threshold parameter that prevents new clusters from being formed because of random fluctuations in the event data that are unrelated to phase transitions.6 (A large negative value of LML, means that the point is still firmly in the current cluster, that is, much closer to points in the past than to points in the future. When the LML, value is strongly positive, it indicates the point is far away from past points and close to future points.) From the perspective of cluster analysis, this approach is similar to the minimum spanning tree approach (see Backer 1995, chap. 1) in dividing the clusters at places where a large gap is found between adjacent points; it differs in using the dimension of time rather than a graph to determine which points are adjacent.

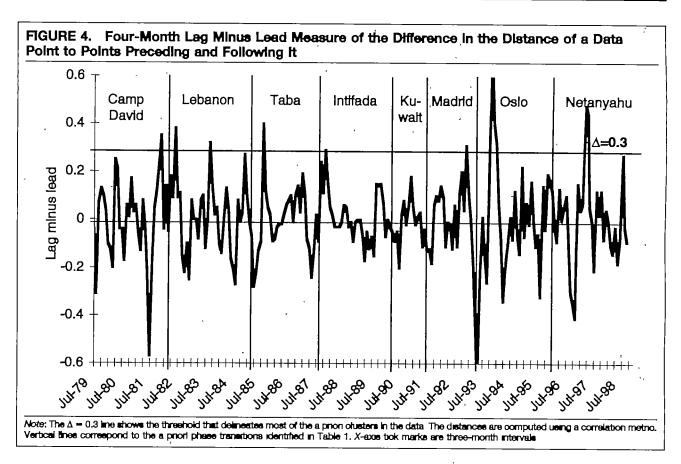
In our analysis, k is measured in months, although in principle it could represent some other interval, such as weeks or even days. The threshold Δ is a free parameter whose value depends on the level of differentiation the analyst wants between the clusters (and the characteristics of the data being used to measure the political activity). For example, if Δ were set to a very high value, such as 0.8, then we would see only two clusters in the data: pre-Oslo and post-Oslo. If it were set to a low value, such as 0.15, then we would find several additional subclusters within the a priori clusters specified in Table 1.

Figure 4 shows the results of analyzing the Levant data set using this algorithm for k=4 and the correlation metric

$$||x_t-y_t||=1-r_{xy},$$

where r is the Pearson product moment. (In other words, this metric is based on the correlation between

⁶ These calculations were done with a simple (600-line) Pascal program that produced various tab-delimited files that were read into Excel to produce the figures.



the Goldstein-scaled scores of the 56 dyads at two times.) The vertical lines on the graph correspond to time points where the a priori cluster divisions from Table 1 are located.

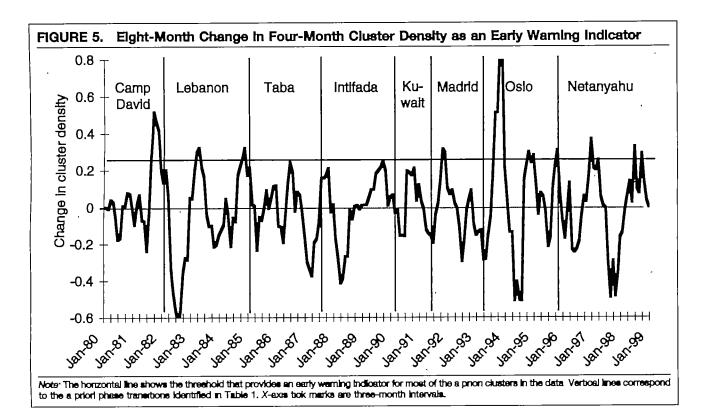
A cluster division occurs when $LML_i > \Delta$. With the threshold level set at $\Delta = 0.30$, the LML measure, by increasing sharply at or very close to the demarcation lines, correctly identifies four of the seven phases determined a priori: Lebanon, Taba, intifada, and Oslo. Several other plausible transitions are suggested by peaks that do not correspond to the original phases: (1) a pre-Lebanon change, probably reflecting increased tension between Israel and the PLO in 1981; (2) two pre-Taba changes that may correspond to shifts in Israeli, Syrian, and U.S. policy in Lebanon (including extensive diplomatic activity related to the May 17, 1983, agreement and its subsequent abrogation); and (3) a transition in January 1993 that may reflect the change in U.S. policy toward the Middle East that occurred with the shift from the Bush to Clinton administrations. Finally, the analysis identifies March 1997 as the end of the Oslo period. This is reasonable. Although the Netanyahu government came to power in the June-July 1996 period, it was nominally committed to continuing the Oslo peace process initially, and the observed nine-month delay before the end of that process, while unanticipated, is in retrospect unremarkable.

Our LML measure misses two previously chosen transitions: the Kuwait war and the Madrid phase. Including the Kuwait transition as an a priori phase was probably an error. Although Iraq's invasion of the

emirate and the subsequent war had profound implications for the Levant, the initial interactions that characterize this period occurred among actors outside the group we are studying, so it is not surprising that the data do not reflect it. It is less obvious why we fail to identify the Madrid transition. This failure might also be a side-effect of the Kuwait invasion. Much behavior during late 1990 and 1991 was due to circumstances outside the Levant, so the very real shift in politics that accompanied the Madrid process may have been less apparent in the data than were the other transitions.

Change in Cluster Density as an Early Warning Indicator

The LML measure cannot be used for early warning. It requires information from both before and after a phase transition, so it can only be used to delineate clusters of behavior retrospectively. Consequently, in order to develop an early warning indicator, some alternative measure is required. An examination of Figure 4 shows that the LML measure often begins a rapid increase several months before a phase transition. This is consistent with the underlying theory: The changing interactions in the system cause the points to pull away from the cluster center before a final break, rather like what happens when pulling on a piece of taffy. This pattern suggests that a decrease in cluster density—the extent to which the points within the cluster are close—may serve as an early warning. Since a change in cluster density can be determined solely on



the basis of information available up to and including time t, it can be identified prospectively.

Figure 5 shows the behavior of the cluster density change (CDC) measure. The measure is calculated by first computing the average total distance between the points in a cluster of four consecutive months,

$$CD_{t} = \frac{1}{6} \sum_{i=0}^{3} \sum_{j=i+1}^{3} ||x_{t-i} - x_{t-j}||,$$

and then calculating the difference between CD_t at points that are eight months apart:⁷

$$CDC_t = CD_t - CD_{t-8}$$

The CDC measure generally corresponds well with both the a priori phases and LML-identified transitions, despite the fact that the LML clusters were based on post-hoc information. At each point where the CDC exceeds a threshold set at one standard deviation (0.23), there is a corresponding LML cluster transition within 1–10 months of the point, as seen in Table 2. Unlike the LML measure, the CDC analysis does identify the Madrid transition—it exceeds the 0.23 level in November 1991 ($x_t = 0.24$) and December

1991 ($x_t = 0.27$)—but still fails to show the Kuwait transition. A peak in the CDC measure in October 1989 ($x_t = 0.24$) probably corresponds to the decline of reports of activity in the Palestinian intifada (Gerner and Schrodt 1998); the peaks in early 1995 and 1996 may reflect changes associated with the problems encountered in the Oslo peace process before the cluster break identified in March 1997.

The CDC measure is continuous and can be interpreted as proportional to the probability that a major change will occur; most of the event data models developed in the 1970s provided only a yes/no prediction of change. From the perspective of early warning analysis, the disadvantage of CDC is that it indicates only that some sort of systemic shift is pending; it tells nothing about what form that change will take. Furthermore, the phases determined by CDC do not necessarily correspond to the overt military-political changes that one might wish to forecast with an early warning system.

This is most conspicuously the case for Lebanon in 1981–82. According to the CDC measure, the system shifted into the Lebanon phase about a year before the Israeli invasion in June 1982. At the time of the actual invasion, the CDC measure is at one of the lowest points seen in the entire time series. On the one hand, the Israeli policies that culminated in the invasion were put into effect long before, so placing the true phase shift in mid-1981 is politically plausible. It is widely accepted that Israel planned the invasion of Lebanon as much as a year in advance, and then engaged in provocative military maneuvers throughout the first half of 1982 in an effort to good the Palestinians into a

⁷ There are six possible comparisons within a cluster of four months: x_i to x_{i-1} , x_i to x_{i-2} , through x_{i-2} to x_{i-3} . Therefore, the divisor six is used to compute the average. The lag of eight months was established empirically. In other regions of the world it may be shorter or longer, depending on the time required for political decisions to be made. The choice of eight months is not critical. Because the CDC_i calculation involves data from two periods of four consecutive months, values of CDC_i at lags one or two months shorter or longer than the eight-month lag employed here will be strongly correlated with the eight-month value.

initial Date of Cluster	Political Characteristics	A Priori	LML Cluster	Nearest
		Cluster?	$\Delta > .30$	CDC Peak
July 1979	Camp David; pre-Lebanon	y es	NA	NA
December 1981	Increase in Israeli activity against PLO in Lebanon before the June 1982 invasion	no	yes	Oct. 1981
June 1982	Israell invasion of Lebanon	yes	yes	Oct. 1981
September 1983	Partial Israell withdrawal from Lebanon; increased conflict between Lebanese and International forces	no .	уев	Apr. 1983
August 1985	lsrael withdraws south of Litani River, Taba negotiations	yes ^a	yes	Apr. 1985
December 1987	Palestinian intifada begins	yes	yes	Sept. 1987
August 1990	Invasion of Kuwalt	yes	no	Oct. 1989 ^b
December 1992	Madrid peace process	['] yes⁰	yes	Dec. 1991
November 1993	Oslo peace process	уев	yes ·	Oct. 1993
March 1997	Post-Oslo period	yes ^d	yes	Dec. 1996

The a priori ofuster break was two months earlier, in June 1985

"The a priori cluster break was almost a year earlier, in November 1991.

response that would justify armed intervention (Gerner 1994; Hiro 1992; Jansen 1982; Morris 1999, chap. 11; Petran 1987; Randel 1983; Reilly 1983). On the other hand, the situation on the ground looked very different in July 1982 than in April 1982, a period during which the CDC measure plummeted. This is clearly not a "barometric" early warning that allows a political analyst to say: "The CDC indicators are real low this month, nothing to worry about." This may be because CDC is based on a correlation distance and is sensitive to changes in policy configurations—who is coordinating policy with whom—rather than to the direction of change. Using CDC as an early warning in combination with a Euclidean measure sensitive to the direction of change might provide both types of information.

Our research generally supports the crisis phase model, but the measures are somewhat ad hoc. How do we know our findings are not simply a statistical artifact, reflecting a combination of chance and self-deception? In Appendix B we compare our results to those obtained with a randomly generated simulated data set that has the same mean, variance, and auto-correlation as the observed data. The Monte Carlo analysis of the LML and CDC measures found that these two indicators behave quite differently in the randomly generated simulated data sets and in the observed data. This suggests our results are due to actual political characteristics in the region and are not simply an artifact of the clustering methods.

SUMMARY OF FINDINGS

Table 2 summarizes the empirically determined clusters in Levantine political behavior for the period studied. For the most part, these divisions correspond to the a priori clusters; when they do not, the differences are plausible. The LML cluster analysis identifies two phases that were not part of the a priori set. The first is an increase in tension between Israel and the Palestinians before the Lebanon invasion; the second is a postinvasion, pre-Taba period that corresponds to continuing instability and the Israeli withdrawal from the area around Beirut. The end of the Oslo phase occurs about nine months into the Netanyahu administration; we did not anticipate this particular date, but it is quite plausible.8 The CDC measure—although not the LML cluster analysis—indicates significant changes following the Oslo phase. Based on CDC, we might also have designated a postintifada, pre-Madrid cluster beginning in late 1989. All our analyses missed the Kuwait transition.

The CDC score probably corresponds to the end of the intifade and the Synan consolidation of power in Lebanon, rather than a forecast of the Kuwart invasion.

⁴A break at some point during the Netanyahu government period was anticipated, the exact timing was not

⁸ This cluster break occurs near the splice between the NEXIS and Reuters Business Briefing (RBB) sources: The "leading" cluster for March 1997 contains about ten weeks of NEXIS data and about six weeks of RBB. It is possible that the change in data source affects the delineation of the cluster, but this seems unlikely for three reasons. First, this cluster shift is consistent with the changing policies of the Netanyahu administration. Second, the cluster break is anticipated by four months in the CDC measurements that contain no RBB data. Finally, most of the LML scores across the splice are quite low: Many are close to zero, and only the March and April 1997 calculations show evidence of a shift in behavior.

The CDC measure usually provides two to six months of warning, but it gave no signal of the Oslo transition and no distinct alert before the June 1982 invasion of Lebanon. The CDC measure also has some false positives where it peaks just below the critical level. This is to be expected: Any measure that does not contain false positives is probably insufficiently sensitive to political events. For example, the several CDC peaks associated with the Oslo and Netanyahu periods are consistent with the fits and starts of that negotiation. More generally, we have not included in this model interactions among domestic actors—such as the various political parties and factions within Israel and Lebanon, and sometimes those interactions can be important. We are not dealing with a deterministic system, and at times a false positive may reflect precursors to transitions that failed to occur because of a reaction in the international system or in domestic subsystems that prevented the phase change.

The pre-Lebanon peak in LML may be an example. In 1981, allies of Israel may have persuaded Prime Minister Menachem Begin that an invasion of Lebanon would result in eventual Syrian hegemony there, the development of a militant Islamic fundamentalist movement on Israel's northern border, and the end of Begin's political career. Only after another year had passed did the contrary and disastrous design of Defense Minister Ariel Sharon prevail.

CONCLUSION

With the end of the DARPA-sponsored research, the development of quantitative early warning models went into eclipse. When evaluated against what is possible today, the DARPA efforts were necessarily primitive in their dependence on human coding and mainframe computers with only a tiny fraction of the speed and memory now available. The event-based quantitative forecasting efforts of the late 1970s were unsuccessful, but the contemporary situation is quite different. The quantity, consistency, and timeliness of event data have improved substantially due to the advent of machine-coded data sets based on news wire sources. With increased computer power and greater sophistication of statistical research in international relations, a variety of new early warning techniques can now be applied to that data.

We draw two general conclusions from our analysis of time-delimited clusters. First, the empirical results support both the theoretical concept of crisis phases and the strategy of analyzing the movement of a point defined by the vector of dyadic interactions in an international system. The pattern of variation in LML_t seen in Figure 4 is exactly what we expected the model to generate: brief periods of large movement followed by long periods of little movement.

These time-delineated clusters are much cleaner and more consistent than clusters determined by cross-sectional techniques, such as the K-Means analysis found in Schrodt and Gerner (1997). The $LML_t > \Delta$ method we used to delineate the clusters is conceptually straightforward and computationally efficient; in

fact, the algorithm is sufficiently simple that it may be possible to determine analytically some of its statistical properties. Nonetheless, it locates most of the clusters we expected to find.

The CDC measure also appears promising as the basis of an early-warning indicator. It provides a two-to six-month warning for most of the changes in the data set, and its behavior is consistent with the theoretical predictions of the crisis phase approach. Both the LML and the CDC measures act quite differently in real-world and simulated data, which suggests that they reflect underlying political behavior rather than statistical artifacts.

Time-delimited clusters are a dynamic rather than a structural early warning approach, but their effectiveness should not be regarded as evidence against the structural approach; we regard these two research strategies as complementary rather than competitive. Structural methods are particularly good at telling analysts where to look for potential trouble, and they are apt to provide theoretical guidance about why a specific system is likely to experience problems. This, in turn, may provide insights into the types of action to take to resolve an impending crisis. Structural models are unlikely to excel at predicting the exact timing of breakdowns, however, because the variables they identify as theoretically important change much too slowly. This is where dynamic models come into play.

Our analysis does not consider an alternative class of dynamic models, those based on event sequences, rules, patterns, and precedents (see Cimbala 1987; Hudson 1991). These models generally provide more contextual information than is supplied by numerical time-series methods. As a consequence, they, too, may be useful in identifying the immediate events leading to a crisis. For instance, although the Kuwait transition is invisible in our cluster analysis, the events preceding the Iraqi invasion follow very closely Lebow's (1981) "justification of hostility" crisis type. Recognizing such patterns could be useful for very short-term forecasting. Also, an assortment of computationally intensive nonlinear forecasting techniques have been developed in recent years (e.g., Casdagli and Eubank 1992; Richards 2000), although relatively little attention has been paid to these in the quantitative international political literature. In short, a variety of unexplored methods may be applicable to the early warning problem.

We believe the ideal early warning model should combine elements of both the structural and dynamic approaches. The optimal model might vary depending on the structural precursors in a specific case. Presumably, an internal breakdown in Lebanon, which is relatively wealthy and highly differentiated by religion, does not occur in the same fashion as a breakdown in Rwanda, which is relatively poor and not differentiated by religion. The literature on domestic conflict and state breakdowns (e.g., Esty et al. 1995, 1998; Gurr and Lichbach 1986) could provide theoretical guidance on this issue.

The reason integrated models have not emerged is due largely to resources: The political science discipline is still developing accurate structural and dynamic models, and no researcher has been able to assemble data sets sufficiently large to study structural and dynamic dimensions simultaneously. As the investigation of both types of models identifies more focused sets of variables and techniques, it should be practical to combine the approaches.

APPENDIX A: DATA SOURCES AND MEASUREMENT

Our source for April 15, 1979, to June 10, 1997, was the NEXIS "REUNA" file; Reuters Business Briefing (RBB) was used for June 11, 1997, to December 31, 1998. The change of sources was required because Reuters stopped supplying data to NEXIS on June 10, 1997. The two data services provide a somewhat different mix of stories, but we see no evidence of a significant discontinuity when the stories are coded and aggregated at a monthly level.

The following search command was used to identify relevant stories in NEXIS:

(ISRAEL! OR PLO OR PALEST! OR LEBAN! OR JORDAN! OR SYRIA! OR EGYPT!) AND NOT (SOCCER! OR SPORT! OR OLYMPIC! OR TENNIS OR BASKETBALL)

To locate stories in RBB, we used the RBB search software (version 2.0 for Macintosh) to select "Political" and "General" news stories that dealt with Egypt, Israel, Jordan, Lebanon, and Syria; the "Reuters Sports" source was explicitly excluded. (The "Israel" category includes stories on the Palestine National Authority as well as Israel.) Some additional filtering was done on both the NEXIS and RBB downloads to eliminate Reuters "Highlights," historical calendars, and other irrelevant material.

The KEDS machine coding program does some simple linguistic parsing of the news reports—for instance, it identifies the political actors, recognizes compound nouns and compound verb phrases, and determines the references of pronouns—and then employs a large set of verb patterns to determine the appropriate event code. Details on the mechanics of the KEDS system are found in Gerner et al. (1994) and Schrodt, Davis, and Weddle (1994). For example, consider the following lead sentence: "Palestinian President Yasser Arafat accused Israeli Prime Minister Benjamin Netanyahu on Tuesday of intentionally prolonging their peacemaking crisis."

The key components of the sentence are
Subject: Palestinian President Yasser Arafat
Verb: accused

Object: Israeli Prime Minister Benjamin Netanyahu Based on the dictionaries used by the KEDS program, the proper noun in the subject—"Arafat" (or the adjective "Palestinian")—is assigned the actor code PAL and becomes the source of the event. The proper noun in the direct object—"Netanyahu" (or the adjective "Israeli")—is assigned the actor code ISR and becomes the target. The event is determined by the verb ("accused") and is assigned a value (121 in this case) corresponding to the appropriate code in the WEIS event coding system. Bond et al. (1997), Huxtable and Pevehouse (1996), and Schrodt and Gerner (1994) discuss extensively the reliability and validity of event data generated using Reuters and KEDS.

We coded only the lead sentences of the stories, which produced a total of 92,687 events. The search command, however, generated a number of events outside the 56 directed dyads considered in this study. Those 56 dyads produced 43,328 events.

The data and the source code for the computer programs used in the analysis, as well as the KEDS program (version 0.9B6.2) and the dictionaries used for this coding session, are available from the web site http://www.ukans.edu/~keds (accessed August 15, 2000).

APPENDIX B: COMPARISON WITH A NULL MODEL

Here we develop a null model and look at the distribution of various indicators in simulated data generated by that model. We wish to determine whether a Monte Carlo analysis using randomly generated data with the same means, variances, and autocorrelations as our Levant dyads will show the same patterns of change in distance that we found in the actual data. If this occurs, it means our results are simply an artifact of the analytical method, rather than a reflection of the underlying politics.

Our null model duplicates the sample size (192) and number of dyads (54) found in an earlier version of the data set, as well as the mean, variance, and first-order autocorrelation of the data within each dyad. Specifically, we generated simulated data using a first-order autoregressive (AR[1]) process,

$$y_t = c + \phi y_{t-1} + \varepsilon,$$

where $c = \mu (1 - \rho)$; $\phi = \rho$; $E(\varepsilon) = 0$; $Var(\varepsilon) = s^2 (1 - \rho^2)$. As Hamilton (1994, 53-4) notes, this will produce a time series with mean μ , variance s^2 , and first-order autocorrelation ρ . In order to avoid initial value effects, the simulated data were taken from the interval $[y_{51}, y_{242}]$, with $y_0 = \mu$. To save computation time, ε were generated by random selection from a table of 5,000 normally distributed random variables produced by Excel 4.0. We created a sample of 1,000 such data sets.

This specification represents a compromise between a null model that is excessively random and one that duplicates all the features of the original data set. On the one hand, in a null model using white noise (no autocorrelation), points generated by the dyads will jump around in the vector space far more than one would ever expect to see in event data based on actual political behavior and presumably will show only very small clusters. On the other hand, if we also duplicate the cross-correlation between dyads, the simulated data set will have most of the statistical characteristics of the actual data, and it would not be surprising to find similar results. Our choice is an intermediate model in which the simulated time series have the same general characteristics within each dyad but have no relationship between dyads. (Autocorrelation above the first order is significant in only a small number of the dyads in the original data.)

In comparing the simulated data with the actual data, we looked at the following measures:

- the total number of points at which $LML_t > \Delta$, with $\Delta = 0.2$, the threshold that best delineated clusters in this 1979-96 data set;
- the number of $LML_t > \Delta$ points that signal a new cluster. This is defined (somewhat arbitrarily) as an $LML_t > \Delta$ point that had no $LML_t > \Delta$ points in the previous two periods. These times are called "cluster-defining points";

⁹ This analysis was done in April and May 1996. Because the simulations are quite time consuming, we have not rerun them with the data set that goes through December 1998. There is no reason to believe that the results would be any different using the newer data. ¹⁰ In other words, this definition ignores the strings of consecutive $LML_t > \Delta$ points that are generated by rapid movements away from

	TABLE B-1. Statistics Computed from 1,000 Simulated Data Sets, $\Delta = 0.2$						
Statistics for $\Delta = 0.2$ ($N = 1,000$)	Simulated Mean	Simulated Standard Dev.	Observed Value	One- Talled Proportion			
Total $LML_t > \Delta$	31.55	5.67	15	0.003 (<)			
Ctuster-defining $LML_t > \Delta$	15.63	2.61	9	0.006 (<)			
St. dev. of CDC	0.30	0.04	0.23	0.026 (<)			
St. dev. of LML	0.25	0.03	0.15	0.001 (<)			
Cluster break at t and CDC _{t-k} > St. dev.							
k = 0	0.41	0.11	0.56	0.090 (>)			
k = 1	0.22	0.10	0.22	0.481 (>)			
k = 2	0.21	0.09	0.11	0 893 (>)			
k = 3	0.20	0.09	0 11	0 869 (>)			
$LML_t > \Delta$ within $t \pm k$ of a priori							
k = 0	0.03	0.03	0.07	0.136 (>)			
k = 1	0.10	0.06	0.27	0.011 (>)			
k = 2	0.17	0.08	0.40	0.006 (>)			
k = 3	0.23	0.09	0.47	0.008 (>)			

- the standard deviation of *LML*, and the early warning measure, CDC; the means of both measures are zero;
- the number of CDC measures greater than one standard deviation above zero at 0, 1, 2, and 3 months prior to a cluster-defining point; and
- the number of $LML_t > \Delta$ points within 0, 1, 2, and 3 months of the six a priori cluster transitions we identified in our data set, as a proportion of the total number of $LML_t > \Delta$ points.¹¹

Because the CDC measure can only be computed after twelve months of data are available, and computing the LML_t requires three additional months, the interval on which these measures were computed contains 192 - 11 - 3 = 178 time points. (The total number of months in a sixteen-year period is 192. We eliminate the first eleven months where the CDC measure cannot be computed and the final three months where the LML measure cannot be computed, leaving 178 months.)

The results of the Monte Carlo analysis are presented in Table B-1, where the "one-tailed proportion" indicates the proportion of the values in the simulated data that are less than (<) or greater than (>) the observed value. The distribution of the values of the statistics are generally smooth, symmetrical, and more or less normally distributed;

an existing cluster; these are quite common in the simulated data and are seen in the actual data in the Lebanon transition. This measure should also be less sensitive to the level of Δ .

TABLE B-2 Statistics Computed from 1,000 Simulated Data Sets, $\Delta = 0.35$ Simulated One-Statistics for $\Delta = 0.35$ Simulated Standard Observed Tailed (N = 1,000)Proportion Mean Dev. Value Total $LML_{i} > \Delta$ 13.58 4.34 15 0.680 (>)Cluster-defining $LML_t > \Delta$ 8.48 2.49 Я 0.680 (<)St. dev. of CDC 0.30 0.04 0.23 0.026 (<)St. dev. of LML 0.25 0.03 0.001 (<) 0.15 Cluster break at t and CDC_{t-k} > St. dev. 0.54 0.56 0.482 (>) k = 00.17 0.22 0.731 (>) 0.31 0.16 k = 1k = 20.30 0.16 0.11 0.915 (>) 0.28 0.15 0.11 0.903 (>) k = 3 $LML_t > \Delta$ within $t \pm k$ of a priorl break 0.030.06 0.07 0.247 (>) k = 00.27 0.074 (>)k = 10.10 0.10 k = 20.16 0.13 0.40 0.054(>)0.080(>)k = 30.23 0.14 0.47

the probabilities are based on the actual distributions of the statistics in the simulated data rather than on a normal approximation.

With the exception of one set of statistics—the relationship between CDC and the cluster-defining points—the values observed in the actual data are substantially different from those found in the simulated data and vary in the expected direction. The number of $LML_t > \Delta$ points found in the actual data, whether total or cluster defining, is about half that found in the simulated data. The standard deviations of the LML and CDC measures are substantially less in the observed data than in the simulated data. Generally, an $LML_t > \Delta$ point is about twice as likely to occur near one of the a priori cluster breaks in the actual data than in the simulated data.

The relationship between CDC and the cluster-defining points is somewhat puzzling. The observed k=0 point is significantly greater (at the 0.1 level) than the simulated values, as we expected. The k=1 value, however, is simply equal to the mean, and the k=2 and k=3 values are significantly less than the simulated data at the 0.15 level. This suggests that, on average, CDC_{t-k} may be a better early warning indicator than demonstrated in this data set, although its performance is due to autocorrelation in the data rather than to any more complex political characteristics that involve dyadic interactions.

The large number of $LML_t > \Delta$ points combined with standard deviations of LML and CDC that are higher in the simulated data than in the observed data suggests that the value of Δ —a free parameter that was established arbitrarily—may have been set too low for the simulated data. We reran the simulated data sets with $\Delta = 0.35$, a level of Δ that gives roughly the same number of cluster-defining points in the simulated data as were found in the observed data with

¹¹ In the simulated data, these a priori transitions are arbitrary because they were determined by changes in the actual, rather than the simulated, data. Nonetheless, these indicators measure the likelihood of finding $LML_t > \Delta$ points in the vicinity of a set of six transition points spaced at the intervals in the a priori set. We look at the proportion because the number of $LML_t > \Delta$ points in the simulated data is substantially higher than in the actual data.

 $\Delta=0.2$. This adjustment of Δ effectively eliminated one additional degree of freedom in the simulated data; the results of this analysis are reported in Table B-2.

This modification changes the one-tailed probabilities somewhat but in general does not alter the conclusions of the analysis. The curious pattern of CDC and the cluster-defining points is retained—and actually strengthened at k=2 and k=3—except that the k=0 point is no longer significant. The relationship between the $LML_t > \Delta$ measures and the a priori breaks is slightly less strong, but the k>0 probabilities are still quite low. We conclude that the behavior of the predictive measures is not solely due to the difference in the number of $LML_t > \Delta$ points.

The results of the Monte Carlo analysis are affected by the existence of the free parameter Δ , but in no circumstances do the results from the analysis of random data closely resemble those found in the real data. If we assume the $\Delta = 0.2$ separation threshold, then the observed data have far fewer clusters than we would expect the null model to generate. By raising the level of Δ , we can match the number of empirically determined clusters, but the behavior of the CDC statistic and the coincidence of $LML > \Delta$ points and the a priori points are still quite different in the simulated data. Furthermore, the necessity of raising the value of Δ to match the expected number of clusters means that the number of points at which a large change occurs in LML, is greater in the simulated data than in the observed data because the variance of LML is higher in the simulated data. This in turn would be expected if the observed data actually settle into clusters and remain there for a time—as predicted by crisis phase theories—rather than jump around. We suspect that the standard deviation of LML, is lower in the observed data because of cross-correlation (and in a few dyads, higherorder autocorrelation) of the dyads.

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International Law and State Behavior: Commitment and Compliance in International Monetary Affairs

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International law have on state behavior? Very little empirical research tries to answer these questions in a systematic way. This article examines patterns of commitment to and compliance with international monetary law. I consider the signal governments try to send by committing themselves through international legal commitments, and I argue that reputational concerns explain patterns of compliance. One of the most important findings is that governments commit to and comply with legal obligations if other countries in their region do so. Competitive market forces, rather than overt policy pressure from the International Monetary Fund, are the most likely "enforcement" mechanism. Legal commitment has an extremely positive effect on governments that have recently removed restrictive policies, which indicates a desire to reestablish a reputation for compliance.

International relations has long been concerned with the role of rules in the organization of international political life. Why do governments commit themselves to international rules, and under what conditions do they comply with their commitments? This is a pioneering area of research for international relations and legal scholars alike (Burley 1993; Chayes and Chayes 1995; Downs, Rocke, and Barsoom 1996; Weiss and Jacobson 1998; Young 1979). What is at stake is whether and to what extent sovereign behavior can be influenced by a legal commitment to rules of conduct. This question, in turn, is at the root of major disagreements between realist and institutionalist theorists in international relations (see Keohane and Martin 1995; Mearsheimer 1994–95).

The literature on international rule compliance is quite disparate in methods, theoretical orientation, and findings. Early quantitative work suggested that much international behavior is consistent with international law, even in the conduct of hostilities between states (Kegley and Raymond 1981; Tillema and Van Wingen 1982). It has been far more difficult to show a convincing causal link between legal commitments and behavior, however. Some progress has been made in the examination of compliance with environmental agreements; in-depth case studies have demonstrated that technical "capacity" is a necessary although not sufficient condition to secure compliance with agreements that require technical policy implementation. Weiss and Jacobson (1998) compare the performance of nine countries with respect to five environmental accords and conclude, among other things, that administrative and technical capacities (including knowledge and training, adequate authority and financial resources,

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and access to relevant information) are crucial to compliance (see also Haas, Keohane, and Levy 1993). These arguments are less telling, however, where governments are obligated to refrain from particular activities rather than implement complex technical accords.

Others argue that the domestic regime type is essential to understanding international law compliance (Slaughter 1995). Governments based on the rule of law and, especially, the independence of the judicial branch are, in this view, much more likely to comply with international obligations than those that are not. Liberal democracies are likely to commit to rules that reflect their ideological biases and to comply with them. This proposition has not been put to a rigorous test, but it seems to dovetail with the strand of the democratic peace literature that argues regard for domestic constitutional constraints carries over into the conduct of foreign policy (Dixon 1993; Risse-Kappen 1995).

The most serious challenge to any of these studies is that they are not able to show credibly that compliance is based on anything other than immediate state or governmental interests. This leaves most studies on compliance subject to the criticism that rules add little to our understanding of international relations: They reflect rather than alter governments' interest in pursuing a particular course of action (Downs and Rocke 1995; Mearsheimer 1994–95).

My argument is that international legal rules do alter governments' interests in compliant behavior. International legal commitment is a bid to make a credible commitment to a particular policy stance. The acceptance of treaty obligations raises expectations about behavior that, once made, are reputationally costly for governments to violate. An international legal commitment is one way that governments seek to raise the reputational costs of reneging, with important consequences for state behavior. I demonstrate this effect in the context of the public international law of money, an area in which governments traditionally have guarded jealously their sovereign decision-making status (Cohen 1998). In 1945, for the first time in history a treaty regime was established governing money and the inter-

national payments system, to be administered by the International Monetary Fund (IMF). This agreement required adherents to keep their current account free from restriction. Put most simply, if a bill comes due for imports or for an external interest payment, national monetary authorities should make foreign exchange available to pay it. This obligation is assumed voluntarily, but once made it is legally binding and permanent. The IMF Articles of Agreement provide an opportunity to test whether a rule influences behavior, even when governments face unanticipated circumstances that make compliance inconvenient or even costly in the short run.

The first section provides an overview of the substantive monetary rules under consideration and formulates expectations for commitment and compliance. The second section presents data to address three questions. Why do governments commit themselves to these rules? Under what conditions do they comply? Does commitment to an international rule affect government behavior? The final section explores the findings, provides interpretations, and draws conclusions.

The evidence suggests that legal commitments are crucial to policy choice. Taken as a whole, the findings are most consistent with an interpretation that governments make commitments to further their interests and comply with them to preserve their reputation for predictable behavior in the protection of property rights. Strong regional effects suggest that reputations develop around regional standards of behavior. A positive reputation for respecting the rule of law domestically is associated with compliance, which suggests that whenever possible governments will avoid damaging a valuable reputation for law-governed behavior, this consideration deters them from breaking the law in the international sphere. The influence of a formal legal obligation is especially strong in the first few years after a period of restrictions, which is consistent with the desire of legally committed countries to reestablish their credibility for openness. The implication is that despite the formal ability of the IMF to enforce the rules, it is likely to be the market that "enforces" the public international law of money. The broader message for theorists of international relations is that enforcement need not be overt and centralized to give behavioral rules their bite.

MARKETS AND INTERNATIONAL MONETARY LAW: EXPECTATIONS REGARDING COMMITMENT AND COMPLIANCE

The IMF's Articles of Agreement are the first international accord in history to obligate signatories to particular standards of monetary conduct. Article VIII spells out the general obligations of members, and Section 2 rules prohibit restrictions on the making of payments and transfers for current international transactions. The purpose is to do what international trade

law had been doing for decades: provide a framework that facilitates the exchange of goods, services, and capital among countries.²

Governments may not wish to maintain an open current account for two main reasons. One may be to support developmental objectives that favor certain kinds of transactions (exports, capital inflows) over others (imports, capital outflows) based on established state priorities. The other may be to ameliorate balance-of-payments problems (Edwards 1985, 381–2). Under these conditions, governments usually want to retain the flexibility of intervening to conserve foreign exchange in whatever ways they consider appropriate.

The IMF has always viewed such systems of control as dangerous substitutes for economic adjustment and as inhibitors to the development of free foreign exchange markets. But because many of the IMF's founding members could not immediately achieve full convertibility, Article VIII obligations are voluntarily. Upon joining the fund, new members can avail themselves of transitional arrangements under Article XIV, which effectively grandfather restrictions in place upon their accession.³ Yet, the articles do not specify either a time limit on the transitional period or a set of criteria for ending it (De Vries and Horsefield 1969, 225). The IMF encourages countries it believes are in a position to do so to make an Article VIII commitment, but there are no direct positive or negative incentives.

Furthermore, the IMF does not directly enforce obligations, although it publishes data on states' policies from which one can infer compliance (see the Appendix). The Executive Board can approve restrictions (or not), and this role may be quite important in ensuring a degree of transparency in distinguishing policies consistent with a contingent rule from the exercise of sheer policy discretion. Technically, the Executive Board can declare a member ineligible to use the resources of the fund if it "fails to fulfill any of its obligations" under the articles, and noncompliance sometimes interrupts drawings under stand-by and extended arrangements. The IMF has used these formal remedies very sparingly. Noncompliers rarely

business and tourist travel, family remittances, royalties, dividends, interest, and other noncapital transactions. The articles explicitly permit the regulation of international capital movements (Article VI, section 3).

¹ Current transactions include payment for goods, services, and "mvisibles"—insurance charges, warehousing charges, shipping,

² Article IV, section 1. The original White Plan advocated "the general policy of foreign exchange trading in open, free and legal markets, and the abandonment as rapidly as conditions permit of restrictions on exchange controls" (Horsefield and deVines 1969, 64).

³ Article XIV, section 2. An Article XIV country also can adapt its restrictions without the need for IMF approval, but it cannot introduce new restrictions without approval; cannot adapt multiple currency practices without approval; and cannot maintain restrictions that the member cannot justify as necessary for balance-of-payments reasons (see Horsefield 1969, 248–59).

⁴ Article XV, section 2(a).

⁵ According to Gold (1988, 466): "All standby arrangements include a uniform term on measures that directly or indirectly affect exchange rates. Under this term a member is precluded from making purchases under an arrangement if at any time during the period of the arrangement the member: '(I) imposes [or intensifies] restrictions on payments and transfers for current international transactions; or (ii) introduces [or modifies] multiple currency practices, or (iii) concludes bilateral payments agreements which are inconsistent with

have to worry about direct retaliation, since members that vote for some kind of punishment may be concerned about similar treatment in the future. The fund is much more likely to try persuasion than apply a remedy for continued noncompliance (Gold 1979, 185).

My analysis focuses on enforcement that flows from market rather than official sources (Greif 1992, 1993; Greif, Milgrom, and Weingast 1994; Milgrom, North, and Weingast 1990). The literature on time-inconsistent policy stresses private actors' broadly held belief that a government's optimal ex post policy may differ from its optimal ex ante strategy (Barro and Gordon 1983; Canzoneri and Henderson 1991; Kydland and Prescott 1977; Staigler and Tabellini 1987). In the case of time-inconsistent policies with fixed preferences, it is difficult for a government to commit to refrain from altering policy in the future. Alternatively, one can understand a commitment to Article VIII as an attempt by governments to signal their future policy stance under conditions of incomplete information. In this case, resolving the credibility problem requires the government to be able to signal its "true type." Of course, such a signal is only meaningful if it helps private actors separate true liberalizers from governments that are more committed to other goals, such as redistribution. The key is for the true reformer to be able to send a signal that distinguishes it from a less committed type (Persson 1988; Persson and Tabellini 1989; Spence 1974). Rodrik (1989) emphasizes that in order to send a credible signal of true intentions in an incomplete information environment, a government may have to send a stronger signal than it would in the absence of a credibility problem.

My argument is that Article VIII commitment is one way in which governments may seek to enhance their credibility to markets that doubt their ability or willingness to maintain current account policy liberalization into the future. Governments that are interested in efficiency gains from international transactions have good reasons to establish their credibility through such a commitment. True, there are alternative mechanisms (Rowlands 1993), but few provide as clear a signal or as potentially binding a constraint as an Article VIII commitment. For governments whose credibility suffers from a basic problem of time inconsistency, Article VIII triggers more stringent scrutiny than would be the case with a simple policy pronouncement.

Whatever the source of government credibility, Article VIII raises the costs of policy reversal in three ways. (1) It focuses expectations on a clear codified standard, the exact meaning of which has been authoritatively interpreted by the Executive Board of the IMF. (2) It provides transparency through regular consultations with the fund staff. (3) It mobilizes a new set of external actors (private economic, governmental, government that is considering or engaging in rule violation. Indeed, as Lipson (1991) argues, treaties are

and legal) who may exert pressure to comply on a

designed, by long-standing convention, to raise the credibility of promises by staking national reputation on adherence to them (see also Abbott and Snidal 2000). The potential for incurring costs can be very helpful in making a credible commitment in the first place (Martin 1993).

This is not to suggest that Article VIII is a straightjacket on governmental policy choice, but few international commitments are. In a time-inconsistency framework, contingent rules allow for temporary departures under well-understood circumstances (Bordo and Kydland 1995; Bordo and Rockoff 1996). Recent innovations in signaling models explore the conditions under which reneging on a pledge need not harm reputation: If it occurs under circumstances that are known to be especially adverse, then this action will not reveal how a particular government will behave in more normal times. Rule violation, in extreme cases, is therefore not informative for purposes of developing a reputation (Drazen 1997). Review by the IMF Executive Board regarding temporary approval of restrictions that violate Article VIII obligations provides the flexibility recently incorporated into reputational models.

The literature gives relatively little attention to the problem of establishing credibility in an international setting when there is competition among jurisdictions. An emerging empirical stream suggests, however, that countries with poor protection for investors, measured by both the character of legal rules and the quality of law enforcement, tend to have small and narrow capital markets (La Porta et al. 1997) as well as more limited and costly access to the pool of international capital (Sobel 1999). Studies also have established the link between corruption and other national risks and trends in foreign investment (Ramcharran 1999; Wei 2000). Investors and traders can choose among a range of business venues, and they prefer to do business in venues characterized by a national commitment to the protection of property rights. Therefore, governments compete for international business through various reputational devices (see, e.g., Maxfield 1997) in the same way that they are forced to compete in a broad range of regulatory areas when factors of production are mobile (Aman 1995; Cerny 1994; Oberhansli 1997; Sinn 1999; Tiebout 1956).

The first expectation is policy convergence, especially among countries whose venues are near substitutes for one another. Governments that lag significantly the policies of their more credibly liberal competitors place their business sector at a competitive disadvantage. Yet, economic agents are likely to tolerate noncompliance if it is rampant among similar states. There are two reasons for this, and both relate to the consequences of noncompliance. First, to renege when everyone else does is likely to be interpreted as a situation sufficiently dire to justify the contingency of the rule. In a signaling framework, noncompliance in the context of generalized violation may not provide clear information on a country's future policy intentions. Second, investors and traders incur high costs (and run into collective action problems) if they attempt to punish numerous violators. Noncompliance is

much less costly for a particular government if many others are also in violation. Thus, the decision to liberalize the foreign exchange market is strongly influenced by the policies adopted by international competitors.

The second expectation is that a reputation for the rule of law correlates with compliance. One implication of viewing market pressures as the primary enforcement mechanism for international monetary rules is that a government should be very concerned to develop a reputation for openness and especially predictability. Moreover, governments that have invested in and rest on a stable legal framework at home are unlikely to jeopardize this reputation by lightly flouting international legal obligations. Governments that regularly ignore property rights do not have much reputation to lose. Similarly, after a period of noncompliance, governments committed to Article VIII have an incentive to reestablish their credibility swiftly by returning to predictable, open behavior as soon and as convincingly as possible. In short, a public legal commitment to the international community raises expectations and encourages compliance, even under difficult circumstances. Compliance is enhanced by a decentralized system in which competition and concern for reputation motivate behavior.

COMMITMENT, COMPLIANCE, AND THE EFFECT OF RULES: FINDINGS

Why Commit to Article VIII?

If the reasoning outlined above is correct, then the decision to commit to Article VIII should be strongly influenced by markets that value certainty and policy liberalism. Therefore, the choices of economic competitors should be important factors. Two variables are used here to test this proposition. The first is the proportion of IMF members committed to Article VIII (see the Appendix for all definitions and sources), and the second is the proportion of jurisdictions within a given region that have made such a commitment. The hypothesis is that as the latter proportion increases, so does the likelihood that any given country in that region will make such a commitment.

A serious test requires a battery of economic, institutional, and domestic political controls to minimize the possibility that any regional correlation is spurious. Downs and Rocke (1995) argue persuasively that commitments are endogenous to expectations about future compliance. Because countries with an economy that is vulnerable to sharp swings are likely to find it difficult to comply, susceptibility to balance-of-payments pressure is expected to make a government less likely to accept Article VIII obligations in the first place. Developmental level and the direction of the business cycle also may influence the decision to make a legal commitment with respect to economic policy. Therefore, the following analysis controls for gold and foreign exchange reserves as a proportion of GDP (Reserves/GDP), volatility in this proportion (Reserve Volatility), GDP per Capita, and GDP Growth.

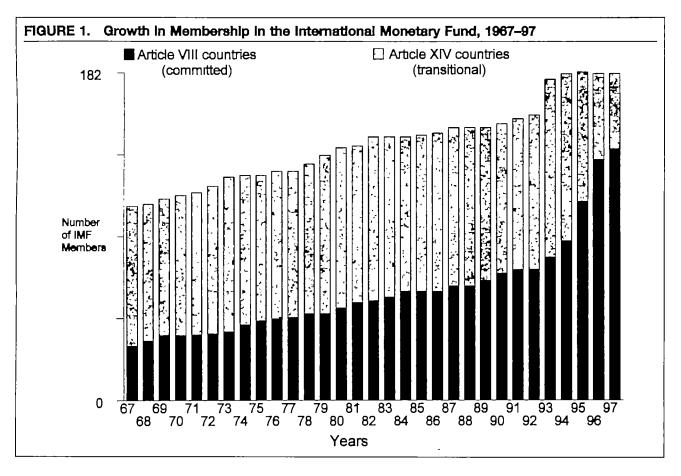
A second set of controls addresses the institutional context. Two of these relate to the IMF's potential role. First, the fund may have policy leverage through the distribution of its resources. If it applies leverage to commit to liberalization, then the use of IMF credits should be associated with a greater propensity to commit to Article VIII. Second, the fund changed its surveillance policy in 1977 in ways that may affect the decision to commit. Previously, governments willing to announce acceptance of Article VIII obligations could avoid multilateral surveillance (Gold 1983; 474-5).6 Article XIV countries, in contrast, were subject to wide-ranging and even invasive "consultations," during which the IMF staff broadly reviewed and the Executive Board passed judgment on the member's balanceof-payments position. Therefore, the acceptance rate should be higher, ceteris paribus, before 1977 than after. The third control in this set relates to the varying degree of flexibility in exchange rates among countries. Flexible rates absorb some of the balance-of-payments adjustment burden and mitigate the need for large reserves to defend the currency. At any given reserve level, greater exchange rate flexibility should be associated with a higher propensity to commit.

Domestic political conditions are another likely influence on the decision to commit to Article VIII. Most obviously, a government may choose liberal policies because that is what the polity demands. Article VIII provides a right of access to foreign exchange for residents and nonresidents, which is a valuable guarantee to the traded goods sector. For importers, it signals to foreign firms that the government is not likely to interfere in the foreign exchange market or to intervene arbitrarily in international business transactions.⁷ This provision is also likely to be favored by export groups, who are concerned with issues of reciprocity and retaliation (Gilligan 1997). Trade dependence should positively influence the legal commitment to free and open foreign exchange markets. Finally, one might expect that civil society's demand for guaranteed foreign exchange access is most likely to be addressed by a democratic regime. In political terms, private interests are likely to oppose the state, which stands to garner concentrated rents as the dispenser of limited access to hard currency. If so, then democratic governance should contribute to a higher rate of Article VIII acceptance.

Before proceeding, a graphic presentation of the data is useful. Figure 1 shows that membership in the IMF has grown considerably over the past three decades, as has the proportion of countries committed to Article VIII. By 1995 a clear majority of members had abandoned transitional status and obligated themselves to openness.

⁶ Consultations with Article VIII countries were established in 1960 but were completely voluntary (de Vries and Horsefield 1969, 246–7).

⁷ According to de Vries and Horsefield (1969, 285–6), for example, "Article VIII status had come to signify over the years either that a country had a sound international balance of payments position, or that if its payments position was threatened, it would avoid the use of exchange restrictions"



For many countries, the transitional period was lengthy, as indicated by the Kaplan-Meier "survival function" in Figure 2, which uses yearly observations for 153 countries who were members by 1995.8 The Kaplan-Meier function estimates about a 25% chance of accepting Article VIII status in the first five years of IMF membership, a 50% chance within 31 years, and about a 75% chance after 40 years. Clearly, many countries have not rushed to make a legal commitment to keep the current account free from restrictions.

What factors affect the rate of acceptance of the obligations under Article VIII? Note that this is a unidirectional decision: Once made, it cannot be rescinded, although a country can fail to comply with its commitment (discussed below). I use techniques of survival analysis that focus on the spell of time until the event of interest occurs (in this case, an Article VIII commitment). A hazard model is appropriate in this case because it captures the accumulation of "risks" over time that affect the decision to commit.9 Specifically, I employ a Cox proportional hazard model to

examine the effects of a number of continuous and categorical predictors; because some of these vary over time, the tests presented here use time-varying covariates. The Cox model estimates a "hazard rate" for Article VIII acceptance at a particular moment, 10 and this hazard rate is modeled as a function of the baseline hazard (ho) at time t, which is simply the hazard for an observation with all explanatory variables set to zero, 11 as well as a number of explanatory variables, the estimates of which indicate proportional changes relative to this baseline hazard.

Table 1 presents the findings of the Cox proportional hazard estimation for the variables discussed above. (Note that a ratio of more than 1 indicates an increase in the rate of Article VIII acceptance, and a ratio of less than 1 indicates a reduction in the rate of acceptance.) Model 1 is a reduced form version that includes only relevant variables for which a very strong case can be made that they are exogenous to the commitment decision. Models 2-4 present the effects of variables that are arguably endogenous to some degree, as well as other exogenous variables for which one cannot reject the null hypothesis of no effect on the baseline

The literature usually terms the event of interest a "failure" and the time elapsed until its occurrence as "survival," regardless of the substantive problem modeled. Proponents of international openness and free markets would view "survival" analysis as "transition" analysis and an Article VIII commitment as a "success"; those who favor closer government management of markets might consider the customary appellations more apt.

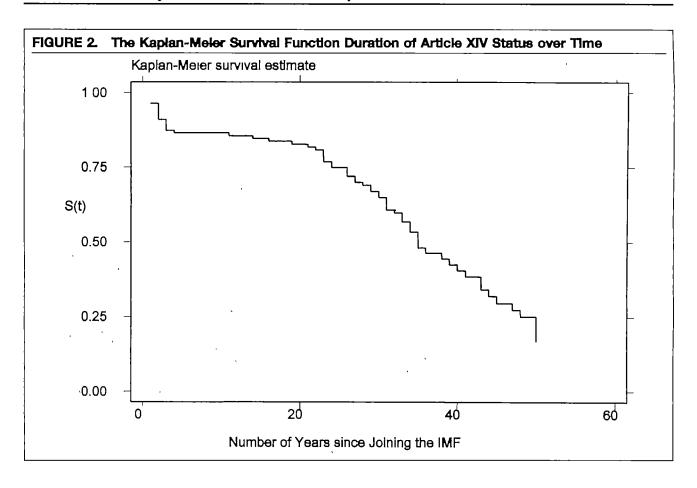
⁹ The hazard model is more general than a panel probit because it allows the underlying probability of committing to Article VIII to change each year. In addition, the structure of the data (all 0s and a single switch to 1 at the point of each country's commitment) is

analogous to "death" in the epidemiological studies in which such models are frequently employed.

¹⁰ The hazard rate is defined as:

 $h(t) = \frac{\text{probability of accepting Art. VIII between times } t \text{ and } t + 1$ (probability of accepting Art. VIII after time t)

¹¹ In this case, I have set all variables to their minimum value in order to avoid interpretations based on deviations from unobserved values of the explanatory variables.



hazard rate. Data availability causes the sample size to change somewhat, but none of the conclusions discussed below are altered substantially by analyzing a common sample.

The first two variables, Universality and Regional Norm, test the proposition that taking on an obligation is likely to be related to similar actions by others. Both of these have a large and positive influence on the acceptance rate. According to model 2, for example, every increase of one percentage point in the proportion of IMF members who accept Article VIII increases by 5.5% the likelihood of acceptance by other members. A similar increase in the regional proportion of Article VIII adherents increases a country's likelihood of acceptance by 2.7%. This translates into an increase of 31% in the rate of acceptance for every increase of 10% in regional accession.12 Clearly, as the number of countries who adhere to Article VIII increases, there is a much greater chance that an uncommitted government will do so. Note that this effect is significant even when controlling for time (Year in model 4), which reduces the likelihood that the universality and regional norms variables simply reflect an increase in adherents over time.

There is also evidence that institutional incentives make some difference in Article VIII acceptance. Surveillance (a dummy variable with the value of 0 before 1977, 1 thereafter) has the expected negative

effect, although it is not robustly statistically significant for this sample of countries, many of which joined the IMF after surveillance became mandatory for all members. Moreover, the coefficient is significantly weakened by including year as an explanatory variable. This is understandable, since surveillance is a dichotomous dummy that distinguishes years before and after 1977. Flexible exchange rates probably increase the likelihood of making an Article VIII commitment, since these minimize the foreign exchange reserves needed to defend a beleaguered currency, but the statistical significance varies with the model specification.

Perhaps most interesting is the fairly strong and consistent negative effect of the use of fund credits on the Article VIII decision. This provides evidence that the IMF is not effectively using resources as leverage to pressure borrowers into making a legal commitment.¹⁴ In fact, *Use of Fund Credits* reduces the proportional

 $^{^{12}}$ This is calculated by raising the estimated hazard ratio to the tenth power.

¹³ For countries that had joined the IMF by 1980, earlier research shows that the change in the surveillance regime had a very strong negative effect on the decision to commit to Article VIII, which indicates a rather perverse incentive to commit. Mandatory surveillance for all countries drastically reduced the probability of accepting Article VIII by countries that were members during the regime change (Simmons 2000).

¹⁴ This finding is consistent with archival research, which suggests that the IMF is more likely to recommend that a country delay commitment to Article VIII when its balance of payments is under pressure (Simmons 2000). The reason is that such a commitment not only would lack credibility but also would allow the "leakage" of IMF resources to pay private creditors rather than to buy time for the government to make fundamental economic policy adjustments

Explanatory Variable	Reduced form Model 1	Model 2	Model 3	Model 4
Universality	1.066* (.010)	1.055* (.011)	1.24 7* (.089)	1.040 (.024)
Regional Norm	1.029* (.005)	1.027* (.005)	1.038* (.010)	1.028* (.005)
Use of Fund Credits	_	.534* (.160)	.577* (.241)	.548* (.169)
Flexible Exchange Rate	-	1.52 (.418)	2.659* (1.286)	1.512 (.409)
Survelliance	_	_	0.46* (.053)	.407 (.295)
Openness (Trade Dependence)	1.008* (.002)	1.009* (.003)	1.019* (.004)	1.009* (.179)
Democracy	_	-	1.028 (.034)	_
GNP/Capita	1.00007* (.00002)	1.00007* (.00003)	1.00009* (.00004)	1.0000 (.0000
GDP Growth	1.033 (.020)	1.035 (.021)	1.021 (.041)	1.036 (.022)
Reserves/GDP	_	1.740 (.493)	.950 (1.192)	1.744 (.505)
Reserve Volatility	_	.770 (.157)	.883 (.300)	.753 (.155)
Year	_	_		1.052 (.051)
No. of countries	133	128	106	128
No. of acceptances	77	72	36	72
Time "at risk"	2,462.99	2,375.95	2,177.96	2,375.95
_og-likelihood	-228.089	-200.354	-88.305	-199.51
r ²	163.58	165.36	80.20	163.61
$0 > \chi^2$	0.00	0.00	0.00	0.00

hazard rate by about 44% (model 1), which is consistent with an argument that links fund assistance with problems of moral hazard. This result makes sense in light of the signaling literature, which suggests that contingent aid makes it much more difficult for a government to signal its true intent about future policy to the market (Rodrik 1989). Because such a signal would be difficult to interpret, there is little reason for a borrowing government to send it.

Domestic political demands that flow from trade openness also have an important influence on the acceptance rate. Openness to the international trade system raises the proportional hazard rate significantly. According to model 1, every increase of one point in imports plus exports as a proportion of GDP increases by .8% the likelihood of Article VIII acceptance. This can account for a difference of 26% in acceptance probability for countries with trade profiles as different as, say, Malaysia (where imports plus exports approxi-

mate 80% of GDP for the period under consideration) and the Philippines (where the corresponding ratio is a little more than 50%). The presence of a democratic regime has no independent effect on the propensity to commit to openness (model 3).

Finally, the economic controls basically fulfill expectations, although most fall short of traditional standards of statistical significance. A commitment to external liberalization is more likely under good and improving economic conditions. High per-capita income, high GDP growth, and strong reserves of foreign exchange are likely to influence a government to commit (the latter two with a probability of <.10). Reserve volatility, although not statistically significant at traditional levels, is correctly signed.¹⁶ What is

 $^{^{15}}$ This is calculated by raising the estimated hazard ratio to the 29th power.

¹⁶ Current account balance and volatility as well as terms of trade

TABLE 2	Rates and Years of Noncompliance Due to Restrictions on Current Account, by
Country, 1	967–97

Country	Rate of Noncompliance (1967–97)	Years Committed (1967–97)	Dates of Restrictions	
Domlnican Republic	1.00	31	1967–97	
El Salvador	.87	31	1967– 9 3	
Jamalca	.81	31	1 968–69 , 1973– 9 5	
Guyana	.77	31	1967, 1971– 9 3	
Chile	.75	20	1983 -9 5, 19 96-9 7	
South Africa	.71	24	1979 -9 3, 19 96-9 7	
Cyprus	.71	7	1 9 91–93, 19 96–9 7	
Iceland	.64	14	19 84-0 2	
Morocco	.60	5	1993, 199 8-9 7	
Argentina	.59	29	1972–77, 1983–93	
Costa Rica	.55	31	1972–73, 1975, 1982– 9 5	
Peru	.55	31	1971–78, 1985 –9 2, 1996	
St. Lucia	.53	15	1981–86, 19 96–9 7	
Guatemala	52	31	1967–73, 1981–89	
St. Vincent	.50	14	1982–86, 19 96–9 7	
Barbados	.50	4	19 96-0 7	
Israel	.50	4	19 96-9 7	
Nicaragua ·	.48	31	1979–93	
Ecuador	.41	27	1983–93	
Greece	.40	5	1996–97	
Tunksla	40	. 5	199 8-9 7	
Honduras	.39	31	1982 -9 3	
FIII	.28	25	1989–92, 1996–97	
taly	.26	25 31		
•			1975–82	
Swazland	.25	8	1996-97	
Turkey	.25	8	19 96-9 7	
Grenada	.25	4	1997	
Austria	_, .23	31	1967–73	
Bollvla	.23	30	1982–86, 19 96–9 7	
Korea	.22	9	1 996 –97	
Beltze	.21	14	1982, 1 996–9 7	
St. Kitts and Nevis	.18	11	19 96-9 7	
Mexico	.16	31	1983-87	
United Kingdom	.16	31	1967–71	
Antigua and Barbuda	.14	¹ 14	1 996-0 7	
France	13	31	1969–71, 1983	
Наль	.13	31	1968–71	
Dominica Dominica	.1.1	18	1996-97	
Japan	.10	30	1968–70	
Japan Oman	.09	23	1996–70 1996–97	
Papua New Guinea	.09	23 22	19 96-9 7 19 96-9 7	
Papua New Guinea Bahamas				
Banamas Netherlands Antilles	.08 .06	24 31	19 96-9 7	
			19 96 9 7	
Solomon Islands	.06 ,	18	1997	
Singapore	.05	21	1997	

Note: Noncompliers with three or fewer years of Article VIII commitment (1995-97) are Algeria, Bengladeeh, Benin, Botswana, Burkma-Faso, Cameroon, Central African Republic, Chad, China, Comoro, Congo, Croatia, Czechoslovakia, Gabon, Georgia, Ghana, Guinea-Bassau, India, Ivory Coast, Jordan, Kazakhistan, Kenya, Madagasioar, Malawi, Malta, Moldova, Namible, Nepal, Niger, Pakletan, Paraguay, Philippinee, Poland, Senegal, Sierra Leone, Slovak Republic, Slovenia, Sri Lanka, Tanzania, Togo, Tonga, Ukraine, Western Semoa, and Zimbabwe.

				Mo	del 4
Explanatory Variables	Model 1	Model 2	Model 3	Coef.	Δprob.
Constant	-1.907* (8.56)	-2.173* (.984)	-3.154* (1.038)	-2.09* (.898)	·
Regional Noncompliance	6.409* (.9 96)	5.973* (1.002)	6.427* (1.145)	5.90* (.966)	.62
Rule of Law	−.535* (.137)	−.572* (.148)	−.593 * (.168)	−.569* (.146)	45
Bureaucratic Quality	.409* (.142)	.476* (.153)	. 621* (.170)	.447* (.150)	.38
Democracy	_	_	0011 (.008)	_	
Openness	_	.051 (.301)			
Exchange Rate Flexibility	_	123 (.284)	. - .	_	
Use of Fund Credits	_	.742* (.355)	1.126* (.3 99)	.676* (.341)	.16
Average Balance of Payments/GDP	−.098 * (.034)	096* (.032)	−.131* (.047)	−.091 * (0.30)	32
Terms of Trade Volatility	.609* (.257)	.642* (.266)	.662* (.302)	.660* (.265)	.28
World Interest Rate Shocks (non-OECD countries)	−.177 * (0.57)	208* (.061)	221* (.065)	−.205* (.0 6 0)	30
No. of cases	691	646	607		691
$p > \chi^2$	0.00	0.00	0.00		0.00
Log-likelihood	-155.95	-151.76	-127.65		-154.02
Pseudo-R ²	.623	.618	.654		.628

Note. The dependent variable is current account restrictions. The range of analysis is Article VIII countries only, 1982–96. The results are for a logit model with correction for time dependence of observations coefficients (robust standard errors). Three cubic splines were included but not reported here. For model 4, Δ prob refers to the effect on the producted probability of a restriction of an increase of two standard deviations in the variable's value (centered on its mean), with all other variables held at their means, with the exception of use of fund credits and the cubic splines, which are held at 0. For use of fund credits and the cubic splines, which are held at 0. For use of fund credits, Δ prob is calculated moving from 0 to 1. $^{\circ}p > |Z| = .06$.

interesting is the apparent strength of external behavior as an influence on the commitment decision, even when controlling for economic conditions to which governments obviously feel pressures to respond. This seems to suggest that "peer pressure" in a competitive market context, rather than either IMF pressure or economic conditions alone, are in part driving the willingness of governments to make a legal commitment to a liberal international monetary policy.

Why Comply with an Article VIII Commitment?

Members of the IMF are legally required to fulfill their commitment to keep the current account free from restrictions and to maintain unified exchange rates. Although 38 members have a perfect record on both

volatility were also analyzed. The results were imagnificant and are not reported here.

counts,¹⁷ the same cannot be said for the countries listed in Table 2. Most of the long-term noncompliers are concentrated in Latin America, despite the marked trend toward liberalization in this region during the mid-to-late 1990s. The global financial crisis of 1996–97 elicited foreign exchange restrictions in a number of Article VIII countries that otherwise had exemplary records (e.g., Singapore and Korea).

What explains the variance in compliance among countries that have chosen openness? This section focuses only on cases in which governments have

¹⁷ As of 1997: Armenia, Bahrain, Canada, Denmark, Djibouti, Estonia, Finland, Gambia, Germany, Hungary, Indonesia, Kiribati, Kyrgyz Republic, Latvia, Lebanon, Malaysia, Marahall Islands, Mauritius, Micronesia, Mongolia, New Zealand, Norway, Panama, Portugal, Qatar, Russia, San Marino, Saudi Arabia, Seychelles, Spain, Sweden, Switzerland, Thalland, Trinidad and Tobago, United Arab Emirates, United States, Vanuatu, and Yemen Arab Republic. There are very few yearly observations on several of these cases, however.

committed to Article VIII and then implemented restrictions on the current account. ¹⁸ The most obvious explanation is economic pressure that makes the maintenance of an open current account and unified exchange rates very difficult. Certainly, that is a likely factor in Latin America in the 1980s and in Asian countries in recent years. In the tests that follow I present results that control for Average Balance of Payments/GDP, Terms of Trade Volatility, and World Interest Rate Shocks. These are chosen not only because they can have a substantial effect on the ability of governments to maintain a current account free of restrictions, but also because they are unmistakably exogenous to a government's policy at any particular time.

My central concern is the pressure that the behavior of other similarly situated countries places on the decision to comply. Investors and suppliers should prefer to do business in countries whose legal framework protects international contracts. In Table 3, Regional Noncompliance reflects the extent to which countries within the same region have implemented restrictions on current accounts. On the one hand, the expectation is that if Article VIII countries in the region often disregarded their commitment, then the probability increases that any given country in that area will not comply. On the other hand, the more competitors are willing to comply, the greater is the pressure for any one country to comply, even in the face of conditions that favor protecting the national economy through restrictions.

The institutional context also may be important for the compliance question. First, if the IMF is "enforcing" liberal legal obligations through the conditional distribution of resources, then one would expect the use of these credits to be associated at the margin (i.e., controlling for economic conditions) with compliance. Second, compliance may be more palatable if it is not essential to maintain reserves to defend a fixed value of the currency. Flexible rates should be associated with greater compliance.

Finally, it is important to consider characteristics of the domestic polity itself, Several authors imply that compliance with international legal commitments is much more prevalent among liberal democracies, due to the constraining influence exercised by domestic groups with an interest in or a preference for compliant behavior (Schachter 1991, 7; Young 1979; see also Moravesik 1997). In this view participatory politics may pressure the government, especially if noncompliance curtails the rights of residents to foreign exchange. Others argue that the most important characteristic of liberal democracy in terms of international compliance is a strong domestic commitment to the rule of law. The many variants of this argument range from that of Slaughter (1995), who maintains that independent judiciaries in liberal democracies seem to share some of the same substantive approaches to law, to a more general contention that domestic systems that value rule-based decision making and dispute resolution are also more likely to respect rules internationally.¹⁹ In essence, these are affinity arguments: Domestic norms regarding limited government, respect for judicial processes, and regard for constitutional constraints "carry over" into the realm of international politics.²⁰ They rest on an intuitively appealing assumption that policymakers and lawmakers are not able to park their normative perspectives at the water's edge (Risse-Kappen 1995).

There are other reasons to expect the rule of law to be associated with Article VIII compliance. Rule-oflaw countries have a strong positive reputation for maintaining a stable framework for property rights. Markets expect them to keep their commitments, and to undermine this expectation would prove costly. Countries that score low with respect to the rule of law do not have much to lose by noncompliance; erratic behavior is hardly surprising to investors and traders. I use an indicator that is especially appropriate to test the market's assessment of the reputation for rule of law: a six-point scale published by a political risk analysis firm expressly to assess the security of investments (see Knack and Keefer 1995, 225). The scale measures the willingness of citizens peacefully to implement law and adjudicate disputes using established institutions. Higher scores indicate such institutional characteristics as a strong court system, sound political institutions, and provisions for orderly succession. Low scores reflect extralegal activities in response to conflict and to settle disputes.

Because I have argued that compliance is market enforced and that markets prefer certainty in the legal framework, the comparison between the participatory characteristics of democracy and rule-of-law regimes should be especially telling. There is no reason to expect that democracy alone provides the stability that economic agents desire; on the contrary, popular participation along with weak guarantees for fair enforcement of property rights can endanger these rights. It is true that the two variables are positively correlated (Pearson = .265), but they are certainly conceptually distinct and may have very different effects on the decision to comply with Article VIII. In particular, I directly compare two regime characteristics that are often conflated: the participatory dimensions of Democracy and the procedural emphasis of the rule of law. A measure for Bureaucratic Competence is also included to control for the differential capacity of states to intervene in foreign exchange markets.

In this case the compliance decision is modeled using a logistical regression (logit) model; the dependent

¹⁸ This is presented as a priori evidence of noncompliance, even though at this point I do not examine the technical question as to whether the IMF Executive Board approved the restrictions in place, rendering them "legal" temporarily.

¹⁹ This captures the flavor of some of the democratic peace literature, such as Doyle (1986), Dixon (1993), and Raymond (1994).
²⁰ "International law is not unlike constitutional law in that it imposes legal obligations upon a government that in theory the government is not free to ignore or change" (Fisher 1981, 30). Constitutional constraints most often rest on their shared normative acceptance rather than on the certainty of their physical enforcement, which is another possible parallel to the international setting.

variable has a value of 1 for the presence of restrictions, 0 for their absence. (Since I am analyzing only Article VIII countries, restrictions constitute noncompliance.) Because the data consist of observations across countries and over time, with a strong probability of temporal dependence among observations, a logit specification is used that takes explicit account of the nonindependence of observations (Beck, Katz, and Tucker 1998).²¹ The results are reported in Table 3.

One of the most important findings is, again, the clustering of compliance behavior across regions. Article VIII countries are much more likely to put illegal restrictions on current accounts if other countries in the region are doing so. Can this be due to common economic pressures in the region? That possibility cannot be ruled out completely, but it is rendered less likely by the range of economic variables included in the specification. Various measures of the current account, trade volatility, and interest rate shocks failed to wash out apparent regional mimickry. Three other economic variables (GNP per capita, change in GDP, and reserves as a proportion of GDP) were included in the analysis but are not reported here, since they were not statistically significant and did not affect the results reported above. Compliance decisions are apparently not being made on the basis of economic conditions alone but with an eye to standards of regional behavior. The most obvious reason would be reputational consequences in a competitive international economy. Indeed, one possible indication of the importance of reputational pressures may be found in the influence of world interest rates. As these increase (indicating a more competitive global environment for capital), non-OECD governments were much less likely, ceteris paribus, to violate Article VIII commitments, despite the fact that higher rates are likely to increase balanceof-payments pressures.

The domestic political variables tell an interesting story with respect to regime characteristics. First, the evidence is strong that states must have the bureaucratic capacity to renege on their commitments. The strong positive relationship between bureaucratic quality and restrictions implies that these choices are more likely to be made when the capacity exists to implement them. Second, trade dependence has virtually no effect on these results. Third, in contrast with theories of international behavior that concentrate on the law-consciousness of democracies, the evidence here suggests that democracy contributes little or nothing when other factors are held constant, ²² but a strong domestic com-

mitment to the rule of law contributes positively to compliance. In fact, a move from one standard deviation below to one standard deviation above the mean on the rule-of-law scale (from 2.3 to 5.7 on the six-point scale) reduces the probability of noncompliance by .45, according to model 4, holding other variables constant.

If the IMF enforced compliance, we might expect borrowing from the fund to have a strong negative effect on the dependent variable. Yet, the coefficient is positive and statistically significant. The fund is tolerating a good deal of noncompliance when it comes to restricting access to foreign exchange, ²³ a finding that is inconsistent with the successful use of leverage.

To Commit or Not to Commit: What is the Difference?

I now consider whether the law matters. Specifically, does an Article VIII commitment have an independent effect on behavior, once we take into account the obvious economic factors that are likely to lead to restrictions?

In order to answer this question, I examine the entire data set (133 countries, including Article VIII and Article XIV cases, with observations over time averaging 20 years) using logit analysis to explain the existence of restrictions.²⁴ Article VIII commitment is forced to compete with a broad range of economic conditions that clearly are associated with restrictions: volatility in the terms of trade, reserves, and the balance of payments; poor economic growth rates; and low GNP per capita.²⁵

Table 4 presents the results of this analysis. Most of the economic variables have the expected effects, and many are significant. Strong influences are associated with terms of trade volatility and business cycles (both clearly exogenous to the decision to restrict current accounts), balance-of-payment difficulties (reasonably exogenous), and low reserves. But because reserves may be endogenous to the decision to restrict the current account, they are not included in model 1, the reduced form exogenous version. As seen in earlier analysis of the Article VIII countries alone, trade openness is al-

²¹ This solution takes advantage of the fact that annual time-series cross-sectional data with a dichotomous dependent variable are equivalent to grouped duration data. To take explicit account of the temporal grouping of the dependent variable, a counter vector was created, such that t=0 if a restriction is in place or if a country is in the first year of current account liberalization. Successive years of no restrictions are coded 1, 2, ..., n for each year beyond year one. Three cubic splines were then calculated based on this count according to a routine made available on Richard Tucker's website (http://www.fas.harvard.edu/~rtucker/papers/grouped/grouped3.html). The cubic splines were included as explanatory variables, but their coefficients are not reported here. The splines are important as a corrective for assuming that observations are time-independent in this time-series cross-sectional logit framework.

²² For a subset of countries that were IMF members by 1980, earlier

research shows a negative correlation between democracy and compliance with respect to restrictions on current accounts (Simmons 2000).

²³ This is completely consistent with archival research, which has uncovered evidence that stand-by arrangements are often accompanied by the temporary approval of restrictions in order to conserve foreign exchange.

²⁴ The same technique for time-dependence of observations is used as described above.

²⁵ The previous analysis suggests that Article VIII status itself is endogenous, but this endogeneity is not taken into consideration here. First, it is not obvious how to go about creating an instrument for Article VIII, given the structure of the data and the events analysis in the first phase of the argument. Second, a case can be made that one should avoid instruments in cases in which the instrument does not correlate very strongly with the endogenous variable (Bound, Jaeger, and Baker 1995). Third, an anonymous reviewer suggests that the observable variables in the compliance model are very likely to control for nonrandom selection into Article VIII status. For a discussion of controlling for selection on observables, see Heckman and Robb 1985, 190–1.

			Model 3		
Explanatory Varlables	Model 1	Model 2	Coef.	Δprob.	
Constant	699 (.413)	.680* (.331)	.598* (.355)		
Article VIII Commitment	−.903 * (.136)	-1.101 * (.135)	−1.111 * (.130)	18	
Regional Restrictions	4.00* (.395)	-	_		
Terms of Trade Volatility	.337* (.0 99)	. 417* (.0 95)	.403* (.094)	.18	
Balance of Payments/GDP	−.016* (.008)	022* (.008)	−019 * (.007)	09	
GNP/Capita	.00004 (.00002)	-	.—		
Reservers/QDP	-	1.43* (.526)	.957* (.353)	.05	
Change In GDP	−.032 * (.013)	−.026* (.012)	−.027* (.011)	14	
Openness	002 (.001)	003 (.002)	_		
Use of Fund Credits	_	.826* (.132)	.880* (.131)	.34	
Flexible Exchange Rates	_	.146 (.156)	- .		
Years since Last Restriction	-1.226* (.108)	-1.272* (.111)	-1.26* (.109)	38	
No. of cases	3,053	3,060	3,100		
$p > \chi^2$	0.00	0.00	0.00		
Log-likelihood	-751.75	-805.39	-819.89		
Pseudo-R ²	.64	.62	.62		

Note The results of a time-sense cross-section logit model are reported; the dependent variable is restrictions on current accounts, coefficients are robust standard errors. Three cubic splanes were included but not reported here. For model 3, Δ prob refers to the effect on the predicted probability of a restriction of an increase of two standard deviations in the variable's value (centered on its mean), with all other variables held at their means, with the exception of use of fund credits, years since last restriction, and the cubic splanes, which are held at 0. For use of fund credits, Δ prob is calculated moving from 0 to 1. For years since last restriction, Δ prob is calculated moving from 1 to 5. $^{*}p > |Z| = 0.6$

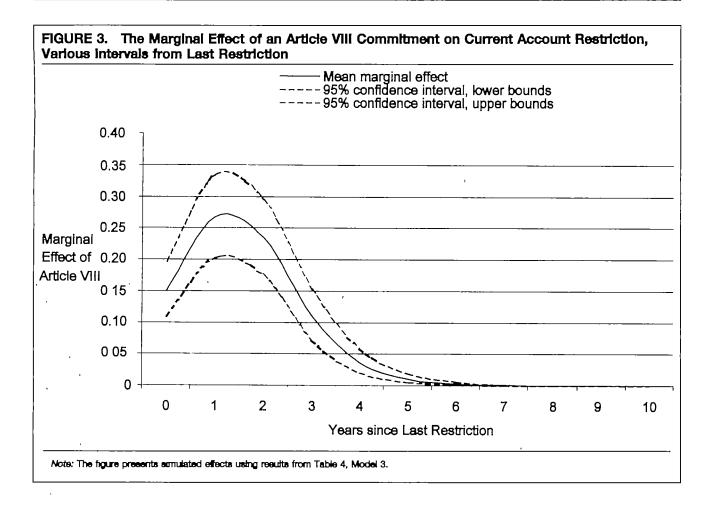
so likely to be associated with liberal policy choice, although certainty does not quite reach standard levels of significance (p < .10). Another interesting finding is that use of fund credits is consistently strongly associated with illiberal policy choice, which again provides evidence that these choices cannot convincingly be explained by fund pressure in the context of extending loans.

Most important for my purposes, however, is that controlling for every likely macroeconomic influence on the decision to implement current account restrictions, a formal declaration of adherence to Article VIII obligations consistently has a strong negative effect on the probability of imposing restrictions. In fact, controlling for all other economic variables as well as for policy inertia, 26 countries that continue to live under

the transitional Article XIV regime have an estimated probability of .87, according to Table 4, model 3, of implementing restrictions; the corresponding probability for an identically situated Article VIII country is only .69. Thus, commitment accounts for a percentage point difference of about 18 in the probability of imposing restrictions on current accounts for the sam-

simulation algorithms developed by King, Tomz, and Wittenberg (1998). These routines have the advantage of producing confidence intervals that take into account the uncertainty surrounding the logit parameters. Conventional methods for calculating probabilities from logit regression assume that the parameter estimates are perfectly estimated and, therefore, do not allow us to report adequately the uncertainty surrounding the probabilities. The simulation algorithms of King, Tomz, and Wittenberg, however, generate a full distribution of parameter estimates that can then be converted to probabilities, a mean can be calculated, and meaningful confidence intervals can be drawn.

Here I report the predicted probabilities as generated by a set of



ple of cases as a whole. Even when we control for the policies of other countries in the region, the legal commitment continues to matter greatly (model 1). Clearly, neither economic conditions nor imitative behavior alone can fully account for the decision to eschew restrictions. The legal commitment itself is an important part of the story.

Interestingly, the effect of the legal commitment is distributed unevenly over time.²⁷ The marginal effects of commitment reported in Table 4 assumed one year had elapsed since a country's previous restriction.²⁸ Figure 3 traces out the marginal effects of commitment on the probability of restrictions as a function of time since the last restrictions. It shows that commitment has the greatest effect in the first two or three years after lifting restrictions. Two years after a restriction, the marginal effect of commitment peaks at about .27, which is a truly significant effect of a legal commitment on behavior. For countries freed from restrictions for the past five years (76 cases), the probability of instituting them is .035 with an Article VIII commitment and .058 without, for a

marginal effect of about .023. There is virtually no independent influence on the legal commitment once a country is six years away from its last restriction.

This suggests that the legal commitment is crucial to policy choice in the first few fragile years of liberalization. That is likely to be a period in which the economy is not yet fully stabilized, and the bureaucratic capacity to intervene may still be available. These conditions provide a severe test of the commitment to maintain liberalization. Governments in Article VIII countries need to convince markets that they are serious about coming back into compliance in order to regain lost credibility. Article XIV countries have much less credibility in the first place. In the absence of reputational effects, there is little evidence that they strive as hard to retain any liberalization they are temporarily able to achieve.

It is not at all surprising that this initial effect should deteriorate over time. The longer a country is free from restrictions, the less likely it is to reimplement them. Over time the bureaucratic capacity to intervene is dismantled. Groups advantaged by the policy are likely to adjust their "portfolios" to the liberalized environment as time passes. This is not to say that a shock would not cause relapse; rather, policy inertia is real, and we should expect it to have a marked influence on outcomes. But this only serves to underscore the effects of the international legal commitment to keep current

²⁷ I have interacted Article VIII with years since last restriction and calendar years and have run analyses in both OLS and logit to see if the results are robust. Nothing changes substantially regarding the importance of making an Article VIII commitment. Additional analysis is available from the author upon request.

²⁸ Denoted as t = 0 for purposes of the counting vector upon which the cubic splines are calculated (Beck, Katz, and Tucker 1998).

accounts free from restrictions. This commitment is a significant factor in whether a liberal inertia has a chance to take hold.

These findings provide fairly strong evidence that legal commitment has a systematic effect on state behavior. It should be underscored that it was shown to affect the propensity to restrict current accounts after controlling for a wide range of economic conditions, the policies of other countries in the region, and the time dependence of observations (policy inertia). It is not easy to counter that these results are merely an endogenous reflection of the actions governments intended to take regardless of a formal commitment. Recall that adherence to Article VIII is a one-way commitment, and in the thirty to fifty years afterward there are certainly likely to be unanticipated occasions on which eschewing restrictions on current accounts proves inconvenient. Nonetheless, in a significant number of such cases, governments apparently decided that their best interests lay in abiding by their commitment to refrain from restrictive policies.

CONCLUSION

The effect of international law on state behavior should be a central concern of international relations scholarship, but few studies have systematically examined this issue. International legal scholars tend to view law compliance as the norm (Chayes and Chayes 1993; Henkin 1979), but political scientists are far more skeptical (Downs, Rocke, and Barsoom 1996; Mearsheimer 1994–95). In the face of daunting conceptual and methodological issues (Simmons 1998), very little evidence has been accumulated to assess basic propositions about why governments commit to and comply with international legal obligations, and whether this makes any difference to outcomes in which we are interested.

The legalization of some central aspects of the international monetary regime after World War II provides an opportunity to inquire into the conditions under which law can influence the behavior of governments in their choice of international monetary policies. In this issue area, we can be fairly precise about what constitutes obligation and compliance, using the IMF's own data. My strategy has been to model the factors that contribute to the rate of Article VIII acceptance, test a set of propositions regarding compliance with this commitment, and assess the effect of that commitment on behavior.

One of the most interesting findings is that the behavior of other countries, especially in one's own region, has far more influence on commitment and compliance than has generally been recognized. Indeed, there is more evidence of competitive policy convergence than of overt pressure from the IMF on borrowing countries. Especially intriguing is the finding that governments are positively influenced by the choices of others in their region. They are more likely to make and to honor a legal commitment if neighbors are doing so. This provides evidence that norms are set and reputations are assessed among groups of roughly

comparable countries, likely through international markets rather than international organizations. Although the IMF undoubtedly has significant influence on some countries at certain times, much more decentralized forces seem largely to be at play.

Among Article VIII countries, two domestic regime effects have clear consequences for compliance. Surprisingly for those who view the international behavior of democracies as somehow distinctive with respect to law and obligation, participatory democracy has little to do with the compliance issue. Regimes based on clear principles of the rule of law are far more likely to comply with their commitments. This indicates that rules and popular pressures can and apparently sometimes do have distinct consequences when it comes to international law compliance. Apparently, governments that provide a stable framework of law and system of property rights domestically are more likely to do the same for international economic transactions. One interpretation is that a credible commitment to a stable system of law is not divisible in the eye of the investor. A rule-of-law government may have even more to lose from noncompliance with an international legal obligation than a more capricious regime.

Perhaps no question has plagued scholars of international institutions as persistently as the challenge to demonstrate that such institutions have a direct effect on state behavior. Does international law order state action? How can we know that governments have not simply committed to do things that they would have done even in the absence of rules? How can we be sure that the rules are not epiphenomenal rather than a constraint on future behavior? "Proof" of such propositions may not be possible, but the evidence presented here addresses such skepticism. Once we control for most of the obvious reasons a government may choose to restrict its current account, Article VIII status still emerges as a truly significant influence on the probability of choosing to restrict. An Article VIII country facing external economic pressures and business cycle conditions matching those of a similarly situated Article XIV country manages much more often to avoid implementing restrictions to cope with these pressures, even if other countries in the region decide to restrict.

This is not simply due to IMF pressure attached to credits, and it is not an artifact of the exchange rate regime. A look at the timing of the estimated effect of commitment reveals something of the law's importance: Significant influence is concentrated in the first few fragile years after a restriction is lifted. Countries that have failed to live up to their obligations seem especially determined to reestablish their credibility. Law seems to matter at a defining moment: Legal commitments can push a country onto a behavioral trajectory of compliance from which it is decreasingly likely to deviate. We should continue to entertain alternative explanations, but these tests indicate that a legal commitment may carry decisive weight in determining some states' international monetary policy.

This article challenges researchers to design projects that will better expose the mechanisms that enhance international law compliance. Does formal commit-

ment trigger greater external reputational consequences associated with noncompliance than does the same policy in the absence of a commitment? Does a legal commitment have consequences in domestic politics that effectively constrain governmental behavior? Does decentralized "enforcement," whether domestic or international, explain patterns of compliance with other sets of rules to which sovereign governments commit? The answers that eventually emerge will have profound implications for both the theory and practice of international relations.

APPENDIX

Dependent Variables

Article VIII Commitment. The variable is scored 1 if a country has accepted Article VIII status, 0 if it remains subject to Article XIV transitional arrangements. Acceptance indicates the end of a "spell" for purposes of the Cox proportional hazard model. Source: IMF various years.

Restriction. Restrictions on payments in current account; scored 1 if restrictions exist; 0 otherwise. When this dependent variable is used only to analyze policies of Article VIII countries, it is interpreted as noncompliance. Source: IMF various years.

Explanatory Variables

Universality. This refers to the proportion of current IMF members, calculated yearly, who have accepted Article VIII status. Source: IMF various years.

Regional Norm. This refers to the proportion of current IMF members within each region who have accepted Article VIII status. Classification of economies by region (East and southern Africa; West Africa; East Asia and Pacific; Eastern Europe and Central Asia; rest of Europe; Middle East; North Africa; Americas) are based on World Bank categories. Source: IMF various years.

Surveillance. This dummy variable indicates whether the period up to and including (0) or after (1) 1977, when a comprehensive regime of IMF surveillance was instituted for all members, whether Article XIV or Article VIII status.

Use of Fund Credit. The variable is scored 1 if a country has made use of IMF credits during a given year, 0 otherwise. Source: World Bank 1995.

Exchange Rate Flexibility. This variable indicates the extent to which exchange rates are flexible (1) versus fixed (0). If the IMF describes a country's exchange rate as "par value applied" or a "unitary effective... fixed rate" (1966–73), I code the case fixed; otherwise it is coded flexible. If the IMF describes a country's exchange rate as "not maintained within relatively narrow margins" (1974–82), I code it flexible, otherwise it is coded fixed. If the IMF describes a country's exchange rate regime as a "more flexible arrangement" (1983–98), I code it flexible, otherwise fixed. Source: IMF various years.

Openness. The measure is imports (total value of goods and services: sum of merchandise f.o.b., imports of nonfactor services, and factor payments at market prices in current U.S. dollars) plus exports (total value of goods and services; sum of merchandise f.o.b., exports of nonfactor services, and factor receipts at market prices in current U.S. dollars as a proportion of GDP. Source: World Bank 1995.

Democracy. The score (ranging from a low of 0 to a high of 10) denotes the degree of democratic institutions within each country. Scores are derived from "subjective codings of the competitiveness of political participation, the openness and competitiveness of executive recruitment, and the level of constraints on the chief executive" (Jaggers and Gurr 1995, 411). Source: Jaggers and Gurr 1996.

Balance of Payments/GDP. The measure is the current account balance (the sum of net exports of goods and nonfactor services, net factor income, and net private transfers as a percentage of GPD, before official transfers) as a proportion of GDP for each country for the period under observation. Source: World Bank 1995.

Current Account Volatility. This is the log of the standard deviation of current account balance as a proportion of GDP (defined above). Source: World Bank 1995.

Reserves/GDP. The measure is central monetary authority foreign exchange reserves (including official holdings of gold valued at London market prices, SDR holdings, reserve position at the IMF) in current U.S. dollars as a proportion of GDP. Source: World Bank 1995.

Average Reserves/GDP. The measure is reserves as a proportion of GDP averaged by country for the period under observation.

Reserve Volatility. This is the log of the standard deviation of reserves/GDP. Source: World Bank 1995.

Terms of Trade Volatility. This is the log of the standard deviation of the terms of trade index, which is the relative level of export prices compared with import prices, calculated as the ratio of a country's index of average export price to the average import price (1987 = 100). Source: World Bank 1995.

World Interest Rate Shock. The measure is U.S. Treasury bill rates, annual average, interacted with non-OECD countries. Source: IMF 1999.

GDP Growth. The GDP average annual growth rate is calculated for the sum of GDP at factor cost and indirect taxes, less subsidies. Source: World Bank 1995.

Regional Noncompliance. This variable is the proportion, calculated yearly, of current IMF members within each region who have committed to Article VIII but who place restrictions on current accounts. For classification by region, see regional norms above. This explanatory variable is used only to analyze policies of Article VIII countries, and is interpreted as noncompliance. Source: IMF various years.

Rule of Law. The measure is a six-point scale that determines where a country's domestic polity falls regarding high (6) or low (0) respect for the rule of law. This variable "reflects the degree to which citizens of a country are willing to accept the established institutions to make and implement laws and adjudicate disputes." Higher scores indicate "sound political institutions, a strong court system, and provisions for an orderly succession of power." Lower scores indicate "a tradition of depending on physical force or illegal means to settle claims." Upon changes in government in countries scoring low on this measure, new leaders may be "less likely to accept the obligations of the previous regime" (Knack and Keefer 1995, 225). Source: International Country Risk Guide

Bureaucratic Quality. A six-point scale measures the extent to which a country's bureaucracy is very capable (6) or incapable (0) of carrying out a range of administrative tasks. For a full discussion of the conceptualization of this variable,

see Knack and Keefer (1995). Source: International Country Risk Guide 2000.

Years since Last Restriction. A vector was created using the STATA routine made available on Richard Tucker's website (http://www.fas.harvard.edu/~rtucker/papers/grouped/grouped3. html). This is coded 0 if the country restricts or is in the first year of lifting a restriction; otherwise, the years of no restrictions are counted.

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Decentralization and Inflation: Commitment, Collective Action, or Continuity?

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o political and fiscal decentralization make it easier or harder to control inflation? Statistical analysis of average annual inflation rates in a panel of 87 countries in the 1970s and 1980s found no clear relationship between decentralization and the level of inflation. Political decentralization, however, does appear to reduce change in countries' relative inflation rates over time. By creating additional veto players, federal structure may "lock in" existing patterns of monetary policy—whether inflationary or strict. Among the (mostly developed) countries that started with low inflation, inflation tended to increase more slowly in federations than in unitary states. Among the (mostly developing) countries that started with high inflation, inflation tended to increase faster in the federations. There is evidence that political decentralization locks in a country's degree of practical central bank independence—whether high or low—and the relative hardness or softness of budget constraints on subnational governments.

he pros and cons of political and fiscal decentralization are vigorously debated, but little is known conclusively about its relationship to inflation. Different theoretical premises, supported by different empirical examples, imply opposite predictions. According to one view, inflation is essentially the result of a commitment problem. Policymakers have an incentive to renege on promises of stable monetary growth because unanticipated inflation has a positive real effect. Only if they can restrict their future actions will their promises be credible and a low inflation equilibrium achievable (Barro and Gordon 1983; Kydland and Prescott 1977). Partial devolution of control over spending or monetary policy to lower levels of government may be one way to restrict the center's ability to renege, and competition among subnational jurisdictions to attract investment may reduce the incentive for regional governments to renege (Qian and Roland 1998). In federal systems, different levels of government can police one another, and the central bank may be better shielded from political pressures (Lohmann 1998). Under this logic, decentralization should reduce inflation.

A second argument attributes inflation to a collective action problem. Stable prices are a public good that will tend to be underprovided when the number of actors who must agree to contribute is large (Samuelson 1954). Although the advantages of low inflation are felt by all, the public spending and money creation that cause it accrue to particular beneficiaries. The more

actors there are with leverage over fiscal or monetary policy, the weaker will be the incentive for each to exercise restraint. Under this logic, decentralization, by dividing up authority among different levels of government and increasing the number of relevant actors, is likely to increase inflation.

Examples that support the commitment view are easy to adduce. In Germany and the United States, some scholars have argued, federal institutions and strong subnational governments have helped discipline central economic policymakers and preserve central bank independence (Lohmann 1998; McKinnon 1997). In Germany, a majority of members of the Bundesbank's policy-making council are selected by the Land governments, giving the state governments direct influence over monetary policy. In the United States, although state governments do not have any direct role in monetary policy, the representation of regional reserve banks on the Federal Reserve's policy-making committee may help to ensure that the interest of some regions in loose money is balanced by the interest of other parts of the country in stricter policy. But examples that make the opposite case are also numerous. In various Latin American countries, political decentralization seems to encourage higher public spending, foster excessive public sector borrowing, and weaken the one actor with an encompassing interest in price stability, the central government (Campbell, Peterson, and Brakarz 1991). The hyperinflationary last years of Yugoslavia serve as a particularly clear illustration of the "dangers of decentralization" (Prud'homme 1995).2

A third theory suggests that decentralization will not directly affect inflation but will lock in relative inflation rates, whether high or low, by making it hard to change monetary or fiscal policies and institutions. A federal structure increases the number of veto players whose agreement is required for a change in the status quo,

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Decentralization is defined by various scholars in somewhat different ways. By "fiscal decentralization" I mean the allocation of greater revenue-raising authority and/or expenditure responsibilities to subnational levels of government. "Political decentralization" refers broadly to an increase in the scope of decision making over which subnational governments or legislatures have autonomous authority.

² During the 1980s, control over the Yugoslav central bank slipped progressively from a weakening federal government to the more assertive republics. This exacerbated pressures to inflate the money supply, which hastened the financial and political crisis that undermined the state (see, e.g., Treisman 1996).

and this should make current policy—whether inflationary or austere—more sticky (Tsebelis 1995). The macroeconomic effect of federal institutions will depend in this case on which initial policies are being reinforced. In countries with relatively high inflation, federal institutions will tend to perpetuate the fiscal pressures or politicized monetary system that cause the inflation, and durable stabilization is difficult to achieve. In countries with relatively depoliticized monetary policy and low inflation, federal structures will help perpetuate the underlying conditions.

Regardless of its influence on inflation, decentralization has much to recommend it. Politically, strong local governments are thought to encourage participation, foster civic spirit, and provide a check against central tyranny (Madison [1788] 1961; Tocqueville [1835-40] 1988; Weingast 1997). Economically, competition among subnational jurisdictions may lead to more efficient public good provision, and decision making can satisfy regionally diverse demands more precisely when it is decentralized (Oates 1972; Tiebout 1956). For these reasons, regimes that range from postapartheid South Africa, to postauthoritarian Latin America, to postcommunist Eastern Europe have placed decentralization high on their political agenda. These projects are "in the air" everywhere (Bird 1993); by one scholar's count, they have been adopted in about 85% of developing countries with a population of more than five million (Dillinger 1994). In the developed world, devolution of authority to the states has been a major theme in U.S. politics in recent decades, and Italy, France, and the United Kingdom have all embarked on major decentralization projects.

The benefits of decentralization are better understood than its economic or political costs. If its inflationary consequences are different in different settings, we need to know why and whether the negative ones can be avoided. This article examines that issue empirically. I analyze the relation between political and fiscal decentralization and average inflation rates in a broad sample of countries in the 1970s and 1980s, and I find support for the third view: Decentralization tends to lock in either relatively high or relatively low inflation. Whereas unitary states often change policies rapidly, going from high to low inflation and vice versa, decentralized political institutions make a change in macroeconomic policies, whether good or bad, more difficult. In some (mostly developed) countries, federal structure has helped entrench the practical independence of the central bank and low levels of central government lending. In other (mostly developing) countries, federal structure has served to perpetuate the political vulnerability of central banks and the tendency of central governments to bail out debtors.

In the next section I examine theories about the relationship between decentralization and inflation. I then present a statistical analysis. First, I show that the hypothesis that federal structure enhances the continuity of inflation rates is supported by the data, whereas neither the commitment nor the collective action theory is supported at all. Second, I outline a simple model of how different political and economic conditions as

well as aspects of the fiscal and monetary policy process affect inflation rates. I demonstrate that the key relationships postulated in the model are statistically significant in this data set. I then explore which aspects of fiscal or monetary policy—or political or economic conditions—are locked in by federal structure. Two variables that help determine inflation rates have significantly greater continuity over time in federal than in unitary states: the practical independence of central banks and the share of net lending in central government outlays. I interpret this as evidence that federal structure preserves the relative level of central bank independence, whether high or low, and the relative vulnerability of central government to demands for bailouts, whether high or low. In the last section I elaborate on the implications.

COMMITMENT, COLLECTIVE ACTION, AND CONTINUITY

Most economists and political scientists agree that price inflation is almost always a monetary phenomenon, caused by disproportionate increases in the money supply, and that high (although perhaps not moderate) inflation is both economically costly and politically unpopular. Yet, in scores of countries authorities expand the money supply at inflationary rates. Two views dominate the debate over why this is the case.³

The first attributes suboptimally high inflation to the inability of governments to commit credibly to monetary restraint. All governments press for a high level of monetary growth, regardless of the cost of inflation, for the simple reason that this is their dominant strategy (Barro and Gordon 1983; Kydland and Prescott 1977). On the one hand, if markets expect low inflation, then boosting the money supply has positive real effects; on the other hand, if markets expect high inflation, then it is still less costly in the short run to accommodate these expectations than to thwart them. High inflation occurs in equilibrium, although both markets and policymakers view it as an inferior outcome. Only some institutional mechanism that shields monetary policy from politics can stabilize prices. One that is frequently discussed is independence of the central bank, which has been found empirically to correlate with lower inflation rates.4

The second view associates high inflation with political fragmentation. The main idea is that monetary stability is a public good. If it is provided for one user of the currency, it is provided for all (nonexcludability), and one person's enjoyment of low inflation does not

³ A third set of models are not addressed here because their implications for decentralization are less clear. They attribute sub-optimally high inflation to asymmetric information and the competition among social actors to avoid bearing the costs of stabilization. The classic example is Alesina and Drazen 1991.

⁴ See Aleuna and Summers 1993; Cukierman 1992, Cukierman, Webb, and Neyapti 1991. Cukierman finds that in developed countries legal independence of the central bank is associated with lower inflation. In developing countries, legal independence appears to differ from actual independence, as proxied by a low frequency in turnover of the bank's governor. It is actual independence that significantly correlates with inflation.

diminish that of another (nonrivalness). How much monetary stability is provided depends on the extent to which each party with control over fiscal and monetary policy agrees to "contribute" by restraining its demands for increased public spending and newly created money.⁵ Public goods tend to be underprovided if the number of consumers is large and no single actor has an "encompassing interest" in providing them (Olson 1965). Weakening the central government's stake in lower inflation and increasing the number of other actors with leverage over policy should, by this logic, reduce the "amount" of price stability provided.

Empirically, there is some support for the view that too many fiscal cooks spoil the macroeconomic broth. Among OECD countries, those with more fragmented party systems and coalition governments tend to have higher public sector budget deficits (Grilli, Masciandaro, and Tabellini 1991; Roubini and Sachs 1989). Some scholars suggest that ethnic fragmentation also may reduce public good provision, inhibit financial system development, or have other undesirable economic consequences (Alesina, Baqir, and Easterly 1997; Easterly and Levine 1997). The argument in this work has been that political fragmentation increases the pressures on central policymakers to overprovide fiscal benefits and underprovide the public good of price stability.

These two perspectives have opposite implications for how political or fiscal decentralization should affect inflation. If inflation is caused by an inability of the central government to commit credibly to stable fiscal and monetary policies, then the subdivision of power may help. The checks and balances of a federal system may make it harder for central policymakers to renege on commitments, that is, boost deficit spending before elections or pressure the central bank to ease interest rates. At times, the autonomy of organizations (such as the central bank) can be enhanced by dividing authority over them among competing actors with conflicting interests, which hampers the ability of any one of them to dictate policy (Moser 1997; Persson, Roland, and Tabellini 1997). Dividing power over the central bank between central and subnational governments may strengthen the bank's ability to resist political pressures and increase the credibility of its monetary policy.

Lohmann (1998) argues that exactly such a decentralization helps explain Germany's low inflation in the postwar period. The independence of the Bundesbank was enhanced by the way it was embedded into the country's federal institutions. A majority of the bank's council members were appointed by the Land governments. Central and Land elections were staggered, and the parties dominating governments at the two levels often differed. The Lander were also represented in the Bundesrat, the upper house of parliament, which could veto changes to central banking legislation. These factors served to check attempts by central government to inflate the economy in order to buy popularity during electoral campaigns. Germany's inflation in this

period was among the lowest in Europe. Along these lines, Wildasin (1996, 324) argues that in South Africa "strengthening provincial and local institutions may create a credible institutional constraint on the exercise of the redistributive powers of the public sector."

If inflation results from a collective action problem, however, then the subdivision of power should make the situation worse. Increasing the number of (central and subnational) actors with influence over fiscal or monetary policy should exacerbate the collective action problem in the same way that increasing the number of parties in government or ethnic groups lobbying for benefits is thought to do. The central government is the only actor with an encompassing interest in price stability. Subnational governments have far less reason for restraint: The costs of inflationary spending are spread across all regions, but the benefits can be targeted to their constituents.6 Forcing the center to share power with partly autonomous subnational governments means that actors with more inflationary preferences are given a stronger say.7

In essence, these two arguments differ over the source of inflationary pressure. The commitment view places it with the central government. Because boosting the money supply is its dominant strategy, the central government prefers to inflate. The collective action view attributes the inflation motive to regional governments, which benefit from particularistic central spending programs whose costs are shared with other regions. Shifting the balance of power between central and regional governments will have opposite results depending on which of these assumptions about preferences is right. Of course, both may be correct, in which case decentralization will not have much effect on the inflation rate. And each may be correct in different settings, in which case the effect of decentralization is context dependent.

A more recent theory suggests a third type of relationship between political decentralization and inflation. Tsebelis (1995, 1999, n.d.) argues that major changes in central government policies will be rarer when the number of veto players—collective or individual decision makers whose agreement is necessary for a change in the status quo—is greater.8 Federal constitutions are a deliberate way to increase the number of veto players. They may provide subnational governments with the right to veto certain central policies within their jurisdiction and/or formal rights in central decision making, and they sometimes establish the courts as arbiter between levels of government, or chief veto player. Therefore greater continuity in monetary and fiscal policies—and in the relative inflation rates they generate, whether high or low-may be

⁵ For an application of this argument to China, see Huang 1996, chap. 9.

Weingast, Shepale, and Johnson (1981) show, in a similar vein, why the representatives of geographical constituencies in a legislature often favor excessive spending.

⁷ According to the World Bank (1997, 124): "Because decentralization increases the number of actors and of budgetary accounts, countries facing serious budgetary and inflationary pressures will be confronted with additional challenges and risks."

⁸ Franzese (1998) finds greater continuity in deficit and debt patterns in OECD countries that have a larger number of veto players.

expected in federal than in unitary states. In a sense, the commitment of the government to current policy is rendered more credible in this theory, but unlike the commitment view, no association is assumed between commitment and fiscally responsible policy. Bad policies are made more "sticky," too.9

Various contextual factors are thought to influence the way all three models work and complicate their implications for decentralization. Arguments, and in some cases empirical evidence, suggest that inflation rates may be affected by the level of economic development, political instability, import penetration, the exchange rate system, the political strength of the financial community, and the nature of the political regime (democratic or authoritarian). In the empirical analysis, I include indicators of these in a set of controls.¹⁰

The commitment, collective action, and continuity theories all appear to be based on valid logic, but their implications conflict. The more political power is shared among levels of government, the weaker will be the central government's capacity to inflate the economy unilaterally (the commitment argument); the weaker will be the government's ability to change the arrangements by which monetary and fiscal policy are determined, whether or not they are inflationary (the continuity argument); but the greater will be the capacity of subnational governments to press for inflationary central aid (the collective action argument).

When theories conflict, the most useful way forward is often an empirical approach. Should policymakers in inflation-prone countries decentralize political power and fiscal responsibility in the hope of insulating monetary policy? Or should they centralize power in order to avoid collective action problems in macroeconomic stabilization? Does the most auspicious strategy depend on whether they start from high or low inflation? To answer these questions, we need to know more about which logic predominates in particular settings. I turn now to an investigation of the actual patterns of

inflation in a broad selection of countries in the 1970s and 1980s.

EMPIRICAL ANALYSIS: DECENTRALIZATION AND INFLATION

Which of the various possible relationships between decentralization and inflation holds in practice? I analyzed average inflation rates of the Consumer Price Index (CPI) in 87 countries for four five-year periods in the 1970s and 1980s.11 The dependent variable is the log of average inflation over the five-year period, taken from the International Monetary Fund's (IMF) International Financial Statistics Yearbook, various years. The log is used, as in numerous other studies, to avoid giving excessive weight to observations of extremely high inflation; hyperinflation is widely thought to be generated by a nonlinear process. Averaging over five years also reduces the influence of episodic high inflation and seems appropriate since many of the independent variables change relatively slowly or are only available as multiyear averages (e.g., the indicators of central bank independence). The sample includes all countries for which data are available for the main independent variables.

The estimation strategy is least squares with dummy variables.12 Wherever possible, I use panel-corrected standard errors (pcse's) as recommended by Beck and Katz (1995) to correct for panel heteroskedasticity and contemporaneous correlation of the errors. In some cases, gaps in the data made it impossible to compute pcse's. In such cases, I corrected for panel heteroskedasticity and modeled the expected contemporaneous correlation directly by controlling for the average value of the dependent variable in all other countries (lagged one period to reduce problems of endogeneity). 13 To correct for serial correlation, I included a lagged value of the dependent variable, as recommended by Beck and Katz (1996).14 In each regression, I include three dummy variables for period (1975-79, 1980-84, 1985-89; the excluded category is 1970–74) and five dummy variables for region (Asia, Latin America and the Caribbean, sub-Saharan Africa, the Middle East and North Africa, Eastern Europe and the former Soviet Union; the excluded category is Western Europe and North America). I also experimented using different regional groupings to check that this did not affect the main results.15

Political decentralization is not the only way to increase the number of veto players: Various other constitutional devices may entrench the power of competing institutional actors. Also, the political fragmentation created by decentralization may be overcome in some cases by a cohesive, nationwide party that aligns the interests of actors in different power centers. Thus, one would expect a correlation but not a perfect correspondence of political decentralization with more veto players.

¹⁰ Some work suggests that inflation and other macroeconomic outcomes also may be related to a country's budget process and institutions. Various scholars believe that when the budget is formed by numerous spending agencies or levels of government submitting bids, which are then aggregated, fiscal discipline tends to be weaker than when the budget parameters are set by a central actor and then disaggregated downward (Alexina et al. 1996, Von Hagen and Harden 1996). No comprehensive indicator of cross-national differences in budget process is available, so I was unable to control for such differences systematically. A recent study provides comparative indicators for 20 countries in Latin America and the Caribbean (Alesina et al. 1996). It rates the relative power of finance ministries over other ministries in preparing the budget, parliament's authority to amend the budget, whether government regularly assumes the debt of public agencies, and whether state and local governments can borrow domestically without central restrictions. None of these correlates with federal structure.

¹¹ The choice of period was dictated by data considerations. Reasonably comprehensive information on central and local budgets is only available from the early 1970s onward.

¹² For a discussion of the technique, see Stimson 1985. The fixed effects for which I control are not for individual countries but for regions and periods.

¹³ I am grateful to Robert Franzese for suggesting this technique.
¹⁴ The analysis was done using STATA, with the xtpcse option. Beck and Katz (1995, 1996) show with Monte Carlo experiments that ordinary least squares (OLS) with panel-corrected standard errors, including a lagged dependent variable to correct for serial correlation, is generally preferable to either the Parks generalized least squares or Kmenta's approach to analyzing time-series cross-section data sets.

¹⁵ In particular, I tried using instead the IMF classification of

As a preliminary check, I first tried to replicate in this data set the results of various works on the causes of inflation. Different authors find evidence that inflation is reduced by greater central bank independence (Alesina and Summers 1993; Cukierman 1992; Cukierman, Webb, and Neyapti 1992), higher exposure to imports (Romer 1993), and pegged exchange rates (Ghosh et al. 1995; Giavazzi and Pagano 1988) and is increased by political instability (Cukierman, Edwards, and Tabellini 1992; Cukierman and Webb 1995; Edwards and Tabellini 1991).

Regressing log inflation on the region and period dummies along with a measure of each of these, I was able to replicate three of these findings (at least in regressions that did not include other controls). First, using the indices of Cukierman, Webb, and Neyapti (1992), I confirmed their finding that central bank independence is linked to lower inflation.16 Their indicator for the practical independence of central banks infrequent turnover of the bank's chief executive—is significantly associated with lower inflation (at p <.01).17 Second, I replicated the Ghosh et al. (1995) finding that countries with pegged exchange rates tend to have lower inflation than those with floating rates.¹⁸ Third, I found some limited support for the claim that inflation is higher in more politically unstable countries. I tested three indicators of political instability: (1) whether the country underwent a war with an external power during the decade, (2) whether it underwent a civil war, and (3) the number of revolutions or coups during the decade. 19 No indicator was significant in the complete sample, but civil war was significantly associated with higher inflation among the OECD countries.20 I did not find significant evidence that inflation was lower in countries with a higher share of imports in GNP; the coefficient was negative as expected, but the p level was only .16. This may, of course, be because the regression lacked appropriate controls.

I also tested for evidence in support of two other hypotheses. Posen (1995) and Cukierman (1992) argue that the strength of the financial sector should correlate with lower inflation because banking interests lobby for conservative monetary policy. I used two

countries. This grouped all industrialized nations together and grouped developing countries by location in Asia, Africa, Europe, the Middle East, and the Western Hemisphere. When this yields slightly different results, I so indicate in footnotes.

¹⁶ Since the indices of central bank independence are calculated only by decade, I use the same values for the two halves of each decade.
¹⁷ I did not find their indicator of the *legal* independence of central banks significant in any regression.

¹⁸ The variable I construct from their annual ratings takes the value of 3 for a pegged exchange rate system, 2 for an intermediate system, and 1 for a floating system. I use the average annual rating for the given five-year period. This variable may well be endogenous, so the question can still be raised whether pegged exchange rates reduce inflation, or high inflation prompts the adoption of pegged exchange rates.

¹⁹ The revolution or coup variable is the main one used to measure political instability in Barro 1991. All variables were taken from Easterly and Levine 1997. Data for war and civil war were compiled by Sivard (1993); the coups and revolutions variables were compiled by Banks (1994).

²⁰ This is entirely attributable to the high inflation of Turkey, which experienced civil war in the 1970s and 1980s

indicators of banking sector development employed in previous studies: total claims of deposit banks as a share of GDP and total claims of deposit banks on the private sector as a share of GDP (Demirgüç-Kunt and Levine 1995; Levine and Zervos 1996).²¹ I found that banking sector development is significantly correlated with lower inflation, using either indicator. The effect loses significance, however, if one controls for GDP per capita, so it is not entirely clear whether banking sector development or some other aspect of economic development leads to lower inflation rates.²²

In recent decades scholars have debated whether the type of political regime, democratic or authoritarian, affects the government's ability to control inflation. Some suggest that democracies are more vulnerable to popular pressures for inflationary government spending (e.g., Skidmore 1977), whereas authoritarian regimes are able to repress social forces in the interest of financial stability (e.g., Haggard and Kaufman 1992; O'Donnell 1973). Significant doubts—both theoretical and empirical—have been raised about the latter argument (e.g., Geddes 1995), but it seemed worthwhile to test whether democracies tend to have higher inflation rates than other regimes. I used as a measure the number of years in the relevant five-year period that the country was democratic.23 Democratic government turns out to be significantly related to higher inflation, controlling for economic development.

To test for the effects of political and fiscal decentralization, I used three admittedly imperfect indicators. The first is whether the state is classified by scholars as federal. I relied on Elazar (1995), but because my data end in 1989 I do not count as federal either Belgium or Spain, which some experts consider federal only in the 1990s. Political scientists debate the finer points of a definition of federalism, but there is basic agreement on the primary characteristic: a constitutionally guaranteed division of power between central and regional governments (see, e.g., Lijphart 1984, 170). In Riker's (1964, 11) formulation, a federal constitution has (at least) two levels of government for the same land and people; each level has "at least one area of action in which it is autonomous"; and this autonomy must be guaranteed in the constitution.²⁴

²¹ I chose the figure as of 1970 so as to minimize the danger of reverse causation—high inflation reducing the size of bank deposits. The variables were lines 22a-d and 22d, respectively, from IMF International Financial Statistics Yearbook, 1987.

²² The effect is also insignificant if the IMF's regional classifications—which group all industrialized countries together—are used for the regional dummies. Posen (1995) conjectures that federal states tend to have more developed banking sectors. This turns out to be true within the OECD. Among non-OECD states, however, the relationship between banking sector development and federal structure is negative (although not significant).

²³ The classifications were those of Alvarez et al. (1996).

²⁴ Obviously, constitutional guarantees are not always observed, so a constitutionally defined indicator of decentralization needs to be supplemented by alternative measures (such as the fiscal ones I also use) and the results checked for robustness.

	Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Federal status	.03 (.07)	00 (.08)				
Subnational spending share (%)			.001 (.003)	- 001 (.003)		
Subnational tax share (%)				ı	.001 (.003)	.001 (.002)
Controls Log average Inflation previous period	.77** (.25)	.65*** (.17)	.72*** (.15)	.63*** (.12)	.77*** (.19)	.68*** (.12)
Turnover of central bank executive		. 26 (.17)		.31* (.15)		.07 (.08)
Central bank legal Independence		05 (.11)		19 (.10)		17 (.11)
Log GDP per capita		05 (.16)		.15* (.06)		.12 (.10)
Fixed exchange rate		06 (.05)		02 (.04)		.00 (.04)
Imports (% GDP)		00 (.00)		00 (.00)		01 (.00)
Civil war in decade		.07 (.07)		.09 (.13)		.05 (.08)
Number of revolutions or coups during decade		.10 (.07)		.28 (.33)	,	.27* (.12)
Bank lending to private sector		003 (.002)		004* (.002)		006* (.003)
Years democratic during the period		.04 (.02)		.02 (.02)		.01 (.01)
Constant	.41 (.24)	.73 (.51)	.41* (.19)	.27 (.18)	.42* (.21)	.42 (.33)
R ²	.5012	.5956	.5348	.7026	.5862	.6962
Wald χ^2	392.43	4.87	153.81	353.98	471.22	3,473.98
p<	.001	.31	.001	.001	.001	.001
N	319 ·	222	178	123	1 96 .	139

Note: The dependent variable is the log average annual change in the CPI for successive five-year periods. Estimation is by least squares durminy variables; panel-corrected standard errors are in parentheses (adjusted for heteroskedastiotty and contemporaneous correlation). p < .05, p < .01, p < .00. For data sources, see the Appendix. To calculate panels for regressions in models 4, 5, and 6 it was necessary to exclude cases for which there were fewer than two observations available. For subsequent and/or previous period N varies across models because of gaps in the data for some variables. Each regression also includes durmines for period and for region (Asia, Latin American and Caribbean, sub-Seharan Africa, Middle East and North Africa, Eastern Europe and the former Soviet Union; the excluded outegory is Western Europe and North America). Coefficients on these (not reported) are available from the author upon request.

This is the definition I use.²⁵ It fits with Elazar's (1995) classification of countries.

The second and third indicators are the share of subnational governments in total government spending (compiled mostly from the IMF Yearbook, data taken as close to the midpoint of the five-year period as possible) and in total tax revenues. These fiscal variables have the advantage of being continuous, compared to the dichotomous federal structure dummy. All three variables are quite highly correlated in the country-years for which data are available (the two fiscal variables with each other at .90, both with federal status at .59).

Table 1 shows the regression of log inflation on federal status, subnational spending share, and subnational tax share for the full sample of country/periods.

All regressions include the already mentioned regional and period dummy variables as well as a one-period lag of log inflation. In each case, I show two models; one includes a set of controls based on the studies discussed above, and one does not.²⁶ None of the decentralization variables have noticeable effects, with coefficients close to zero. Based on this alone, it may seem that political or fiscal decentralization has little to do with inflation.

In Table 2 are test results for the third theory, that decentralization is not associated with either high or low inflation per se, but with greater continuity in either because of the perpetuation of policy and institutions. In other words, federal states with relatively low or high inflation are more likely than similar unitary states to remain in that condition, whereas unitary states with low inflation are more likely than

²⁵ Both India and Pakustan classify as federations under this definition. Article 26 of India's constitution stipulates that states have an exclusive right to legislate on any of a list of areas, including the state courts and local governments. Pakistan's constitution (Article 142) gives provincial assemblies exclusive authority to make laws on any matter not covered by two lists included in the constitution.

²⁶ Log per-capita GDP should capture in part the greater dependence of less developed countries on seignorage (the profit to the state from money creation) because they lack effective tax collection systems.

	Federal	Status	Subnational Sp (9)		Subnational T	ax Share (%)
				lodel		
Decentralization Indicator	(1)	(2)	(1)	(2)	(1)	(2)
Decentralization (measures vary as Indicated In column headings)	28*** (.07)	34 (.18)	011** (.004)	016 ** (.005)	020*** (.003)	021*** (.004)
Decentralization × log Inflation one period earlier	.32*** (.05)	.34* (.14)	.012** (.004)	.014*** (.004)	.020*** (.002)	.020*** (.003)
Log Inflation one period earlier	.71** (.25)	.57** (.18)	.51*** (.12)	.45*** (.11)	.44*** (.10)	.39** (.13)
Controls Turnover of central bank executive		.12 (.18)		.09 (.17)		.13 (.21)
Central bank legal independence		.12 (.15)		.17 (.10)		.05 (.10)
Log GDP per capita	3	05 (.15)		.12 (.12)		.17 (.12)
Fixed exchange rate		06 (.05)		04 (.03)		03 (.04)
Imports (% GDP)		00 (.00)		00 (.00)		004*** (.001)
Ctvll war in decade		.07 (.07)		.28 (.15)		.07 (.06)
Number of revolutions or coups during decade		.11* (.05)		.08 (.28)		.35 (.44)
Bank lending to private sector		003 (.002)		00 (.00)		003*** (.001 <u>)</u>
Years democratic during period		.04 (.02)		.05 (.04)		.01 (.01)
Constant	.48* (.24)	.77 (.52)	.67*** (.15)	.30 (.19)	.76*** (.11)	.42 (.25)
R²	.5132	.6091	.5687	.6506	.7168	.7769
Wald χ^2	326.22	162.33	100.10	101.45	450.87	122.05
p<	.001	.001	.001	.001	.001	.001
N	319	222	178	. 140	172	129

Note The dependent variable is the log average annual change in the CPI for successive five-year periods. Estimation le by least squares dummy variables; panel-corrected standard errors are in parentheses (adjusted for heteroekedastion) and contemporaneous correlation) p < .05, p < .01, p < .00. The data sources, see the Appendix. To calculate pose's for regressions in models 5 and 8 it was necessary to exclude cases for which there were fewer than two observations available. For submational tax share data, figures were also interpolated when data were available for subsequent end/or previous period. N varies across models because of gaps in the data for some variables. Each regression also includes dummies for period and for region (Asia, Latin American and Caribbean, sub-Saharan Africa, Middle East and North Africa, Eastern Europe and the former Soviet Union; the excluded category is Western Europe and North America). Coefficients on these (not reported) are available from the author upon request.

similar federal states to let the rate slide upward, and unitary states with high inflation are likelier than similar federal states to stabilize successfully. To test this, I include in the regressions in models 1 and 2 a term for federal structure and an interaction term for federal structure and log inflation in the previous period. (As before, I also control for the log of average inflation in the previous period.) If federal structure increases the continuity of either relatively low or relatively high inflation, then the interaction term should have a positive coefficient, and the coefficient on the federal dummy should be negative. If so, this means that inflation for a federal state (I_{tV}) , given low levels of

previous inflation, but higher than inflation for a unitary state, given high levels of previous inflation.

The reason is easy to demonstrate formally. Inflation is modeled as an exponential growth process. For federal states: $I_{tF} = a_p I_{(t-1)F}^{\ \ \ \ \ \ \ \ }^{bF} + [\text{other factors}]c$; for unitary states: $I_{tU} = a_U I_{(t-1)U}^{\ \ \ \ \ \ \ \ \ \ }^{bU} + [\text{other factors}]c$ (a_F represents the coefficient and bF the exponent on previous inflation for federal states; a_u and bU are the corresponding coefficient and exponent for unitary states; c is a vector of coefficients on other factors). My hypothesis is that at low levels of I_{t-1} , $I_{tF} < I_{tU}$, but at high levels of I_{t-1} , $I_{tF} > I_{tU}$. This will be the case if two hypotheses are both true: H1, bF > bU; H2, $a_F < a_U$. The equation estimated statistically is the following:

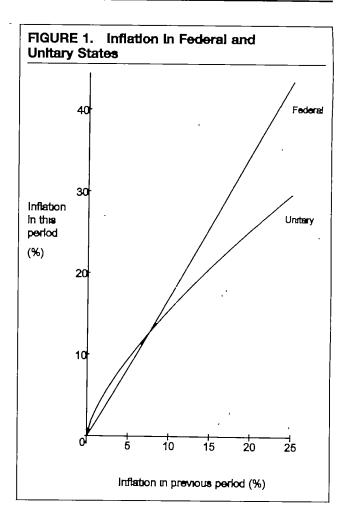
 $\log(I_t) = \alpha + \beta_1 \text{federal} + \beta_2 \log(I_{t-1}) + \beta_3 [\text{federal} \times \log(I_{t-1})] + [\text{controls}] \gamma + \epsilon_r$ From the estimates, we can derive: $a_F = 10^{(\alpha + \beta_1)}$; $a_U = 10^{\alpha}$; $bF = \beta_2 + \beta_3$; $bU = \beta_2$. H1 can be rewritten as $\beta_2 + \beta_3 > \beta_2$, or $\beta_3 > 0$; and H2 yields: $10^{(\alpha + \beta_1)} < 10^{\alpha}$, or $\alpha + \beta_1 < \alpha$ and $\beta_1 < 0$. This proved to be the case. Both the interaction term and the federal structure dummy are significant, or very close to significant, with the expected signs, whether or not the controls are included. The other columns of Table 2 show that this relationship also holds when subnational expenditure or tax share is used as the indicator of decentralization.

These findings provide some strong support for the continuity hypothesis. In general, average inflation rates tended to rise during the 1970s and 1980s in both unitary and federal states. Although there was a general upward drift, the rise was less in federations with low inflation in the previous period compared to similar unitary states, and the rise was greater in federations that started from high inflation compared to similar unitary states. The regression suggests a cutoff point around 7.5%. Federations with inflation higher than this five years earlier tended to have a faster rise than similar unitary states, and those with inflation lower than 7.5% five years earlier tended to have a slower rise than similar unitary states.²⁹ Figure 1 graphs this estimated relationship.

Another way to make the same point is to consider the correlations between current and earlier inflation. Among unitary states, the correlation is a respectable .55. Among federal states, it is .82. There is also a far tighter relationship in federal states between average inflation rates across long periods. The correlation between the log of average inflation in 1985–89 and in 1955–59 is .58 for unitary states but .93 for federations. The relationships are shown in Figure 2A and B.

I performed some additional checks in order to be sure that the results were not sensitive to the coding of certain countries on the federal dummy. For instance, some federations in the developing world are less politically decentralized than others. In India, Pakistan, Venezuela, Nigeria, and former Yugoslavia, state governors are appointed by the center rather than elected or selected locally. In Malaysia, some state governors are federally appointed and others inherit the office. I excluded these cases from the sample (both with and without Malaysia) and ran the regressions in Table 2

The estimates imply that a unitary state with milation above about 45% will on average see inflation decrease in the following period at milation (I_{t-1}) of 45%, the estimates from Table 2, model 1, with decentralization set at zero, imply that $I_t = I_{t-1}$. The estimates for federal states (without controls) suggest a tendency for inflation to increase on average from all starting points above zero.



(both with and without the controls). The results were very similar and often stronger than those reported in the table.³⁰

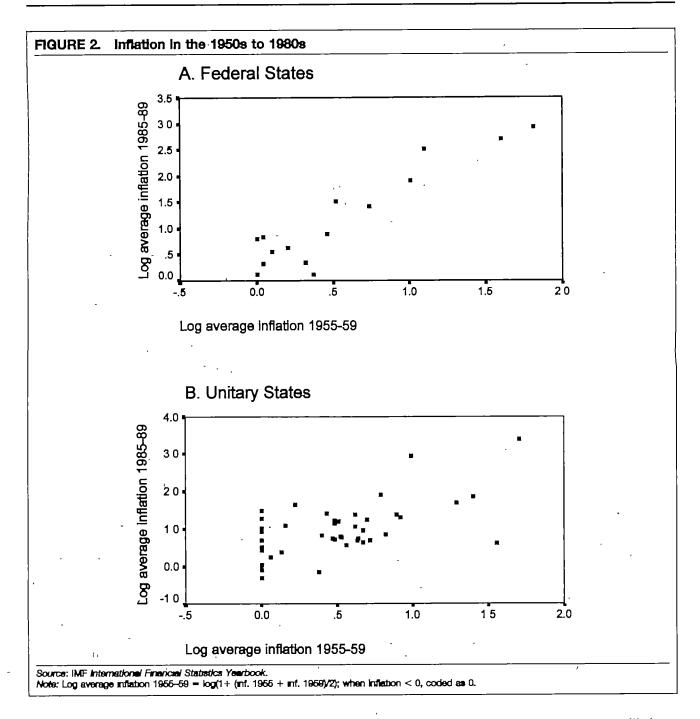
Since the rest of the article focuses on this result, it is worth repeating that it holds regardless of which of these three measures of political or fiscal decentralization is used. Each is imperfect in its own way. Coverage for the fiscal variables is much poorer, and the data may contain considerable measurement error. Furthermore, states that call themselves "federal" are not by any means equally decentralized politically. The fact that the same result appears regardless of indicator adds some confidence in the finding. In most of the analysis below, I use federal status as the primary decentralization indicator, since that information is available for a far larger number of cases than data on either of the fiscal variables, but where relevant I also use the other indicators.

²⁷ The estimated relationship for unitary states: $Log(I_t) = .71Log(I_{t-1}) + .48$, which yields $I_t = 3.02I_{t-1}^{-71}$. For federal states it is: $Log(I_t) = 1.03Log(I_{t-1}) + .20$, which yields $I_t = 1.58I_{t-1}^{-1.03}$.

²⁸ Including the controls, the federal dummy just misses the conventional .05 level of significance (p < 06).

²⁹ The 7.5% cutoff is implied by the estimated regression coefficients not including controls. Controlling for log per-capita GDP, central bank independence, openness to imports, exchange rate system, political instability, size of the banking sector, and democracy, the cutoff is around 10%. Obviously, these should be taken as only approximate estimates.

³⁰ One reviewer wondered whether the results might be influenced by the coding of India, Pakistan, and Germany as federal. He considers these far more centralized than "federal" suggests. I ran the regressions in Table 2 (using both, the dichotomous federal dummy and the continuous fiscal variables, both with and without controls) excluding Germany, Pakistan, and India. The results were virtually identical.

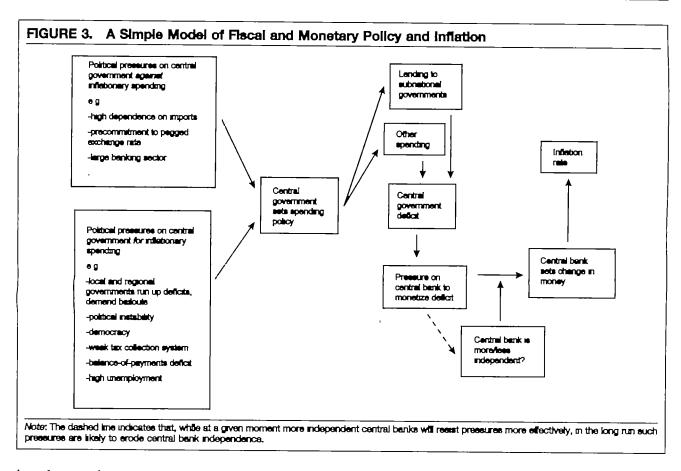


TRACING THE MECHANISMS

What does political decentralization perpetuate? What explains the persistently high inflation in some federal states (Argentina, Brazil, Mexico, Nigeria, and Yugoslavia) and the persistently low inflation in others (Austria, Canada, Germany, Malaysia, Switzerland, and the United States)? To answer this question, I proceed in two stages. First, I sketch a very simple model of how particular economic and political conditions as well as fiscal and monetary policies translate into inflationary outcomes. I show that the relationships assumed by this model are consistent with the evidence available in this data set. Second, I explore what elements in this policy process model are more persistent in federal than in unitary states. That con-

stancy may explain why inflation rates are likely to remain either relatively high or relatively low in federations over long periods.

Figure 3 outlines a simplified model of the causes of inflation. No claims for originality or completeness are made; the goal is merely to distill some of the important relationships assumed in the economics and political science literature. First, various economic and political conditions create pressures on the central government to indulge—or not indulge—in deficit spending. For example, strong regional and local governments that run up deficits and demand bailouts are one source. Political instability may prompt central politicians to spend unsustainably in the hope of retaining office. Some argue that the need to attract votes



in a democratic system may encourage incumbents to inflate the economy. By contrast, import dependence is a source of inflationary restraint, as spending might depreciate the currency and drive up import prices. Also, if a central government commits to keeping inflation low, perhaps by announcing a pegged exchange rate, the desire to maintain credibility is an incentive not to renege. Large and politically powerful financial sectors are also likely to lobby for financial stability.

These various pressures affect the fiscal policy choices of central government. They help determine how much the government spends, what proportion of the budget is devoted to lending (e.g., bailouts for subnational governments), and the size of the deficit. The larger the national deficit, the greater is the pressure from government on the central bank, ceteris paribus, to finance it through money creation. The degree of independence of the central bank will determine its resistance to increasing the money supply, which in turn affects the inflation rate.

Many other sources of pressure and possible links are excluded from this simplified outline. Furthermore, isolating the importance of particular links in the causal chain is extremely difficult (both theoretically and empirically), since each actor is strategic and makes choices based on expectations about how others will act at other points. Keeping these caveats in mind, the model provides a framework for exploring inflation performance in federal states.

In the following section, I seek empirical confirmation of the links in the model. Do the hypothesized pressures encourage central government spending, lending, and deficits? Does this lead, when central bank independence is sufficiently low, to an increase in the money supply? Does that lead, as the quantity theory of money predicts, to rising inflation? The empirical confirmation is partial, since data are not available for some variables in enough periods to permit causal modeling.³¹ Instead, I will only test for the predicted controlled correlations with time-series cross-section regressions, as before. Inferences about the direction of causation must be tentative.

Then I will test whether the greater policy continuity in federal compared to unitary states remains as one pursues the data backward along the modeled causal chain. Does federal status lock in particular monetary policies, the degree of central bank independence, the size of national deficits, the central government's level of spending and net lending, or specific political and economic pressures on central governments to spend more or less?

The Sources of Inflation

To assess how well the model depicted in Figure 3 fits the evidence in this data set, I ran a series of regres-

⁵¹ For instance, the Cukierman, Webb, and Neyapti (1992) measures of central bank independence are only available for (roughly) ten-year periods. Even using such data, Cukierman (1992) finds evidence of Granger causality running both from high central bank independence to lower inflation and from higher inflation to lower central bank independence. It was not realistic to attempt aimilar causal modeling here since data on the key fiscal variables are available only since 1970, which permits the use of only two of Cukierman's decade indicators of central bank independence.

sions to estimate the effect of each "upstream" variable (economic or political conditions, fiscal or monetary policies) on "downstream" variables (e.g., the inflation rate). The goal was to trace the hypothesized causal chain backward, from inflation rates to the monetary and fiscal policies believed to generate them. In each case, I controlled for all variables assumed to cause the explanatory variable in question, but not for those assumed to be caused by it. (For example, I control for central expenditure in a regression to assess the contribution of money growth to inflation, but not for money growth when estimating the effect on inflation of central expenditure levels.) Thus, the coefficients should be interpreted as estimating the additional contribution of the key explanatory variable in determining the dependent variable. In all cases, the "causally prior" control variables are those previously used: the share of imports in GDP, the exchange rate system, civil wars, revolutions or coups, log per-capita GDP, the size of the banking sector, and democracy. Each regression also includes region and period dummies as well as a one-period lag of the dependent variable.³²

The results, shown in Table 3, provide clear evidence for most of the links in the model. The first nine columns trace the causes of inflation backward along the hypothesized chain. (In each regression, the key explanatory variables are in boldface, to distinguish them from the causally prior controls.) First, I confirmed the quantity theory of money in this data set (column 1). Even controlling for central bank independence, central and subnational deficits, central spending, and the share of net lending in central outlays, higher money growth is very significantly related to a faster rise in inflation (coefficient of .65, significant at p < .01).³³ (Since the dependent variable is inflation controlled for its previous level, it makes sense to interpret this as a measure of the increase in the average inflation rate.) Second, as already replicated in an earlier section, the frequency of turnover of central bank chief executives (indicating lower independence of the central bank in practice) is positively associated with a faster rise in inflation (column 2).34

Are large national deficits associated with higher inflation? The estimated coefficient (.004), while positive as predicted, is not significant. Experimentation with an interaction term clarifies the relationship (see column 4). A large deficit when the central bank is very independent is not necessarily inflationary, but when the bank has little independence (i.e., frequent turn-

over of chief executive), a large deficit is significantly associated with a faster rise in inflation.³⁵ The politically weak bank cannot resist the government's pressure to increase the money supply.

Central governments that devote a large share of outlays to net lending tend to experience significantly larger jumps in inflation (column 5: coefficient of .02, significant at p < .01). Central governments with higher spending as a percentage of GDP tend to have larger deficits (column 11: coefficient of .24, significant at p < .01). When the central bank is less independent, higher central spending is strongly associated with a rapid rise in inflation (column 7: coefficient of .015 on interaction term), but this is not the case when the bank is more independent. Finally, larger subnational deficits are accompanied by greater increases in central government expenditure (column 14); an additional 1% of GDP in subnational budget deficits is associated on average with central government layouts about 1.65% of GDP higher.36 If the central government devotes significant resources to lending, sizable subnational deficits also lead to larger increases in the national deficit (column 13: coefficient of .10 on interaction term).37 When the central bank is less independent, subnational deficits also are associated with higher inflation (column 9: coefficient of .83 on interaction term).

The evidence in Table 3 suggests a plausible account of how inflation rates are determined. Fiscal pressures percolate upward through the system as expected; subnational deficits stimulate central spending, lending (presumably, in part, for bailouts), and deficits, which in turn stimulate money growth and inflation. These effects are moderated—and perhaps can even be eliminated—by a central bank that is in practice independent of government. Whether or not subnational deficits lead to central government deficits appears to depend on the extent to which the central government is in the business of extending loans.

This is a picture painted with broad strokes. The use of five-year averages limits the ability of regressions to fill in finer details, which remain to be explored with annual data, different specifications of the variables, and different lag structures. My goal is not to test each hypothesis as conclusively as possible but merely to establish empirical support for a model with which to explore the effects of political decentralization.

Two intriguing questions are left for future study. First, an independent central bank appears to act as a

³² Ideally, the regressions would also have controlled for unemployment, balance of payments, and quality of the tax collection system. Difficulties collecting systematically comparable data precluded this, however. Per-capita GDP should correlate with more effective tax collection and is included partly for that reason. Because neither unemployment nor the balance of payments is likely to correlate with decentralization, the results will not be biased.

^{33 &}quot;Money" here is demand deposits plus currency outside banks.
34 In this case, I am controlling for central and subnational deficits, central spending, and net lending share. It is possible that low central bank independence is not the cause of inflation but an effect of fiscal pressures that are themselves the true cause of high inflation (i.e., the dashed arrow in Figure 3). Yet, the significant negative relationship between central bank independence and inflation in Table 3 when indicators of such fiscal pressures are controlled suggests that central bank independence probably does have some direct effect.

³³ The significance of the interaction terms of fiscal variables with central bank independence also suggests that the influence of central bank independence on inflation will vary with the fiscal context (and underlying political factors). A given level of bank independence has a greater inflation-reducing effect when fiscal and political pressures to spend are weak. This point is demonstrated convincingly in Franzese 1999.

³⁶ Fornasari, Webb, and Zou (1998), analyzing annual data for 32 countries in the 1980s and early 90s, also find a significant relationship between subnational deficits and higher central government expenditures.

⁵⁷ Fornasari, Webb, and Zou (1998) find a significant relationship between subnational and central deficits and that this effect is larger in countries with less independent central banks.

								Dependent Variable	rlable						
					Log Infertion						Central Government Deflott	ment Defot		Central Govt.	Centra Govt
					<i>'</i> 			Model							
Key Policy Vanable	(£)	Ø	63	£	9	Đ	E	9	69	(10)	(11)	(12)	(13)	41	HB.
Log money growth	,06 (13)									,					
Central benk executive turnover	4 1.	9 <u>2</u>													
Central government deflort (% GDP)	014**	900)	700 7003	600 7					ı						
Central gov*t															
central bank exec. turnover				90; 89											
Net central government lending (% total central	P. (8	8	8	1					8					
Central government sounds (%GDP)	8 8 8	8 0 5	8 8 8	8 9 5	8 8 8 1 8	8, 8	000			§ 8 8	4 5				
Central gov't, apending × central bank exec. tumover		}	<u>}</u>	3	(C)	. .	1 1 1 1 1 1 1 1 1 1			00	§				
Subnational government defioit (% GDP)	\$. 90: -	i. 2 <u>5</u>	90. 60.	8 8	007	8 8	ਬ §	16***	8 0.–	8; 8	E .	8	1.06	1.
Subnet gov't deficit × central benk exec. turnover	Ţ		Į	Ĵ.	ĺ	3	()	1	1. 1. 1. 1. 1. 1. 1. 1. 1. 1. 1. 1. 1. 1	(f) (f)	જે	1	<u>¶</u>	<u>§</u>	3
Subnet gov't, deflort × net central gov't lending			-								ı		.10		
£.	986	,8394	8228	.8453	8248	7382	.7581	.6961	7684	.6175	9. 20.	4818	{ }	8371	6758
_z ∕ ppe,M	2,131	7117	450	28	4	7	08 2	234	374	55	<u>‡</u>	88	119		8
2	88	8	88	8	8	8	ç	,	,	í					;

Note: All regressions include the region and period durinties, a one-period lag of the dependent variable, and the following controle; log GDP per capita, pegged exchange rate, importa/GDP, cwil wer, revolutions or coups, acts of the native which is a defendent variable of the operation of the variable of the contract variable of the variable of variab

circuit breaker between fiscal pressures and inflation, but we do not know for sure what causes practical independence to be higher in some countries than others. Greater legal independence of the central bank is clearly not sufficient; in the Cukierman, Webb, and Neyapti (1992) study indexes of practical and legal independence of central banks are not positively correlated (r = .02), whether or not one controls for log per-capita GDP). A complete answer requires much additional research, but this data set offers some clues about where to look. As suggested in previous work, political instability is likely to undermine the bank's independence (Cukierman and Webb 1995), as is a large outstanding domestic currency debt, which the central government will seek to devalue by pressing the bank to inflate the currency. Fiscal pressures—and the inflation they stimulate—may themselves over time erode the bank's independence.38

As a preliminary test of these hypotheses, I regressed the Cukierman, Webb, and Neyapti (1992) index of the frequency of central bank executive turnover (decade averages for the 1960s through 1980s) on regional and period dummies, lagged executive turnover, legal central bank independence, log average inflation rates lagged one decade (as recommended by Cukierman 1992), revolutions or coups, civil war, and the domestic debt of central government as a percentage of GDP at the beginning of the decade. The results, presented in Table 4, strongly support the conjecture that political instability reduces central bank practical independence (the more revolutions or coups occur, the greater is the turnover of central bank governors) and weakly support the notion that governments with a large domestic currency debt are more likely to pressure the central bank to inflate it away (the coefficient is positive, as predicted, but only significant if one does not control for per-capita GDP). There is little evidence that inflation ten years earlier undermines central bank independence, although there might be a stronger effect with a different lag structure. I was able to reproduce Cukierman's (1992, chap. 20) finding that lagged inflation is related to central bank governor turnover when controls are not included, but the significance of the effect falls when region and period dummies are included and when other controls are added.

The second intriguing question is why different governments devote different shares of total outlays to net lending. When governments lend more, subnational deficits appear to get pushed upward, eroding central budget balance and worsening inflation. Central governments' lending share thus serves as a kind of proxy for soft budget constraints. But what causes this? Why, for instance, did Brazil and Egypt devote more than 10% of outlays to net lending in 1987, whereas Mexico and Romania had a net lending balance of almost zero? We do not yet know, although the literature offers plausible conjectures. More cohesive national parties may give central politicians greater leverage over regional officials demanding loans (Ordeshook 1996; Riker 1964), as may a constitution in which central government has the right to dismiss regional officials.³⁹ Interregional inequality may influence the strength of pressures on the central government to extend loans to poorer regions. Data are not available to test such propositions at this point, but that is a priority for future work.

What Does Federal Structure Perpetuate?

If the model of the causes of inflation sketched above accords with the evidence, then what aspect of the process does federal structure make more durable? Political decentralization may induce stickiness at several points. It may lock in either a high or low level of central bank independence by increasing the number of veto players who can block changes in the system of appointing bank members or the statutory definition of its role. It may increase intertemporal continuity in the size of central or subnational deficits by making it difficult to change status quo spending levels. It may lock in the level of central government lending, which in this data set seems to be a proxy for the readiness to bail out subnational units. Finally, it may lock in a particular level of exposure to imports or some other economic or political determinant of inflation.

To test each of these hypotheses, I ran a series of regressions of the following form: $\Delta X_t = \alpha + \beta_1 X_{t-1}$ + β_2 Federal + β_3 (Federal $\times X_{t-1}$) + $\beta_4(\Delta X)_{t-1}$ + β_5 regional dummies + ε_r . For each possible determinant of inflation, X, I regressed the change in that variable since the last period, ΔX_p on the lagged value of that variable, X_{t-1} , expressed as the deviation from the mean. I included an interaction term for federal structure and the lagged value of the main variable, Federal $\times X_{t-1}$, as well as the federal dummy and the region dummies. To correct for serial correlation, I included a one-period lag of the change in the main variable, $(\Delta X)_{t-1}$. As before, I used panel-corrected standard errors where possible, and where not I modeled the expected contemporaneous correlation directly and corrected for panel heteroskedasticity. The results are given in Table 5.

If the variable in question adjusts after a shock by regressing toward the mean, one would expect a negative coefficient on the lag of the policy variable in these regressions. Because this variable is expressed as the deviation from the mean, it will be negative when relatively low and positive when relatively high. A negative coefficient on the lagged policy variable means that the adjustment will be upward if the variable is relatively low (a negative coefficient times a negative variable produces a positive change), but downward if the variable is relatively high (a negative coefficient times a positive variable produces a negative change). The larger the size of the negative coefficient the faster will be the adjustment. The interaction term is included to test whether adjustment is significantly different in federal than in unitary states. If the variable adjusts more slowly—i.e., is stickier—in federal than in unitary states, one would expect a significant positive coefficient

³⁶ These hypotheses are found in Cukierman 1992.

³⁹ Dillinger and Webb (1998) see this as an important difference between Argentina and Brazil.

on the interaction term. That is, the positive coefficient on the interaction term when added to the negative coefficient on the lagged policy variable will reduce its size and slow down the pace of adjustment.

Of the seven variables examined in Table 5, two fit the pattern—a significant negative coefficient on the lagged value of the variable and a significant positive coefficient on the interaction with federal structure. These two variables adjust more slowly in federal states than in unitary ones.

First, the degree of practical central bank independence is stickier in federal than in unitary states (Table 5, column 1). (For this regression, I used the Cukierman, Webb, and Neyapti [1992] decade data going back to the 1950s.) In a unitary state with a high turnover rate for the central bank governor-perhaps one per year, or 1.0 on the Cukierman, Webb, and Neyapti index, above the mean—this rate would drop by the next decade, other things equal, to about .39 above the mean. In a federal state with a similar turnover rate, on average the reduction would be to about .74 above the mean.40 The effect of this slower adjustment can be seen clearly if one compares countries across long periods. The correlation between turnover in the 1950s and in the 1980s was .86 for federal structures but just .24 for unitary states. Thus, federalism appears to slow change in the degree of central bank practical independence, which in turn entrenches inflation rates.

Second, stickiness emerges in the extent of central government net lending (Table 5, column 4). Other things equal, in a unitary state whose central government devotes 20% more of outlays to net lending than the mean state in one five-year period, the share of net lending will drop by 8.8 percentage points in the next period (i.e., $20 \times -.44$). In an otherwise similar federal state, on average the drop will be no more than one or two percentage points.41 Sharp adjustments after episodes of heavy central lending are the norm in unitary states but are clearly the exception in federations. In unitary states, the share of central government outlays going to net lending can vary quite substantially from period to period; in federal states, it tends to stay either high or low. If, as suggested, this indicates in part the readiness of central government to bail out subnational units, the budget constraint in federal states appears to be generally either hard or soft but relatively constant.

The data do not support the conjecture that federal structure locks in larger or smaller central or subnational deficits. And there was no evidence that federal states adjust expenditure levels more slowly than unitary states. Federal structure might lock in some aspect of economic or political conditions that affect the strength of inflationary political pressures. The last two columns in Table 5 suggest, however, that neither the level of economic development nor the degree of exposure to imports is more persistent in federal than in unitary states. In both cases, the interaction term is

TABLE 4. Determinants of the Practical Independence of Central Banks

Independence of Central	Banks	
	Мо	del
	(1)	(2)
Legal central bank Independence	.19 (.18)	.19 (.18)
Log average Inflation previous decade	.03 (.07)	.03 (.06)
Number of revolutions or coups in decade	.14** (.03)	.14** (.03)
Civil war in decade	.02 (.01)	.02 (.02)
Domestic debt of central govt. (% GDP) beginning of decade	.0011** (.0004)	.0011 (.0008)
Controls Central bank governor turnover previous period	.25*** (.05)	.25** (.09)
Log per-capita GDP beginning of the decade		.01 (.11)
Dummles Asia	.08** (.03)	.09 (.08)
Latin America + Caribbean	.20*** (.05)	.20** (.05)
Sub-Saharan Africa	.03***	.04
Middle East + N. Africa	(.01) 03 (.05)	(.09) 03 (.13)
E. Europe + former Soviet Unlon	no cases left in	no cases left in
1972–79	.01	.01
1980–89	(.06) .02 (.08)	(.08) .02
Constant	(.06) 04 (.05)	(.12) 07 (.31)
R ²	.3424	.3426
Wald χ^2	18.01	80.08
p<	.001	.001
N	101	101

Note: The dependent variable is the Culoerman, Webb, and Neyapti (1992) index of turnover of central bank chief executive, over roughly ten-year penoda. Model 2 controls for log per-capita GDP Estimation is by least squares dummy vaniables; penel-corrected standard errors are in parentheses (adjusted for heteroskedestroity and contemporaneous correlation) "p < .05, "p < .01, ""p < .01 For data sources, see the Appendix. Penoda: 1960–71, 1972–79, 1980–89. Log average inflation previous decade is calculated as follows: 1960s = average of 1960 and (1965+59)/2, 1960s = average of 1960–84 and 1965–96, 1970s = average of 1970–74 and 1975–79, in each case, log of 1 + average inflation, all countries for which average inflation < 0 coded as 0. Domestio debt of central government is from IMF, *International Financial Statistics Yearbook*, 1994, lines 88b or 88a, as % of GDP, figures for 1990, 1970, and 1980. Log per-capits GDP in purchasing power parity terms, for 1960, 1970, 1980, is from Penn World Tables 5 8a

not at all significant. The discontinuous indicators of exchange rate system and political instability are not suitable for regression analysis. Instead, I compared

 $^{^{40}}$ The estimated change is .04 (i.e., the federal dummy effect) + (-.61 + .31) \times 1.

⁴¹ The estimates imply a change of $(-.44 + .43) \times 20 = -.2$.

the mean change in each indicator between periods for federal and unitary states (in absolute value terms). In the case of exchange rate, the figures are almost identical (.339 federal, .342 unitary). As for political instability, federal states had a larger average absolute value change in the number of revolutions and coups between decades, which suggests that the level of instability is not locked in by federal structure, although the difference is not at all significant.

What can we conclude about how federal structure entrenches either high or low inflation? I did not find convincing evidence that federalism systematically influences the stability of either central government deficits or spending levels. These wax and wane in both federal and unitary states. There does seem to be some influence on the relative degree to which fiscal imbalances pass upward through the system and induce change in monetary policy. This occurs at two key points in the process. First, governments in federal states apparently find it harder than those of unitary states to change the share of outlays they devote to lending, whether low or high. The problem is serious in the latter case, as subnational government deficits are pushed upward, adding to central deficits. Second, central banks that are in practice more independent are less likely to respond to fiscal pressures from large subnational or central deficits by increasing the money supply. Federal structure, by increasing the number of veto players required to change the system of control over central bankers, tends to lock in the degree of central bank independence, whether high or low. In the United States and Germany, where federalism has helped to preserve high central bank independence, this has served to keep inflation low, exactly as Lohmann (1998) argues. But in countries with weak central banks, federal structure makes it hard to reduce the inflation-causing politicization.

A few examples illustrate. Argentina and Brazil, two non-OECD federations, experienced high inflation rates in the 1970s and 1980s and had particular difficulty stabilizing (although both achieved some success in the 1990s). In both countries, the restraints on fiscal pressure afforded by low lending share and a strong bank were ineffective. Each had a high level of central lending, substantially above the median for developing countries, for most of the 1970s and 1980s.42 In that situation, subnational deficits tend to correlate closely with central deficits. Also, the extension of credit by the federal government or central bank was a major cause of inflation. In Argentina in the 1970s and 1980s, provincial governments borrowed heavily from provincial banks, which then discounted the loans to the central bank. In Brazil, the federal government bailed out subnational governments' debt to foreign investors and to domestic credit institutions, such as the federal housing and savings bank (Dillinger and Webb 1998).

Furthermore, throughout the whole postwar period, the governorship of the central bank in both countries turned over with far greater frequency than the average for developing countries. A Practical independence of the central bank was low in the 1950s and remained low in the 1980s. In each country, the central bank governor was particularly likely to be replaced during the six months after a change of government (Cukierman and Webb 1995). This article suggests why Brazil and Argentina had to struggle so painfully and ineffectively to stabilize an unbalanced fiscal and monetary system: Federal structure locked in the failures of the past.

Malaysia, by contrast, shows how a strong central bank can break the link between fiscal imbalances and inflation. In this case, federal structure may have helped preserve central bank independence. The country had large subnational deficits in the 1970s and 1980s (14.3% of subnational outlays in 1972), the central government devoted a relatively large proportion of outlays to net lending (11% in the early 1970s, 15% in the early 1980s), and central deficits were high (10% in the early 1970s, 16% in the early 1980s). Yet, despite fiscal pressures, monetary policy remained tight, and inflation stayed low (less than 10% in the 1970s and 1980s). The secret of success appears to be the high degree of central bank practical independence.44 In the 1970s, the Cukierman, Webb, and Neyapti (1992) index of turnover for Malaysia is zero, and in the 1980s it is .20. The Cukierman and Webb (1995) index of the political vulnerability of the central bank—the average number of changes in central bank governor during the six months after a change of government—for 1950-90 is zero.

To see how low central government lending can block the upward passage of deficits, consider Germany in the 1970s and 1980s. The deficits of Länder and localities in both decades were relatively high, reaching 2.1% of GDP in 1982, compared to a median that year of .5 for all countries in the data set. Yet, central government net lending did not exceed 2% of total outlays. Consistent with this relatively hard budget constraint, central deficits stayed low (a surplus of .7% of GDP in 1972, deficits of 2.1%, 2.0%, and 1.1% of GDP in 1977, 1982, and 1987, respectively; median central deficits those years were 2.6%, 3.6%,

⁴² Average annual share of net lending in central government outlays was 10.2%, 20.6%, 24.8%, and 46.8% in Brazil in 1972, 1977, 1982, and 1987, respectively. In Argentina, comparable figures were 2.1%, 7.9%, 11.8%, and 9.6% in 1973, 1977, 1982, and 1987 (1972 data were unavailable). The median share of net lending share among developing countries in the four periods was 4.8%, 5.0%, 4.9%, and 1.7%.

⁴³ For the 1950s, 1960s, 1970s, and 1980s, the Cuklerman, Webb, and Neyapti (1992) index of turnover had values of .71, 1.08, .88, and 1.00 for Argentina and 1.01, .50, .38, and .80 for Brazil, respectively. The median for developing countries in the four periods was .20, .26, .25, and .20

⁴⁴ Malaysia's access to credit markets also may have helped. Deficits were financed by heavy borrowing, first on domestic and then on foreign markets, before a sharp adjustment triggered by high interest rates brought the deficit down to manageable levels (Demery and Demery 1992). But credit may have been forthcoming in part because the markets believed in the central bank's conservatism. At the same time, that very high interest rates were sustained long enough to induce a major adjustment suggests the effective shielding of monetary policy from fiscal pressures.

The net lending share came to 1.3%, .64%, 1.1%, and .54% of central outlays in 1972, 1977, 1982, and 1987, compared to median values these years of 5.5%, 4.3%, 3.3%, and 1.2%, respectively, for all countries in the data set, and 5.3%, 4.2%, 2.8%, and .57% for the OECD countries.

,			\$	Model	ange III		
	(1)	(2)	ව	(4)	(2)	(9)	6
-	Central Bank Practical Independence	Central Gov*t. Deficit (% GDP)	Subnational Gov*t, Deficit (% GDP)	Central Gov't. Net Lending	Central Gov't. Spending (% GDP)	Log GDP Per Capita	Imports (% GDP)
Lagged variable (expressed as deviation from mean)	61*** (.08)	18 (.13)	.52 (.34)	44*** (.11)	05 (.12)	.17* (.08)	11 (.16)
Federal $ imes$ lagged variable (as deviation from mean)	.31** (.10)	15 (.32)	- 20 (.38)	.43* (.20)	05 (25)	.01 (.04)	. –.04 (.18)
Federal structure	.04*** (.01)	.14 (.89)	04 (.32)	1.44 (1.05)	.84 (2.23)	.01 (.01)	40 (2.32)
Lag of change in variable	19** (.06)	—.48** (.14)	.16 (.17)	12 (.11)	35 (.19)	24** (.08)	03 (.17)
Dummles Asla	.01 (.04)	50 (.94)	.08 (.49)	13 (1.49)	-1.22 (2.42)	01 (.02)	50 (3.80)
Sub-Saharan Africa	02 (.03)	-1.14 (1.32)	.41 (.37)	16 (1.18)	-7.67* (3.45)	11* (.05)	-4.17 (2.86)
Latin America + Caribbean	.14*** (.01)	.84 (1.18)	25 (.43)	.94 (.97)	-4.53 (3.62)	06 (.04)	-2.85(3.25)
Middle East + N. Africa	- 00 (.05)	-2.26 (1.77)	.34 (.45)	1.71 (1.26)	-10.75* (5.32)	- 02 (07)	-2.57 (2.56)
E. Europe and former Sowlet Unlon	.02 (05)	6.52*** (.95)	- 23 (.45)	.63 (.80)	no cases left in	00 (.04)	-2.33 (2.68)
Constant	04** (.01)	3.56*** (.94)	-26 (28)	-2.35*** (.60)	1.75 (1.72)	.99*** (26)	-32.04*** (3.48)
8 L	.4513	.4439	.3255	2596	.2266	2927	.4337
Weld χ^2	93.23	225.51	50.81	32.24	25.55	14.00	99.43
. > d	.001	.001	.00	.001	.003	89.	.001
2	112	125	98	137	121	259	152

4.7%, and 4.0% of GDP for all countries in the data set, and 1.5%, 3.4%, 4.8%, and 2.3% of GDP for just the OECD). The knowledge that the Bundesbank was highly independent and unlikely to monetize the national deficit may have helped focus the minds of central policymakers as they resisted fiscal pressures from below. German federalism also may have helped lock in the government's low lending policies. If an opposition majority in the Bundesrat could prevent the federal government from pressuring the Bundesbank to ease monetary policy and keep voters happy (Lohmann 1998), the same majority presumably could limit the government's freedom to lend irresponsibly to its allies in Land governments.

CONCLUSION

Scholars from Tiebout (1956) and Buchanan (1950) to Oates (1972) and Weingast (1997) have described the economic benefits to be expected from political and fiscal decentralization. Their work informs current efforts to restructure political and economic institutions in the postauthoritarian and postcommunist world. Recently, however, some scholars have raised fears that the widely understood benefits of decentralization may be accompanied by poorly understood but serious dangers (Davoodi and Zou 1998; Prud'homme 1995; Tanzi 1995).

One unresolved question concerns macroeconomic policy. There are several ways that decentralization may affect inflation in theory. On the one hand, decentralization may provide a useful restraint on central profligacy. On the other hand, it may create dangerous incentives for local fiscal free-riding. Or it may lock in current patterns of fiscal and monetary policy, whether profligate or conservative, by increasing the number of actors with a veto over changing the system of macroeconomic governance.

My empirical analysis finds support for the third hypothesis. In decentralized countries, average inflation rates tend to stay either consistently high or consistently low over quite long periods. In centralized states, by contrast, it appears easier for low inflation economies to slip into macroeconomic imbalance and for high inflation economies to stabilize. Average inflation in the late 1960s and the late 1980s correlates at .85 among federations, compared to -.02 among unitary states. In five of six tests, the difference is significant, whether an indicator of political decentralization (federal structure) or indicators of fiscal decentralization (the subnational expenditure or tax shares) are used, and controlling for numerous other possible causes of inflation. In some (mostly developed) countries, such as Germany and the United States, decentralization appears to lock in macroeconomic stability. In other (mostly developing) countries, such as Argentina and Brazil, decentralization has preserved high inflation.

Evidence from the 1970s and 1980s identifies two points in the fiscal and monetary policy process at which decentralization creates stickiness. I find that, as various scholars have argued, greater practical (not necessarily legal) independence of the central bank is associated with lower inflation. It turns out that the central bank's degree of practical independence is far more constant over time in federal than in unitary states. In Germany, for example, the division of power between central and regional governments has insulated the Bundesbank. Regional governments police the center, preventing any change to the established arm's-length relationship between the federal government and the central bank that would benefit federal politicians but not their regional counterparts. In other federations, such as Brazil, a norm of central bailouts of regional governments has developed, and regional governors use their considerable powers to defend the politicized nature of monetary policy.

The second point of stickiness in federal states is the degree to which central government uses its resources to extend loans—a plausible proxy for the hardness or softness of budget constraints. The share of government outlays devoted to net lending ranged in the late 1980s from −7% in New Zealand to 47% in Brazil. When net lending is high, large subnational deficits are associated with large central government deficits, which suggests that deficits are pushed upward for bailout. And when lending is high, so is inflation. The extent of central government lending is far more constant in federal than in unitary states. Some federal systems, such as Germany and Austria, were consistently low lenders in the 1970s and 1980s, but others, such as Brazil and India, were consistently high lenders. Political systems differ in their "fiscal conductivity"—the degree to which imbalances are pushed upward from local and regional to central budgets. Decentralization does not itself cause this, but it does appear to reduce change in the degree of conductivity, whether high or low. In countries like Brazil, with soft budget constraints, federal politics can hamper a shift to harder policy. In countries like Germany, with hard budget constraints, the status quo is preserved by decentralized political institutions that impede change to policies.

Developing countries tend to have weaker central banks and more central government lending than developed countries. As a result, decentralization correlates with lower inflation in the OECD but higher inflation among non-OECD members, although there are exceptions. In the federation of Malaysia, for instance, a very independent central bank apparently resisted fiscal pressures to boost monetary growth to inflationary levels in the 1970s and 1980s. Malaysia had average inflation of 1% in the late 1960s and 1.3% in the late 1980s. Low central bank independence in some developing countries is probably explained in part by their political instability. Revolutions or coups tend to be more frequent in poorer countries and are positively related to central bank director turnover. Malaysia, unlike a good number of Third World nations, experienced none of these events in the 1970s and 1980s.

Constitutional fragmentation is a major reason that political decentralization is associated with a persistent pattern of inflation, high or low. Decentralized systems increase the number of powerful political actors with influence over such areas as fiscal and monetary policy. But other ways of fragmenting political authority also exist. In Chile, for example, one of the more centralized developing countries, a system of elaborate checks and balances was set up by the Pinochet regime before leaving office. Some important administrative posts, including the head of the central bank, are insulated from control by elected officials, and a supermajority is necessary to overturn the laws guaranteeing that insulation (Londregan 2000). Such provisions arguably have helped preserve central bank independence and low inflation in Chile, without political decentralization. A study of whether kinds of political fragmentation other than those produced by decentralization also perpetuate macroeconomic policy would be valuable.

Constitutional fragmentation can be overcome if actors in the different power centers belong to a single cohesive party, and one might expect less policy persistence in federal states under that condition. This may explain the Menem regime's surprisingly successful reforms to the Argentine monetary system in the early 1990s. For the first time since democratization, the president, a majority in both legislative chambers, and a majority of state governors were all from the same party (Jones 1997). Exploring interactions between political decentralization and party dominance is an obvious direction for future research.

Several other questions await further study. First, is a high level of central government lending a symptom or cause of soft budget constraints? I suspect the answer is "both," but more the former than the latter. Second, the kinds of decentralization I was able to examine conflate decentralization to regions or states with decentralization to localities. The dangers created by strengthening intermediate units may be reduced if authority is decentralized even further, to the municipal level. Third, and most important, more research is needed to discover precisely how the institutions of specific federal systems impede changes in the degree of central bank independence or central government lending policies. This article establishes a general answer—the large number of veto players in federal systems—but the details of institutional mechanisms are likely to vary from case to case. Ultimately, the association of political decentralization with a large number of veto players, and of more veto players with more stable monetary policy, should be tested directly in a worldwide sample. This will require the construction of a data set, including both developed and developing countries, of the number of veto players with leverage over central policy enactment and implementation in specific areas. That is a gargantuan task, but an important one.

If my analysis is correct, important policy implications follow from it. Attempts to reduce inflation by introducing federal institutions into countries with soft budget constraints on subnational governments and a politically dependent central bank are likely to prove disastrous. Such institutions will not reduce inflationary pressures, and they will tend to lock in the existing politicization of monetary policy and make stabilization considerably harder. Lessons derived from lowinflation, developed federal states, such as Germany, Switzerland, or the United States, may have perverse consequences if applied in many developing world settings.

Furthermore, the order in which political and economic reforms are implemented appears to be extremely important. Successful stabilization of monetary policy followed by political decentralization can maximize the advantages of both. Political decentralization followed by macroeconomic stabilization is likely to fail and risks a speedier slide into hyperinflation. This parallels the logic of my game-theoretic treatment of decentralization and economic reform (Treisman 1999). Modeling the interactions between center and regions in two-level states, I found that the same political and economic reforms can have drastically different results when implemented in the opposite order. Political decentralization followed by an increase in the center's rate of public good provision can prompt a vicious cycle of fiscal redistribution, regional challenges, and increasing deficits, whereas the same reforms in reverse order can leave the state as fiscally solvent and peaceful as when they started.

Decentralization has many advantages, but my findings suggest at least caution in recommending it to developing countries that face major macroeconomic stresses. As Poland embarks on a plan to create stronger regional governments, South Africa struggles to impose financial discipline on its new provinces, and similar projects are debated in countries around the globe, these issues are likely to become increasingly important.

APPENDIX

Inflation Data: Average of annual inflation rates of CPI 1970–74, 1975–79, 1980–84, 1985–89. Unless otherwise indicated, from IMF, International Funancial Statistics Yearbook, 1994, 106–9 (line 64). Notes: Poland 1971–74; Czechoslovakia 1971–74; Hungary 1973–74; China 1971–74; Kuwait 1973–74; Nicaragua 1973–74; Romania 1971, 1973, 1974; Uganda 1981–84; Jordan 1985, 1987–89.

Subnational Expenditure Shares: Unless otherwise noted these are calculated as (state + local government total expenditures)/(consolidated central government total expenditures + state + local government total expenditures) 1972, 1977, 1982, 1987, from IMF, Government Finance Statistics Yearbook, 1977, 1978, 1979.

Subnational Tax Shares: Unless otherwise noted these are calculated as (state + local government total tax revenues)/(consolidated central government total tax revenues + state + local government total tax revenues) 1972, 1977, 1982, 1987, from IMF, Government Finance Statistics Yearbook, 1977, 1978, 1979, 1986, 1988, 1990, 1997.

Notes on Expenditure and Tax Variables

For 1972 Variables: Canada 1973, 1974 for expenditures; Netherlands 1973, 1974 for expenditures; USA 1973; Austria 1973; France 1974; Korea 1974; Jordan 1974 (budgetary central government); Chile 1974; Spain 1973, 1972 for expenditures; Italy 1973; Ecuador 1974 (tax variables 1975); Mexico 1974 for tax; Brazil expenditure 1974 from Garman, Haggard, and Willis (1996), tax 1972 from Shah (1991, 16);

Colombia 1974; India 1974; Kenya budgetary central government; Zaire 1973; Pakistan 1975; Cameroon 1975 and local revenue is all revenue, not just tax; Iran 1973; Indonesia 1975; Bangladesh 1974 for expenditures, for others 1975 (central is "budgetary central government," local is total revenue); Taiwan (ROC) 1973; Uruguay 1973 (total revenue instead of tax, both levels); Zimbabwe from World Bank (1997) for 1974; Japan 1975, from Shibata (1993, 145).

For 1977 Variables: Belgium 1978; Malaysia local revenue is total revenue, not tax revenue; note Greece unit change from 1972; Italy 1975; Uganda 1980 and "budgetary central government"; Bangladesh central is "budgetary central government," local is "total revenue"; Trinidad and Tobago 1979; Uruguay local is total revenue; Honduras 1976; Zambia is "budgetary central government"; Bolivia 1980; Philippines 1978; Japan 1974; Taiwan 1977; Uruguay both levels are "total revenue"; Japan 1975 from Shibata (1993, 145); Indonesia 1975–76 from Shah (1994, 196); Brazil tax 1977 from Shah (1991, 16).

For 1982 Variables: New Zealand 1981 "budgetary central government"; Poland 1984; Bolivia 1985; Uruguay local is total revenue; Ethiopia 1981; Italy 1985; Ecuador 1980; Italy tax 1985; Japan 1980 from Shibata (1993, 145); Mexico from Garman, Haggard, and Willis (1996); Argentina 1983 from Garman, Haggard, and Willis (1996); Brazil expenditure from Fundacion de Investigaciones Economicas Latino-americanas (1993), tax 1982 from Shah (1991, 16); Venezuela 1980 from Garman, Haggard, and Willis (1996); Taiwan from Statistical Yearbook of the ROC, 1995; Honduras from Nickson (1995), figures are for 1984; Indonesia 1980–81 from Shah (1994, 196).

For 1987 Variables: New Zealand 1987 tax figure is imputed from 1990 figure of 6.9 in World Bank (2000, 217). Switzerland 1987 tax figure is imputed from 1990 figure of 37.0 in World Bank (2000, 217). Malaysia 1988 tax figure used. Japan 1985 from Shibata (1993, 145). Argentina 1987 tax figure imputed from 1990 figure of 38.2 in World Bank (2000, 217). Colombia tax figure for 1986 used. Indonesia 1985-86 from Shah (1994, 196). Venezuela 1989 from Garman, Haggard, and Willis (1996); 1977 tax figure imputed from 1972 and 1982 ones. Pakistan and Nigeria from Shah (1994, 52). Kenya 1986. Taiwan from Statistical Yearbook of the ROC, 1995. Dominican Republic from Nickson (1995) for 1986. Nicaragua from Nickson (1995) for 1989. Peru is figure for 1990 from World Bank (2000, 217). Panama from Nickson (1995) for 1991. Costa Rica from Nickson (1995) for 1984. Brazil tax 1987 from Shah (1991, 16); expenditure for 1988, from Shah (1991, 18). Ethiopia 1987 tax figure is imputed from 1990 figure of 1.6 in World Bank (2000, 217). Costa Rica 1990 tax figure from World Bank (2000, 217) used for 1987; 1977 and 82 figures imputed from 1972 and 1990 figures.

GDP per Capita: From Penn World Tables 5.6a, ppp, 1970, 1975, 1980, 1985.

Imports: From IMF, International Financial Statistics Yearbook, 1994 and 1990, 1970, 1975, 1980, 1985.

Money Growth: Average annual money growth in % (IMF, International Financial Statistics Yearbooks, line 34); "money" = demand deposits + currency outside banks.

Central Government Budget Deficit (-), Surplus (+): As percentage of GDP 1972, 1977, 1982, 1987 from IMF, International Financial Statistics Yearbook, 1987 and 1994.

Central Government Expenditure: As percentage of GDP 1972, 1977, 1982, 1987 from IMF, International Financial Statistics Yearbook, 1987 and 1994.

Central Government Net Lending: As percentage of central government expenditure plus lending minus repayments, from IMF, Government Finance Statistics Yearbooks, 1972, 1977, 1982, 1987, or a close year.

Subnational Budget Deflett (-), Surplus (+): GDP from World Bank 1992; subnational deficit = deflect of state budgets plus deficit of local budgets, from IMF, Government Finance Statistics Yearbook, 1972, 1977, 1982, 1987.

Domestic Debt of Central Government: As percentage of GDP, domestic debt of government as of 1960, IMF, International Financial Statistics Yearbook, 1994, line 88b or 88a. GDP also from IMF, International Financial Statistics Yearbook, 1987 and 1994.

Revolutions and Coups: From Easterly and Levine (1997) data set. Yugoslavia coded as no revolutions or coups in 1970s or 1980s.

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Legislative Professionalism and Incumbent Reelection: The Development of Institutional Boundaries

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It is well established that legislators from highly professionalized bodies are more likely to win reelection than members of less professionalized legislatures. We find that the effect of professionalization on incumbent electoral success is far more pervasive. As the level of professionalism of a legislature increases, the effects of external political and economic forces (such as coattails from higher level elections and national economic conditions) on a legislator's chances for reelection diminish in strength. This implies that legislative professionalization promotes institutionalization by establishing boundaries that insulate members from external shocks. We reach these conclusions by specifying and testing a district-level model of state legislative election outcomes, using as dependent variable the probability that an incumbent will win reelection. The model is estimated with probit using data for more than 42,000 state legislators from 1970 to 1989.

7 hat determines whether a legislator seeking reelection will win? We address this question by paying particular attention to the role of professionalization of legislatures in influencing the electoral success of members. We know that American legislators are highly likely to win reelection (Garand 1991; Jacobson 1987; King 1991). We also know that those from highly professionalized institutions have even better odds than those from less professionalized bodies, as the former have greater resources for travel to their districts (Holbrook and Tidmarch 1991), for casework (Cox and Morgenstern 1993, 1995; King 1991), and for devoting time and energy to campaign activities (Carey, Niemi, and Powell 2000). We maintain, however, that professionalism does more than boost the electoral prospects of incumbents; it changes members' relationships with the legislature's political and economic environment. In particular, professionalization promotes legislative institutionalization by establishing boundaries that shield members from external shocks.

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PROFESSIONALIZATION AND THE ESTABLISHMENT OF INSTITUTIONAL BOUNDARIES

Professionalization is a model used predominantly in the literature on state legislatures and refers to the development of overall legislative capacity (Hibbing 1999). A professionalized legislature is one with abundant resources; in particular, Rosenthal (1996, 1998) refers to the five S's: space, salary, session length, staff, and structure. Institutionalization, in contrast, is a broader model of legislative development employed primarily in the literature on Congress (Hibbing 1999). Polsby (1968) suggests that institutionalized legislatures have three characteristics (see also Cooper and Brady 1981; Hibbing 1988; Squire 1992), two of which relate to internal operations; they are internally complex and rely on universalistic, rather then discretionary, criteria.1 The third and most critical characteristic-the "conceptual core" (Rosenthal 1996, 185) of institutionalization—is the presence of boundaries, the separation of the legislature from its political, economic, and social environment (Cooper and Brady 1981; Huntington 1965; Polsby 1968).

Scholars have set forth two requirements for a legislature to be bounded, both of which relate to the nature of elections. First, according to Polsby (1968, 145–6), in a bounded legislature "it is relatively difficult to become a member," and turnover is low. This implies elections in which incumbents have a high probability of victory. Second, in a bounded legislature, elections are insulated or buffered from the effects of external political and economic shocks (Chubb 1988; Hibbing 1999). Such elections are similar to those characterized by Jacobson (1997) and other students of

¹ Complex legislatures have "functions that are internally separated and parts that are not wholly interchangeable" (Rosenthal 1996, 183); universalistic criteria refer to standard, consistent rules (such as seniority to determine committee rank)

Congress (e.g., Herrnson 1998; Salmore and Salmore 1985) as candidate centered, because the insulation of elections from external forces makes incumbents personally "responsible for [their own] reelection" (Jacobson 1997, 33).

Our central thesis is that with legislative professionalism come the boundaries necessary for institutionalization. Although professionalization and institutionalization generally have been treated as distinct models of legislative development, several scholars have proposed this linkage (Hibbing 1999; Moncrief and Thompson 1992; Rosenthal 1998; Squire 1992). Their argument is straightforward. Professionalization changes both the incentives and capabilities of legislators because it gives them resources (e.g., "money, perquisites, staffers, information" [Hibbing 1999, 163]) that make a long career in the legislature attractive. At the same time, it provides the "electoral resources that members may use to insulate themselves from changing political tides" and make a long career possible (Squire 1997, 41). What has been lacking in the literature is a strategy for a decisive empirical test of the hypothesis that professionalization leads to boundaries essential for institutionalization.

We propose that this hypothesis implies two testable propositions, each related to one of the requirements for a legislature to be bounded. The first is the familiar assertion that incumbency advantage is tied to legislative resources: With all else equal, as the level of professionalism of a legislature increases, its members' probability of winning reelection also rises. This hypothesis has received much empirical support (e.g., Carey, Niemi, and Powell 2000; Cox and Morgenstern 1993, 1995; Holbrook and Tidmarch 1991; King 1991; Weber, Tucker, and Brace 1991).2 The second proposition is that as a legislature's level of professionalism increases, so does the extent to which legislative elections are insulated from external forces. Put differently, external political and economic shocks have their strongest effects on the probability that incumbents will win reelection when professionalism is low, and they have weaker effects when greater professionalism serves to buffer incumbents from these forces.

Support for this second proposition is less definitive. It is certainly consistent with the declining influence of national economic conditions (Campbell 1993; Erikson 1990; Radcliff 1988) and presidential coattails (Campbell 1986; Jacobson 1997) on congressional election outcomes, which occurred as members of Congress were acquiring greater resources to devote to constituency service (Jacobson 1990). Ultimately, however, this hypothesis is difficult to test with congressional data because the number of sessions to observe is

small, and numerous political and economic variables have changed colinearly with congressional resources (King 1991).

Chubb (1988) conducted the most extensive empirical analysis of the hypothesis and obtained mixed results: He found that a high degree of legislative professionalism fails to insulate state legislative elections from the effects of national economic conditions but does buffer them from coattail effects of higher level elections.³ His conclusions, however, are based on aggregate analysis of the shares of legislative seats controlled by the major parties and do not necessarily reflect the influence of external forces on the success of individual incumbents. For example, as Jacobson (1990, 80) notes, aggregate seat shares can "track [higher level election] results even though the [results are] a poor predictor of the . . . winner in any particular district."

A DISTRICT-LEVEL MODEL OF INCUMBENT ELECTORAL SUCCESS

Our primary theoretical focus is the proposition that legislative professionalization promotes institutionalization by creating boundaries that insulate members from external electoral shocks. Our strategy for testing this hypothesis requires us to develop a model explaining the probability that an incumbent legislator will win reelection. We incorporate a variety of factors identified by research on Congress and state legislatures as influencing the vulnerability of incumbents to electoral defeat. In addition to legislative professionalism, these include such individual/district-level characteristics of legislators as their margin of victory in the previous race, their term length, and the type of district in which they compete, as well as such contextual factors as redistricting, coattail effects from higher level contests, and national economic conditions.

We introduce an original proposition about economic conditions. Most previous studies advance a responsible party voting model, which assumes that voters hold the president's party responsible for the state of the economy and that all candidates of the party receive the same credit or blame (e.g., Jacobson 1990; Kramer 1971). In contrast, Hibbing and Alford (1981) offer what Stein (1990) refers to as the restricted in-party culpability thesis, which suggests that presidential party incumbents are held responsible for the economy, but nonincumbents of the same party are not. We propose a more complex differential responsibility model: Nonincumbents of the president's party are held responsible for economic conditions but to a lesser degree than incumbents.

We test our model using data for more than 42,000 state legislators from 1970 to 1989. Although our chief

² Except for Carey, Niemi, and Powell (2000), these studies find evidence of the role of legislative resources in protecting incumbents but do not directly assess the effect of resources on the probability that an incumbent will win reelection. Instead, they look at vote margins, margin-based measures (i.e., sophomore surge, incumbency advantage), or whether an incumbent is challenged.

³ Chubb (1988) refers more frequently to institutionalization than professionalization, but we argue that he blurs the conceptual distinction between the two terms so that they are treated as virtually synonymous.

theoretical interest is in professionalism's role in creating legislative boundaries, our large sample permits improved tests of many hypotheses from the elections literature. For example, there are sufficient degrees of freedom to measure separately presidential, gubernatorial, and senatorial coattails, and to measure these effects with a wide range of statistical controls.

HYPOTHESES

We begin by developing hypotheses about the effects on the probability of reelection of (1) a legislator's historical electoral strength, (2) the strength of candidates running higher on the ticket, and (3) national economic conditions, that should hold at any fixed level of professionalism. Then we turn to our central proposition—that legislative professionalism creates the boundaries necessary for institutionalization. We argue that this implies several hypotheses about the influence of professionalism on the probability of incumbent victory, including ones that specify interaction between professionalism and each of (1) historical electoral strength, (2) the strength of higher level candidates, and (3) economic conditions. We conclude the presentation of our model by considering the effects of various district election rules and redistricting on the probability of reelection.

Historical Electoral Strength

A candidate's strength in one election is a strong determinant of the chances of success in the next race (Garand 1991; Jacobson 1987). Someone who wins by a thin margin probably has significant personal weaknesses or a dearth of partisan supporters in the district, and these problems are likely to persist for the next election. In contrast, candidates who win impressively are likely to be perceived by potential challengers and contributors as secure in the next election (Krasno and Green 1988) and are less likely to attract a well-funded challenger of high quality. Lack of opposition is another sign of strength. This suggests that legislators who ran unopposed in their previous race have a greater probability of winning reelection than those who faced opposition. Yet, it is likely that there is some threshold vote margin at which a victory in a contested election becomes so decisive that it is more intimidating to potential challengers than even an uncontested victory.

HYPOTHESIS 1. Incumbents who were unopposed in their previous race have a higher probability of reelection than those who faced opposition and won with a "typical" margin. Only legislators who won by an overwhelming margin in their previous race have a higher probability of victory than those who were uncontested.

HYPOTHESIS 2. For incumbents contested in their previous race, the greater their margin of victory in that election, the greater is the probability of their reelection.

Coattails

The essence of the coattail proposition is familiar: A party's presidential, gubernatorial, and senatorial candidates influence the prospects of its congressional and state legislative candidates on the same ballot. Popular candidates for higher office bring party supporters to the polls, and candidates at lower levels enjoy a spill-over effect (Campbell 1993; Jacobson 1997). Therefore, in the case of state legislators:

HYPOTHESIS 3. When an election for higher office (presidential, gubernatorial, or senatorial) is being held simultaneously, the greater the vote percentage for that office received by the party of a state legislator, the greater is his probability of reelection.

Previous research on the relative strength of presidential, gubernatorial, and senatorial coattails has yielded mixed results (Campbell 1986, 53; Chubb 1988; Lowry, Alt, and Ferree 1999). We expect presidential coattails to be the strongest, based on the much greater visibility of the presidency in relation to the other two offices. Because there is no systematic difference in the visibility of gubernatorial and senatorial elections, it is less evident which of these should produce stronger coattails. It seems plausible, however, that the choice for state legislator is somewhat more likely to be influenced by the preference for governor than that for U.S. senator, since governors and state legislators deal with the same issues, whereas senators deal with often distinct national policy issues.

HYPOTHESIS 4. The coattail effect is strongest for presidential elections, weakest for senatorial races, and in between for gubernatorial contests.

Economic Conditions

A strong national economy helps congressional candidates of the president's party, and a poor economy hurts them (Hibbing and Alford 1981; Jacobson 1990; Kramer 1971). Governors, under some circumstances, are held accountable for state economic conditions (Leyden and Borrelli 1995; Lowry, Alt, and Ferree 1999), but state legislative candidates are not (Chubb 1988; Lowry, Alt, and Ferree 1999; Stein 1990). Chubb (1988) finds, however, that state legislators of the president's party are subject to the same influence of national economic conditions as members of Congress.

⁴ The share of voters within an incumbent's district who strongly identify with her party is another likely determinant of reelection probability. Our desire to test our model using all state legislative races over two decades makes it infeasible to collect data on district-level party strength. Our empirical analysis may overestimate the effect of victory margin in the previous election because its coefficient may be capturing some of the influence of party strength.

Most economic voting studies rest on the responsible party model, which assumes that voters deem the presidential party responsible for the economy and that all candidates of the party receive the same credit or blame (Chubb 1988; Jacobson 1990; Kramer 1971; Tufte 1975). Hibbing and Alford (1981) and Stein (1990) challenge this view; their restricted in-party culpability thesis assumes that incumbents of the president's party are held responsible for the economy, but nonincumbents (challengers and open-seat candidates) of the same party are not. Tests of this thesis have yielded mixed results (Fiorina 1983; Stein 1990).

We consider these assumptions about nonincumbents of the president's party too extreme and propose a third option, the differential responsibility model. We hypothesize that challengers from the presidential party are held responsible for economic conditions but to a lesser degree than incumbents. Voters recognize that a challenger cannot fairly be blamed for bad economic conditions (or credited for a good economy), but they may still want to punish (or reward) the party they hold responsible; if so, they may act as if challengers from the president's party have partial responsibility for economic conditions.

These three models of economic voting lead to similar expectations regarding incumbents of the president's party: Their probability of victory should be positively related to economic performance. All three models also assume that incumbents in the opposition party receive no credit or blame for economic conditions. This means that the influence of economic conditions on the probability that an incumbent of the opposition party will be reelected depends on the degree to which her challenger is held responsible for the condition of the economy. If the restricted in-party culpability thesis is correct, and thus the electorate does not hold her challenger responsible, then economic conditions should have no effect on the probability that she will win reelection. At the other extreme, if the responsible party model is right, and challengers from the presidential party are held as responsible as its incumbents, then good economic conditions should have a negative effect on the probability that an opposition party incumbent will win, and the magnitude of this effect should be the same as that of the positive effect of good economic conditions on the probability of victory for an incumbent of the president's party. Finally, if our differential responsibility model is correct, then good economic conditions should have a negative influence on the probability that an opposition-party legislator will win reelection, but the magnitude will be smaller than the positive effect of good economic conditions on the likelihood that an incumbent of the presidential party will win. The differential responsibility model leads us to propose the following.

Hypothesis 5. (i) For a legislator of the same party as the president, an improvement in national economic con-

ditions results in an increase in the probability of winning reelection. (ii) For a legislator of the opposition party, a similar improvement in economic conditions leads to a decrease in the probability of winning, but one of smaller magnitude than in part (i).

Our theory of the influence of economic conditions is depicted in Figure 1. At this point, we are not considering the role of legislative professionalism; therefore, restrict attention to the bottom two lines in the graph, in which professionalism is held constant (at a low level). In the figure, hypothesis 5(i) predicts that the slope of line AB is positive, whereas hypothesis 5(ii) suggests that EF has a negative slope smaller in magnitude than that of AB.

Finally, if incumbents of the president's party are held responsible for the economy, but members of the opposition party are not (as all three models of economic voting maintain), two other predictions should be met regardless of the degree to which voters hold challengers from the president's party responsible.

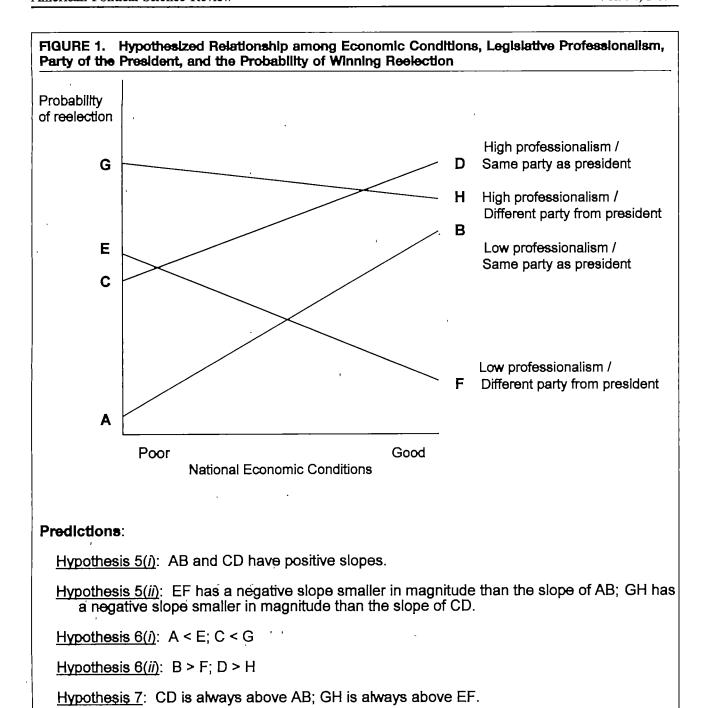
HYPOTHESIS 6. (i) When economic conditions are poor, the probability of reelection is lower for legislators from the president's party than for legislators of the opposite party. (In Figure 1, A < E). (ii) When economic conditions are good, the probability of reelection is higher for legislators from the president's party. (In Figure 1, B > F).

The Role of Legislative Professionalism

We have defined a bounded legislature as one in which two conditions are satisfied: Members have a high probability of winning reelection, and elections are insulated from the effects of external political and economic shocks. Professionalism promotes the establishment of boundaries by influencing the goals of legislators and by giving them resources that help them attain their goals.

The abundance of resources available in a highly professionalized legislature increases the incentive for members to desire a long career by giving them an attractive and productive work environment (Fiorina 1994; Moncrief 1999). Furthermore, these resources enhance their ability to achieve the goal of a long career. Large budgets permit extensive constituency service. Strong staff support enhances the governing capacity of legislatures and thus the ability of members to claim credit for policy initiatives. Both effective governance and constituency service heighten the popularity of members and their chances of reelection. Moreover, in highly professionalized legislatures members tend to be full time and have relatively large travel allowances. These advantages contribute to their public visibility and name recognition, which increase their probability of winning reelection.

Hypothesis 7. As a legislature's level of professionalism rises, its members' probability of winning reelection increases.



Legislators interested in a long career also have a strong stake in avoiding uncertainty by shielding themselves from the effects of external electoral shocks over which they have little control. Of course, such shielding insulates them not only from harmful forces (e.g., a weak presidential candidate on the same ballot) but also from beneficial ones (e.g., a strong presidential candidate). We argue that incumbents are willing to forgo the potential benefits in return for protection from potential costs. They recognize that since a huge majority of incumbents (more than 90%) are reelected,

Hypothesis 8(b): GH is not as steep as EF; CD is not as steep as AB.

there is a bias in their favor. As a result, they perceive the risk from "negative" external forces to be greater than the potential from "positive" forces. (In effect, given a probability of reelection greater than .90, there is much more room to fall than rise.) Because members of a highly professionalized legislature should be able to take advantage of the available resources to focus attention on themselves through both their legislative and campaign activities, they are more likely than members of less professionalized bodies to be able to shield themselves from external forces.

HYPOTHESIS 8. As the level of professionalism of a legislature increases, so does the degree to which elections are buffered from external political and economic forces. These forces have their strongest effects on the probability of winning reelection when the level of professionalism is low, and their effects become weaker as professionalism increases.

We hypothesized earlier that the probability of an incumbent's reelection is influenced by two types of external forces: the outcomes of higher level elections and national economic conditions. We now refine these predictions and hypothesize interaction between level of professionalism and these forces in influencing the probability of incumbent reelection, which yields two corollaries to hypothesis 8.

Hypothesis 8(a). As legislative professionalism increases, presidential, gubernatorial, and senatorial coattail effects decrease in magnitude (i.e., as the level of professionalism of an incumbent's institution increases, the positive effect of the percentage of the vote received by her party in a simultaneous higher level election on her probability of winning reelection declines in strength).

Hypothesis 8(b). As legislative professionalism increases, the magnitude of the effect of economic conditions on the probability that a legislator will win decreases in strength. (In Figure 1, GH is not as steep as EF, and CD is not as steep as AB.)

We apply similar logic to hypothesize that the effect of a legislator's previous margin of victory on his probability of reelection is buffered by a high level of professionalism. We have argued that highly professionalized legislatures are likely to attract career-oriented members with a great stake in being reelected. After surviving a close race, such members should direct much attention in the next term to overcoming the weaknesses that nearly cost them their seat. Moreover, members of highly professionalized legislatures have substantial resources to devote to this effort, which enhances the chance of success. In contrast, the future prospects of legislators with fewer resources are more likely to be determined by their previous success: Those who win big probably can repeat their success the next time around; those who win by a small margin are likely to remain vulnerable.

HYPOTHESIS 9. For incumbents contested in the previous election, as legislative professionalism increases, the positive effect of their margin of victory in the previous race on their probability of reelection decreases in magnitude.

District Electoral Rules

Although most state legislative elections are contested in single-member districts (SMDs), several forms of multimember districts (MMDs) can be found in some states. In free-for-all MMDs—also called true MMDsall candidates for the district's seats run against one another. In MMDs with positions—that is, post elections—candidates must specify the post for which they are running, and typically each post election will pit one Democrat against one Republican. Cox and Morgenstern (1995) found that the incumbency advantage emerged earliest in states with SMDs but then grew at a more rapid rate in states with true MMDs, so that by the 1980s, it was nearly equal in both. More recently, Carey, Niemi, and Powell (2000) found that incumbents in SMDs have a higher probability of reelection than those in either true MMDs or post elections, which is attributed to the "name recognition advantage" legislators in SMDs derive from their status as "monopoly providers of legislative representation for their districts" (pp. 683-4). Carey, Niemi, and Powell hypothesize, however, that although incumbents from true MMDs and post election districts face the same name recognition difficulties, challengers in true MMDs have better prospects for unseating incumbents because of the "larger numbers of candidates competing and smaller overall vote shares required for victory" (p. 684). This reasoning implies the following.

Hypothesis 10. Incumbents running in single-member districts have a higher probability of winning than those running in post elections, who in turn have a greater probability of victory than those in true multimember districts.⁵

Districts also differ in term length. In most states, the variation is by chamber; state senators serve longer terms than members of the lower house. This is not the case in all states, and we prefer to focus on variation in term length rather than variation in the nominal designation of the chamber as upper or lower, because we believe that term length reflects more theoretically meaningful variation. We predict that legislators who serve shorter terms should be less vulnerable to defeat. A shorter term increases the frequency of campaigning, which contributes to greater visibility for incumbents. Also, a longer term makes seats more desirable to challengers but creates fewer opportunities for candidates to run, which increases the probability that a high-quality challenger will emerge in any single race (Carey, Niemi, and Powell 2000).

Hypothesis 11. Legislators holding two-year terms have a higher probability of reelection than those with four-year terms.

Redistricting

Two conditions need to be satisfied for political parties to be able to manipulate the redistricting process to the

⁵ We consider MMDs with alternating years to be functionally equivalent to MMDs with positions, since they impose the same name recognition difficulties for legislators, and they both generally have one Democrat and one Republican competing for a single seat.

benefit of their candidates. First, the legislature and the governor—rather than the courts—must have primary responsibility for redistricting (Campagna and Grofman 1990).⁶ Second, a single party must control the governor's office and both chambers of the legislature.

HYPOTHESIS 12. In an election immediately following redistricting, a legislator has (i) the greatest probability of reelection if his party had full control over the redistricting process (i.e., the legislature and the governor had independent authority to draw districts, and the legislator's party had control of the governorship and both houses of the legislature), (ii) the lowest probability of winning if the other party had full control over redistricting, and (iii) an intermediate probability if the legislature and governor did not have independent control over redistricting or there was divided party control of the legislative and executive branches.

SPECIFYING THE MODEL AND OPERATIONALIZING VARIABLES

Together, the hypotheses developed in the previous section imply the following probit model:

$$WinNext_{t} = \Phi \left(\beta_{0} + \beta_{1} Profess_{t} + \beta_{2} Contest Prev_{t} \right) \\ + \beta_{3} \left[(Contest Prev_{t}) \left(Margin_{t-1} \right) \right] \\ + \beta_{4} \left[(Contest Prev_{t}) \left(Margin_{t-1} \right) \left(Profess_{t} \right) \right] \\ + \beta_{5} PresElecYr_{t} + \beta_{6} \left[(PresElecYr_{t}) (Profess_{t}) \right] \\ + \beta_{7} OwnPartyPresVote\mathcal{H}_{t} + \beta_{8} \left[(OwnPartyPresVote\mathcal{H}_{t}) \right] \\ \cdot (Profess_{t}) \right] + \beta_{9} GubElecYr_{t} + \beta_{10} \left[(GubElecYr_{t}) \right] \\ \cdot (Profess_{t}) \right] + \beta_{11} OwnPartyGubVote\mathcal{H}_{t} \\ + \beta_{12} \left[(OwnPartyGubVote\mathcal{H}_{t}) (Profess_{t}) \right] \\ + \beta_{13} SenElecYr_{t} + \beta_{14} \left[(SenElecYr_{t}) (Profess_{t}) \right] \\ + \beta_{15} OwnPartySenVote\mathcal{H}_{t} \\ + \beta_{16} \left[(OwnPartySenVote\mathcal{H}_{t}) (Profess_{t}) \right] \\ + \beta_{17} EconCondit_{t} + \beta_{18} \left[(Profess_{t}) (EconCondit_{t}) \right] \\ + \beta_{19} NotPresParty_{t} + \beta_{20} \left[(NotPresParty_{t}) (Profess_{t}) \right] \\ + \beta_{22} \left[(NotPresParty_{t}) (EconCondit_{t}) (Profess_{t}) \right] \\ + \beta_{23} \left[(AfterRedist_{t}) (OwnPartyRedist_{t}) \right] \\ + \beta_{24} \left[(AfterRedist_{t}) (OppPartyRedist_{t}) \right] + \beta_{25} Term2Yr_{t} \end{aligned}$$

+ $\beta_{26}SMDist_t + \beta_{22}PostDist_t$).

The unit of analysis is the state legislator seeking reelection, and Φ denotes the cumulative normal distribution function. The conceptual dependent variable, $WinNext_p$, is the probability that the legislator will win the election in year t. The observed dependent variable is dichotomous; it equals one if the legislator wins, zero otherwise.

The independent variable of greatest theoretical interest is *Profess*, the level of professionalism of an incumbent's legislature. We believe that operating budget per member—an indicator used by King (1991), Weber, Tucker, and Brace (1991), Van Dunk and Weber (1997), and others—is the purest measure of the total resources available to a legislature's members and is thus the best indicator of professionalism. For this reason, we use the size of the legislative operating budget per member in real dollars, linearly transformed (to facilitate interpretation) so that zero denotes the lowest value during the period of analysis, and one represents the highest.

One important aspect of the degree of professionalism of a legislature is the amount of compensation members receive. Yet, salary is a weaker measure of professionalism because a legislature that pays well but provides its members few additional resources should not be deemed highly professionalized. Nevertheless, we also estimate our model using average real legislative salary as the indicator of professionalism because that measure is frequently used (e.g., Chubb 1988; Fiorina 1994) and because Carey, Niemi, and Powell (2000) argue that, of the various resources available to highly professionalized state legislatures, a high salary is the most important contributor to incumbent electoral success. Our empirical findings change very little with this alternative professionalism measure. (See Table 2 in our unpublished supplement.)

⁶ We consider the elected branches to have primary responsibility unless the courts are formally charged with redistricting, or legislatures are operating under direct court order.

⁷ Cox and Morgenstern (1993, 1995) contend that operating expenditures should be measured per constituent rather than per member in order to reflect the resources available to legislators for casework. We concur that such a measure appropriately taps the potential for casework, but casework is just one of several avenues that highly professionalized legislators use to enhance their chances of reclection. For our purposes, a per-member measure is superior. (When we estimate our model using operating budget per constituent as the mdicator of professionalism, the results change from highly supportive of our hypotheses regarding professionalism to starkly inconsistent. [See Table 1 in our unpublished supplement.] Even the well-established result that as professionalism rises, so does the probability of reelection, is turned completely on its head when the per-constituent measure is used, with a finding that members of less professionalized legislatures have a better chance of being reelected. We believe this confirms our view that budget per constituent fails to tap well legislative professionalism as conceived in this study.)

⁸ The cases with professionalism scores at or near one are extreme outliers; the mean value of professionalism is .090, and the standard deviation is .115. Budgets for legislatures with biennial sessions vary enormously from one year to the next, and to make such budgets comparable to those for legislatures with annual sessions, we average the per-member budget during the year of the election and the same value during the previous year.

ContestPrev is a dummy variable indicating whether a legislator had an opponent in the previous election (1 = yes, 0 = no); when she did, $Margin_{t-1}$ is her margin of victory in that race (see Appendix A for details). EconCondit denotes national economic conditions. Following Kramer (1983) and Bloom and Price (1975), we operationalize economic conditions by the percentage change in real income per capita during the year before the election, adjusted for ease of interpretation (via linear transformation) so that zero represents the poorest observed conditions, and one denotes the best.9 To facilitate the specification of differential effects of economic conditions by party, we employ a dichotomous variable, NotPresParty, which indicates whether a legislator is from the party opposing the president (1 = yes, 0 = no).¹⁰

Coattail effects are specified using several variables. OwnPartyPresVote% equals zero if there is no presidential election in a given year; if there is, it equals the percentage of the two-party presidential vote received by the candidate's party in the state. OwnPartyGubVote% and OwnPartySenVote% refer to gubernatorial and senatorial elections and are defined similarly. PresElecYr (also GubElecYr and Sen-ElecYr) is a dummy variable that equals one if there is a presidential (gubernatorial/Senate) election in the year, zero otherwise. The effects of redistricting are modeled with three zero or one dummy variables. AfterRedist equals one for the first election following a statewide redistricting. OwnPartyRedist equals one for an election held in a district that was formed when a legislator's party had full control over the redistricting process. OppPartyRedist equals one if the district was established when the opposition party had complete control.

Finally, several dichotomous variables measure district electoral rules. Term length is measured by Term2Yr, which equals one if a legislator is serving a two-year term, zero for a four-year term. District type is measured by two variables. SMDist equals one if the incumbent is competing in a single-member district, zero otherwise; PostDist equals one if she is running in a post election, zero otherwise; incumbents from true MMDs are scored zero on both these dummies.¹¹

EMPIRICAL ANALYSIS

We estimated equation 1 using data for all major party state legislators competing in general elections between 1970 and 1989 for whom information is available

⁹ The comparison is based on the difference between third-quarter income data for the election year and the previous year.

from ICPSR (1992) (n=42,820).¹² In its original format, coding problems make this data set very difficult to use for testing our model. Individuals serving multiple terms are often identified differently across records (e.g., with and without middle initials, with and without nicknames), a problem compounded by numerous typographical errors. Thus, it is difficult to trace a particular legislator from election to election, which is essential for calculating margin of victory in the previous election. We "cleaned" this data set, establishing a common spelling for all names likely to indicate the same person.¹³

We employed two estimation strategies to deal with potential statistical problems relating to the pooling of observations across states. In one, we added a set of dummy variables for the states. In the other, we calculated Huber (robust) standard errors (White 1980). 14 The two estimations yielded nearly identical substantive conclusions. Although both models fit the data well, we report the results with state dummies because that model had a slightly better fit. 15 (Complete results for the alternative model are reported in Table 3 of the unpublished supplement.)

The estimates for the probit coefficients are reported in Appendix B. We have no direct interest in these coefficients, however, since they characterize the effects of independent variables on a hypothetical unbounded latent variable assumed to be measured by our observed (win/lose) binary variable (Aldrich and Nelson 1984). What interests us is the influence of independent variables on the probability that a legislator will win. Such effects can be estimated because the coefficient estimates in Appendix B can be used to compute the predicted probably of winning (and an associated confidence interval) for an incumbent legislator who had any combination of values for the independent variables (King, Tomz, and Wittenberg 2000). Then,

¹⁰ We restrict our sample to major party candidates. (Our replication data set [Berry, Berkman, and Schneiderman 2000] reports the full set of ICPSR party codes treated as Democrat and Republican.)

¹¹ Data sources for all variables are presented in Appendix A.

¹² We exclude Minnesota and Nebraska in those years m which they had nonpartisan legislatures because many of our hypotheses assume a partisan environment. We exclude Lousiana because its system of run-off elections results in no general election candidates in some districts in certain years.

¹³ We wrote several computer programs that implemented various algorithms for identifying when two similar but different names likely indicate the same person. The instances identified were then subject to visual screening by the authors and several graduate student assistants. To assess the success of our efforts, we took a random sample of 401 cases from those in the data set for incumbents competing in general elections. (Each case is a particular legislator in a particular year.) Of the legislators in our sample for whom there was at least one other observation in the data set, using "uncleaned" names, 35% had at least one instance in which their name was recorded differently. When "cleaned" names were used, the figure dropped to 6%.

¹⁴ We employed the *probit* procedure in *Stata* (version 6.0), specifying the state as the "cluster" variable.

is To measure fit, we used a procedure recommended by Beck, King, and Zeng (2000). It sorts observed legislators into "bins" based on their predicted probability of winning and then compares within bins the mean predicted probability of winning with the proportion of legislators who actually win. (See Document 1 in our unpublished supplement for details.)

for each independent variable, we can estimate the effect of that variable on the probability that an incumbent will be reelected by determining how much the probability of winning changes when the value of the variable is adjusted while all other independent variables are held constant at "central" or "typical" values. 16 By central values, we mean the mode for all dichotomous variables (except for the state dummies) and the average for all others.¹⁷ This means our "typical" legislator is competing for a two-year term, in a single-member district, in an election that does not follow a statewide redistricting; he is not of the president's party and is isolated from any coattail effects (due to the absence of any concurrent higher level elections). We compute all predicted probabilities assuming that a legislator is from what might loosely be termed the "median state" in our sample; by this we mean the state that has the median state dummy variable coefficient estimate (Vermont).

The estimates of effects derived from predicted probabilities are shown in Table 1.18 Column 1 presents probabilities of winning for legislators under various hypothetical conditions. Column 2 reports differences between probabilities for two legislators with different values on one independent variable—say, X—but the same value for the remaining variables; these probability differences measure the effect of X on the probability of winning. Finally, column 3 reports differences between two probability differences in column 2. These "differences of differences" are used to evaluate the support for hypotheses concerning interaction with legislative professionalism by assessing the extent to which the effect of a variable on the probability of winning varies depending on the level of professionalism.

In general, our model is well supported by the data. The only hypothesis that fails to receive any empirical support concerns district type (hypothesis 10). There is no evidence that legislators from SMDs have an easier time winning reelection than those running in post elections or MMDs. In fact, the predicted probability of victory is nearly identical in SMDs and post elections and just about .01 higher in MMDs. (To save space, we do not append to this assertion the phrase "when all other independent variables are fixed at 'central' values." All claims in this section about the response of one variable to a change in another are subject to this same condition.) Cox and Morgenstern (1995) found

that the advantage of incumbents running in SMDs over those competing in MMDs declined from the 1970s to the 1980s. When we modify our model to allow the effect of district type on the probability of winning to vary over time, we still find no appreciable influence of district type at any point during the period of analysis.¹⁹

We find support for hypothesis 11. Legislators serving two-year terms have a probability of winning that is .034 higher than those with four-year terms.²⁰ We also detect a redistricting effect: A party with complete control over redistricting seems to be able to use its power to bolster its members' chances of winning the next election. The members of such a party have a .029 higher probability of winning than legislators from the opposition party, which confirms hypothesis 12.

Our model permits us to assess the electoral implications of being unopposed in the previous election. A legislator without opposition in a given race has a .988 probability of winning her next race. She has a large advantage over a legislator who won his previous race by the barest majority, whose probability of winning is only .871. She even has a substantial advantage over someone whose previous margin of victory is at the median, who has a .946 probability of victory. Indeed, our probit results allow us to calculate the margin of victory that is the functional equivalent, in terms of a legislator's prospects in the next election, of having run unopposed previously. An incumbent who ran unopposed has the same probability of winning his next election as a candidate with a victory margin of .55 (e.g., someone who won a two-person contest 77.5% to 22.5%). Only when an incumbent's victory margin was overwhelming (i.e., greater than .55) is she better off than a legislator who ran unopposed. These results support hypothesis 1.

We now turn to our central hypotheses about the effects of legislative professionalism. Consistent with hypothesis 7, we find that for the typical state legislator an increase in the level of professionalism of his institution substantially increases his chances of reelection. When all other independent variables are held at central values, an increase in professionalism from its lowest value across states (.00) to a value two standard deviations above its mean (.32) yields an increase of .063 in the probability of winning. This suggests that members of highly professionalized legislatures are able to capitalize on the resources available to them to

¹⁶ In a probit model, the effect of each independent variable on the probabilities of the two outcomes varies depending on the values of all independent variables.

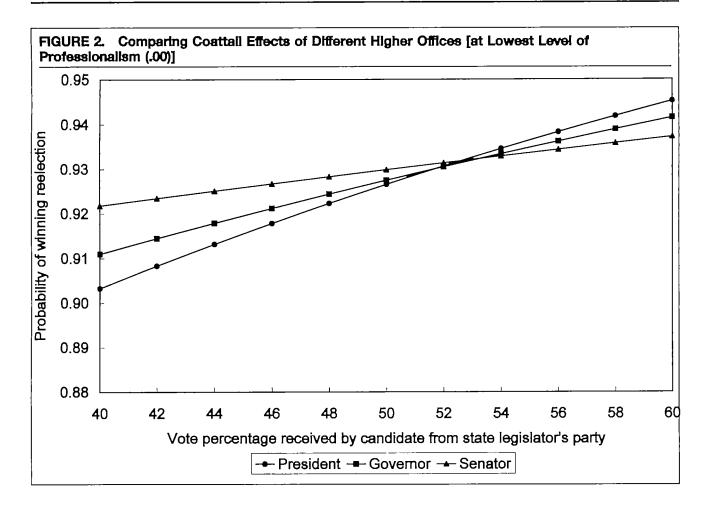
¹⁷ The central value is 0 for *PostDist*, *AfterRedist*, all election year dummies, and all coattail variables, 1 for *Term2Yr*, *SMDist*, *ContestPrev*, and *NotPresParty*, 090 for *Profess*; and .496 for *EconCondit*. *Margin* is skewed to the right, making the mean (.298) a somewhat deceptive measure of central tendency; thus, we treat the median (.228) as the central value.

¹⁸ There is extremely strong multicolinearity in the model; empirical tests showing that our results are insensitive to a variety of changes in model specification are summarized in Appendix A.

¹⁹ This is true whether we specify that the effect of district type interacts with (1) a year counter or (2) a dummy distinguishing the 1970s from the 1980s. (See tables 12 and 13 of the unpublished supplement.)

²⁰ In some social science applications, a probability difference of .034 would be considered very small. But bear in mind that incumbent defeats are extremely rare; 93% of legislators in our sample were reelected Thus, in a randomly selected group of 100 legislators, only 7 would be expected to lose. Our results imply that in a group of 100 legislators with four-year terms, 3 more are expected to lose than in a comparably sized group with two-year terms. Given the rarity of losses, this is a substantial difference.

Condition*	(1) Predicted Probability ^b of Reelection (with 95% conf. Int. In parentheses)	(2) Difference in Probability (with 95% conf int. In perentheases)	(3) Difference of Difference (with 95% conf int. In perentheses)
Effect of District Type	p	ar padriational	. paraticiocoy
Single member	.946 (.925, .963)		•
Poet	.949 (926, .967)	013* (.006, .021)	
Multtmember	.959 (940, .973) ——	—	
Effect of Term Length			
Two years ^o	.948 (925, 983)		
Four years	911 (.677, 930)	034 (080,022)	
Redistricting Effect Redistricting Effect Redistricting Effect Redistricting Effect Redistricting Effect	047 / 000 066		
Redist done under divided control	.947 (.922, .966) .946 (.925, .963)	029* (050,008)	
Redist. done under control of opposite party	.918 (.883, .946) ——		
Effect of Being Contested In Previous Race			
Contested & Margen = 00	.871 (.832, .906) ——	117* (086, .150)	
Unopposed	.988 (.981, .993)		
Contested & Margin = .23 ^{o,d}	.946 (925, .963) ——		
Effect of Professionalism (at Median Value of Margan o	•••		
Profess = .00 Profess = .32*	919 (888, 945)	.063* (.031, .096)	
1	•	.003 (.031, .090)	
Effect of Previous Margin and Interaction with Professi Margin = .00, Profess = .00		_	
Margin = .00, Profess = .00 Margin = .40, Profess = .00	.832 (.779, .877) 958 (.940, .973)	<u> </u>	
Mergin = .00, Profess = 32°	.939 (.886, .974)	.064* (024, .101)	072* (120,018
Margan = .40, Profess = .32	.994 (.985, .998)		
Presidential Coattalis: OwnPartyPresVote% = 40, Profess = .00 OwnPartyPresVote% = 60, Profess = .00 OwnPartyPresVote% = 40, Profess = 32° OwnPartyPresVote% = 60, Profess = 32°	.902 (867, 932) —— .945 (.921, 965) —— .977 (961, 968) —— .981 (.965, .992) ——	.043* (.028, .069)	039* (0 6 3,018
Guibernatorial Coattaila:	010 / 077 0000		
OwnPartyGubVote% = 40, Profess = .00 OwnPartyGubVote% = 60, Profess = .00	.910 (.877, .939)	<u> </u>	
OwnPartyGubVote% = 40, Profess = .32°	.972 (.963, .986)	.031* (020, .044)	021* (040,002
OwnPartyGubVote% = 60, Profess = .32	.982 (.968, .991)		1
Senatorial Coattalis:			
OwnPartySenVote% = 40, Profess = .00	.921 (888, 948) ——	¬	
OwnPartySenVote% = 60, Profess = .00 OwnPartySenVote% = 40, Profess = .32*	937 (.911, .968)		01 7* (0 3 0,008
OwnPartySenVote% = 60, Profess = .32	.982 (.962, .994)		
Effect of Economic Conditions and Interaction with Pro When member of president's party and <i>Profess</i> = .0		rdent ,	
EconCondit = .00 (worst)	.793 (.735, .846) ——	7 4477 (004 457)	
EconCondit = 1 00 (best)	.910 (.873, .940) ——	<u> </u>	
When member of president's party and <i>Profess</i> = 3 EconCondit = .00 (worst)		067* (021, 114)	0 6 0 (120, 010)
EconCondit = 1.00 (worst)	.921 (.855, .967) —— .979 (.964, .993) ——		
When not of president's party and <i>Profess</i> = 00:	· · · · · · · · · · · · · · · · · · ·		
EconCondit = .00 (worst)	.945 (.919, .966) ——	\neg	
EconCondit = 1.00 (best)	.884 (.844, 923)		7
When not of president's party and Profess = .32°:			048* (.009, .0 8 5)
EconCondit = 00 (worst)			J
EconCondit = 1.00 (best)	.974 (.943, .991)		
* Indicates "difference in probability" (in column 2) or "diffe" in all analyses, all variables not explicitly mentioned where All predicted probabilities, differences, and confidence in (Tomz, Wittenberg, and King 1998) * This is the condition of our "typical" legislator. * 23 represents the median level of Margin.	n describing a condition are f	fixed at the "central" value specifie	d in note 17. x B using CLARIFY softwar



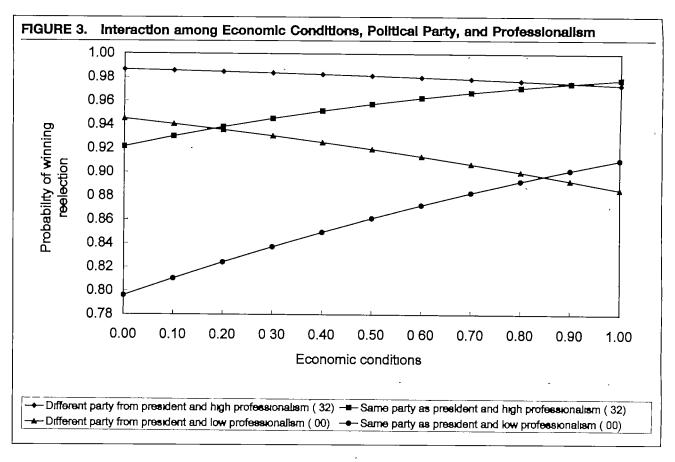
lower their electoral vulnerability. But the effect of professionalism on the electoral success of incumbents proves to be much more pervasive. We find evidence that a high level of legislative professionalism insulates incumbents from the effects of higher level elections, economic conditions, and even their own historical weaknesses (as reflected in a low previous margin of victory). These results support our thesis: Legislative professionalization promotes the establishment of boundaries necessary for legislative institutionalization.

Let us consider our findings in greater detail. Not surprisingly, there is evidence that at any level of professionalism an incumbent's previous margin of victory is positively related with her chances of reelection (hypothesis 2). But, as hypothesis 9 suggests, the effect of previous margin declines substantially as the level of legislative professionalism rises. When professionalism is at its lowest value, an increase in previous victory margin from the narrowest possible to .40 increases the probability of winning by .127. But when professionalism is two standard deviations greater than the mean, the same increase in previous victory margin boosts the likelihood of winning just .054 (i.e., .072) less). At this high level of professionalism, even legislators who won their previous race by the barest of margins can overcome their historical weaknesses and win at a rate of 93.9%.

There is also strong support for our theory of coattails.21 Consistent with hypothesis 4, presidential coattails are the strongest, senatorial the weakest. Figure 2 provides greater detail than Table 1 by showing (for legislatures at the lowest level of professionalism) predicted probabilities that a state legislator will win across a range of higher office election outcomes, from 40% to 60%, for the legislator's party. The way the figure is constructed, the greater the slope of the curve associated with a higher office, the greater is the coattail effect of that office. The slope is greatest for presidential elections (for which an increase in party vote percentage from 40% to 60% yields an increase in the probability of victory of .043), just slightly weaker for gubernatorial races (for which the same increase in party vote percentage prompts an increase in winning probability of .031), and substantially weaker for senatorial contests (for which there is a probability increase of only .016).

The effect of legislative professionalism in buffering coattail effects, as predicted in hypothesis 8(a), is evident in Table 1. At the lowest level of professional-

²¹ There is just one inconsistency between our estimates in Table 1 and our hypotheses about coattails. Contrary to hypothesis 3, at a sufficiently high level of legislative professionalism, the effect of a senatorial election outcome on the outcome of a state legislative race drops to zero and then becomes negative as professionalism rises.



ism, an increase in presidential candidate support from 40% to 60% results in an increase in the probability of winning of .043. When professionalism is two standard deviations greater than the mean, an identical increase in presidential candidate support leads to an increase of only .004 in the probability of winning, for a difference in the amount of increase of .039. The comparable difference in increase is smaller, but still appreciable, for both gubernatorial races (.021) and Senate contests (.017) (see column 3 of Table 1).²²

We also find support for the hypothesized effects of economic conditions. Figure 3 shows for legislators of both the president's party and the opposition, and for those in both low- and high-professionalism contexts, how the probability of winning changes as economic conditions improve from their lowest value (.00) to their highest (1.00). The strong empirical support for our hypotheses about the effects of economic conditions is evident from the similarity of the graphs in figures 1 and 3.

At both levels of professionalism, when economic conditions are at their worst, legislators of the presi-

dent's party have a lower probability of reelection than legislators from the opposition party (hypothesis 6[i]). Consistent with hypothesis 5, however, a strengthening of the economy increases the probability of winning for legislators of the president's party and decreases the probability of victory for candidates from the opposition. And at both levels of professionalism, when economic conditions improve from worst to best, the resulting increase in reelection probability for a member of the presidential party (.117 in the low professionalism context, .057 with high professionalism) exceeds the magnitude of the resulting decrease in probability for a member of the opposition (.061 for low professionalism, .013 with high professionalism). This confirms hypothesis 5(ii): The effect of economic conditions on the probability of election success is stronger for members of the president's party than members of the opposition. This result is consistent with our differential responsibility model of economic voting, which assumes challengers from the president's party are held responsible for economic conditions by voters, but to a lesser degree than incumbents of that party. The result is inconsistent with both the dominant model in the voting literature, which suggests that such challengers share fully in the credit or blame given the presidential party, and Hibbing and Alford's (1981) model, which presumes that challengers are not held responsible at all.

Finally, for legislators from either the president's party or the opposition, the effect of economic conditions is dramatically weakened by a high level of

²² Ideally, the coattail variables should be based on the vote shares received by higher office candidates in the state legislator's district. Since those data are not available, we use statewide vote shares, which should tend to diminish estimated coattail effects in states where vote shares for higher level elections vary substantially across legislative districts. We would expect this in larger, more heterogeneous states. Since such states also tend to have more professional-need legislatures, our finding that coattail effects weaken with a rise in professionalism could partially be due to our use of state-level vote shares.

professionalism, which is consistent with hypothesis 8(b). This is evident in the differing slopes of the curves in Figure 3 and in the pattern of predicted probabilities in Table 1. For example, for members of the presidential party, an improvement in economic conditions from worst to best increases the probability of winning by .117 in the low professionalism context but by only .057 in the high professionalism context, a difference of .060. The comparable difference for members of the opposition is a similar .048.23

CONCLUSION

The large sample allowed us to test many extant hypotheses about legislative elections and to control for a wider range of variables than has been possible in previous research. We confirm Carey, Niemi, and Powell's (2000) finding that state legislators with shorter rather than longer terms are more likely to win reelection. We also corroborate several results observed primarily in studies of Congress. We find that control over redistricting allows a party to help its incumbents win reelection, although the advantage gained is not huge. We confirm, too, the importance of the historical strength of an incumbent: The margin of victory in the previous race has a strong influence on the probability of reelection. Indeed, we go even farther and establish that incumbents who are challenged and win big (i.e., by a margin greater than .55) have better electoral prospects than those who are not challenged at all.

Our research clarifies several lingering issues in the legislative elections literature. We now have a much clearer sense of the relative effects of different higher office elections: Presidential coattails are longest, followed by gubernatorial and then senatorial. We also see that the effects of economic conditions are not as straightforward as suggested by either the responsible party model or the restricted in-party culpability theory. Consistent with our differential responsibility model, voters hold challengers from the president's party responsible for economic conditions, but to a smaller extent than the party's incumbents.24

Most critically, our research shows in great detail

23 As stated in note 4, the estimated coefficient for the margin variable may capture some effect of the excluded variable, historical party strength. Furthermore, we acknowledge in note 22 that the interaction detected between the coattail variables and level of professionalism may be spurious due to our need to measure coattails with state-level vote shares. It is, therefore, comforting that there is strong empirical support for our hypothesis that the effect of economic conditions on the probability of winning weakens as level of professionalism increases. If our general argument about the buffering effect of professionalism (i.e., hypothesis 8) were wrong, and if we had found that coattails and previous margin interact with level of professionalism only because of measurement and specification errors, we would not expect to find that the effect of economic conditions on the probability of incumbent victory declines as professionalism rises.

²⁴ We can draw conclusions about challengers from the president's party without observing them directly because the competing hypotheses about the effects of economic conditions on such challengers predict different effects of economic conditions on the probability of reelection of incumbents from the opposition party, and these effects

can be estimated using our data (see page 4).

how legislative resources alter the dynamics of legislative elections. Members of highly professionalized legislatures have a greater probability of winning than members of less professionalized bodies. Moreover, a high level of professionalism buffers incumbents from external political forces and the effects of national economic conditions. Consider a year in which an incumbent Democratic president presiding over a strong economy wins reelection by a landslide. Because national forces strongly favor his party, Democratic incumbents will hold a distinct advantage over Republican incumbents in an unprofessionalized state legislature, and we can expect a substantially larger percentage of Republicans to lose. In highly professionalized legislatures, however, Democratic incumbents will have significantly less advantage over their Republican colleagues, because members of both parties are insulated from national forces and have a high probability of victory. We find that professionalism even helps protect incumbents from their own historical weaknesses by reducing the effect of their previous margin of victory on the probability of their reelection.

By identifying clearly the buffering role of legislative professionalism, our findings speak directly to a large literature on congressional elections that finds the effect of national forces, including both coattails and the economy, has weakened over time. The relatively limited number of congressional elections makes it difficult to confirm a causal link between legislative resources and this weakening, but our large data set shows that an increase in state legislative resources does mitigate the effects of both coattails and economic conditions on election outcomes. Our results strongly suggest that the declining influence of national forces on congressional elections has been caused by increases in congressional resources.

By conceiving of professionalism as a critical contextual variable, we contribute to the development of a general theory of American legislative election outcomes that can explain both congressional and state races. As legislatures professionalize, the relationships between members and the external environment changes: Elections are less likely to be affected by political and economic conditions outside incumbents' control. Thus, professionalism makes possible the development of boundaries that protect the membership from external shocks and changes. These boundaries are an essential element of legislative institutionalization.

These results offer strong but conditional support for Salmore and Salmore's (1993) contention that state elections have become more like congressional ones; we find that this is the case only when state legislatures are more professional and therefore look more like the Congress. For those state legislatures that continue to professionalize, we can expect further decline in the influence of national forces over state legislative elections, so that these will look even more like congressional elections.

On the surface, this projection may seem at odds with claims by Brace and Ward (1999) and Rosenthal (1998) that state legislatures are deinstitutionalizing.²⁵

²⁵ Some of what these authors call demstitutionalization (e.g., reduc-

Our findings imply, however, only that if legislative professionalism rises and all other factors influencing the level of institutionalization remain constant, the level of institutionalization will increase as legislative boundaries strengthen. In fact, certain forces are contributing to lower levels of professionalism in some state legislatures. In particular, citizens have imposed legislative deprofessionalization in some states via initiatives that limit legislative resources. For example, citizens in several states voted in the 1990s to restrict the length of legislative sessions, and in California Proposition 140 cut the legislature's budget and staff (Rosenthal 1998). Anti-institutional sentiment among citizens combined with the availability of the voter initiative in some states has reduced the level of legislative professionalism.

This same sentiment has led to voter-imposed term limits in some states, which directly contribute to legislative deinstitutionalization in several ways. First, virtually by definition, they weaken legislative boundaries by shrinking the probability of reelection to zero for some. Second, they reduce the attractiveness of a legislative seat to career-oriented politicians, thereby lowering the stake legislators have in erecting the boundaries that shield them from external political and economic shocks. Finally, we contend that, even in the absence of term limits, the mere existence of the initiative option weakens the boundaries of legislatures in some states by allowing the external environment to intrude in ways over which legislators have virtually no control. In these states, constitutional rules may make it impossible for the legislature to maintain strong boundaries in the face of anti-institutional sentiment among the public.

APPENDIX A

Operational Definition of Margin of Victory

A legislator's margin of victory (Margin) is based on the difference between his vote share and that received by the losing candidate with the most support in the district. In SMDs the margin is measured in the traditional manner: the proportion of the vote the winner receives minus the proportion garnered by the second-place candidate. (We treat districts that are multimember with positions as if they were SMDs because each candidate is competing for a single designated seat.) In true MMDs, Margon = (A - B)/(C/D), where A = the number of votes received by the legislator, B = the number of votes received by the losing candidate with the most votes, C = the total number of votes cast in the district's election, and D = the number of seats selected in the district. In effect, margin of victory in MMDs measures how "badly" an incumbent beat the strongest losing candidate, relative to the average number of votes cast for a seat in the district. Thus, in either a SMD or a MMD, Margon can be viewed as an indicator of how close a legislator came to losing.

Data Sources

WinNext. Variable 31 from ICPSR (1992).

tions in legislative salaries) we would describe as deprofessionalization. Of course, our theory would predict that deprofessionalization causes deinstitutionalization. **Profess.** Legislative budgets from State Government Finances, selected years. Number of members in legislatures from Book of the States, selected years. Nominal budgets converted to 1967 dollars using the Consumer Price Index for all urban consumers (U.S./all items), series CUUR000AAU, taken from the site: http://stats.bls.gov/cgi-bin/surveymost.

ContestPrev. Constructed from variable 17 from ICPSR (1992).

Margin. For candidates running in SMDs, variable 28 from ICPSR (1992). For those in MMDs, constructed from data in ICPSR (1992) as described above.

OwnPartyPresVote%, OwnPartyGubVote%, and OwnPartySen-Vote%. Scammon (1995). (For Vermont in 1968 and Virginia in 1973, we assume OwnPartySenVote% = 50 to make for a "neutral" coattail effect. In Vermont, the winning candidate had the nomination of both major parties. In Virginia, although there was no Democrat in the race, both the Republican and independent candidates had been Democrats.)

EconCondit. Real income in 1987 constant dollars from Survey of Current Business, September 1994, Volume 74, no. 9; population from 1995 Statistical Abstract of the United States.

NotPresParty. Constructed from variable 14 from ICPSR (1992) (see note 10).

AfterRedist, OwnPartyRedist, and OppPartyRedist. Information about redistricting in the 1960s and 1970s is from Book of the States (various years), Bushnell (1970), and Hardy, Heslop, and Anderson (1981); information about the 1980s is from Basehart and Comer (1991) and a table titled "1980s Redistricting Authority" based on a 1985 National Conference of State Legislatures (NCSL) survey provided by Tim Storey of NCSL on June 13, 1997. (See our replication data set for details about construction of these variables.)

SMDist, PostDist. Constructed from variable 10 from ICPSR (1992).

Model Specification and Multicolinearity

The presence in the model of numerous multiplicative terms involving legislative professionalism leads to very strong multicolinearity: Regressing Profess on the remaining independent variables in equation 1 yields an R^2 in excess of .99. Were it not for the huge sample size, this degree of multicolinearity undoubtedly would be crippling. Despite the multicolinearity, our results prove robust with regard to changes in model specification. There are three interactions involving professionalism in equation 1, with previous margin of victory, coattail effects, and economic conditions. We estimated equation 1 dropping all combinations of two of the three interactions at a time, so that only one interactive effect was specified, and all other effects were assumed additive. These "submodels" faced less severe colinearity and generated coefficient estimates with lower standard errors. But the coefficient estimates for the submodels consistently led to substantive conclusions about the interaction specified that are very similar to those based on the full model. (See tables 5, 6, and 7 in our unpublished supplement.)

To see whether our results were sensitive to the precise functional form of the interactions concerning legislative professionalism in equation 1, we estimated four additional models, each with a "second-order" product term involving the square of the professionalism variable: (ContestPrev) (Margin)(Profess²), (OwnPartyPresVote%)(Profess²), (OwnParty

GubVote%)(Profess²), or (OwnPartySenVote%)(Profess²). These allow the effects of previous victory margin and the coattail variables on a latent variable assumed to be measured by the win/lose dichotomy to vary nonlinearly with the level of professionalism. (Equation 1 constrains the effects of these variables to vary linearly with professionalism.) The conclusions about our hypotheses remain virtually unchanged in all four alternative specifications. (See tables 8 through 11 of our unpublished supplement.)

APPENDIX B. Probit Coefficient Estimates for Equation 1

Independent Variable	Coefficient Estimate	Standard Error	t-ratio
Profess	1.61	0.70	2.31
ContestPrev	-1.13	0.04	-26.53
ContestPrev × Margin	1.93	0.08	22.92
ContestPrev × Mergin × Profess	1.56	0.70	2.23
P res⊟e cYr	-0.72	0.10	-7.15
Pres⊟ecYr × Profess	1.34	1 16	1.15
OwnPartyPresVote%	0.0153	0.0019	7.86
OwnPartyPresVote% × Profe s s	-0.035	0.021	-1.62
Gub⊟ecYr	-0.51	0.10	-5.22
Gub⊟ecYr × Profess	-0.18	1.01	-0.18
OwnPertyGubVote%	0.0112	0.0018	6.07
OwnPortyGubVote% × Profess	-0.007	0.018	-0.39
Sen <i>E</i> lecYr	-0.219	0.077	-2.85
SenElecYr × Profess	0.91	0.78	1.16
OwnPartySenVote%	0.0058	0.0014	4.09
OwnPartySenVote% × Profess	-0.0215	0.0147	-1.47
EconCondit	0.53	0.07	7.61
Profess × EconCondit	0.32	0.63	0.50
NotPresParty	0.79	0.06	13.41
NotPresParty × EconCondit	-0.93	0.11	-8.20
NotPresPerty × Profess	0.15	0.52	0.29
NotPresParty × EconCondit × Profess	0 01	1 01	0.01
AfterRedist × OwnPartyRedist	0.011	0.050	0.22
AfterRedist × OppPartyRedist	-0.210	0.057	-3.69
Term2Yr	0.256	0.034	7.63
SMDist	-0.137	0.037	-3.73
PostDlst	-0.103	0.066	-1.57
Number of cases	42,820		
-2(log-likelihood ratio)*	3736.41		
Pseudo R ²	0.167		

Note: All results were obtained using the probit procedure in Stata (version 6.0). Coefficient estimates for state dummy variables are excluded (but are reported in Table 4 of the unpublished supplement).

* Distributed as ohi-equare with 74 degrees of freedom.

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Xenophon's Philosophic Odyssey: On the *Anabasis* and Plato's *Republic* JACOB HOWLAND *University of Tulsa*

enophon's Anabasis, a military adventure interwoven with a story of philosophical self-discovery, is a companion piece to Plato's Republic. The Anabasis takes up in deed the two great political problems treated in speech in the Republic, namely, how a just community can come into being and how philosophy and political power may be brought to coincide. In addressing the first of these problems, Xenophon makes explicit a lesson about the limits of politics that is implicit in the Republic. He speaks to the second problem by clarifying the essential role of philosophical eros in his emergence, at the moment of crisis, as the founder and leader of a well-ordered community. Xenophon's self-presentation in the Anabasis, which makes clear his debt to Socrates, illuminates the nature of philosophical courage as well as the saving

Tt is safe to say that the name of Xenophon was much more well known to earlier generations of readers than it is to our own. In antiquity, Xenophon was widely admired as a man of considerable parts, whose virtues of intellect and character were displayed in a noble harmony of speech and deed. Romans and Greeks alike regarded him as an exemplary warrior, a model of political leadership, an eloquent orator, and an inspired author. Xenophon's abilities as a political thinker and leader were also quickly recognized when the study of Greek was revived in Europe at the end of the Middle Ages (Dakyns 1890, xl).

integrity of the philosophical soul.

Xenophon's reputation stemmed largely from the events related in the Anabasis, a tale of courage and endurance that did not fail to impress the most ambitious individuals of later ages. The Greek historian Polybius, writing in the second century B.C.E., suggests that Alexander's expedition into Asia never would have occurred apart from the example of the successful retreat of the Ten Thousand from Persia.1 Plutarch (1968) records that when Mark Antony's army was suffering from famine, fatigue, and sickness on campaign against the Parthians, "Antony, they say, often uttered 'O the Ten Thousand!' in amazement at those who were with Xenophon" (Antony 45.6). Dio Chrysostum (1950), a Greek author of the first century C.E., goes so far as to recommend Xenophon to aspiring orators on the ground that he "alone among the ancients, is able to suffice for a political man" (Discourse 18.14). Dio praises Xenophon's speeches in the Anabasis for their hortatory power, prudence (phronesis), and cunning, and he notes that the persuasiveness of his words derives from his "not having learned from hearing, nor having imitated others, but having done deeds as well as talked about them" (Discourse 18.17). The philosophical statesman Cicero (1952) and the orator Quintilian (1954) were no less impressed with Xenophon's mastery of the written word. The former calls his style "sweeter than honey" and records that "the Muses were said to speak with the voice of Xenophon" (Cicero, Orator 9.32, 19.62); the latter observes that "the Graces themselves seem to have molded his style . . . [and] the goddess of persuasion sat upon his lips" (Quintilian, Institutio Oratoria 10.1.82).²

Niccolò Machiavelli and Francis Bacon held Xenophon in similarly high esteem. Machiavelli seems to have regarded him as the wisest of the ancients with respect to the political things. It has been observed that he "mentions Xenophon in the Principe and the Discorsi more frequently than he does Plato, Aristotle, and Cicero taken together"; what is more, Xenophon's Education of Cyrus is the only book cited by name in the Prince (Strauss 1991, 103, n. 3; Nadon 1996, 361, n. 3). For his part, Bacon ([1859] 1963) holds up "Xenophon the philosopher" as a prime example, alongside Alexander and Caesar, of "the concurrence of military virtue and learning." He marvels at Xenophon's sudden transition during the march of the Ten Thousand from being an object of "scorn" to one of "wonder," and he concurs with the judgment of the ancient historians that Alexander's achievements rested "upon the ground of the act of that young scholar" (Bacon [1859] 1963, 313–4).3

After the Renaissance, the *Anabasis* became the primary text for beginning Greek students, and an Oxford professor writing as recently as 1949 found no need to review the story of the Ten Thousand because the *Anabasis* "has been read by generations of British schoolboys" (Howell 1949, 3). Such a claim could hardly be made today. Xenophon also is not read in translation as a matter of course by students of classical philosophy and political theory, perhaps because his writings meet the disciplinary expectations of the modern academy less clearly than those of other ancient authors (Wood 1964, 37–40). The virtues for which

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¹ Polybrus 1993, *The Histories* 3.6.9-11. See the comparison of Xenophon and Alexander in Arrian, *Anabasis of Alexander* 1 12.2-4. Against all odds, Xenophon led roughly ten thousand mercenaries back to Greece from the heart of Persia after the army failed to overthrow Emperor Artaxerxes.

² A detailed discussion of the classical critical tradition with regard to Xenophon is provided in Dakyns 1890, xvii—xvii.

³ Xenophon was probably a little less than thirty years old when he joined the expedition of Cyrus (Mather and Hewitt 1962, 23).

Xenophon was held in high esteem by the most discerning ancient and early modern students of politics have escaped the notice of most contemporary academicians (but see Strauss 1970, 1972, 1975, 1991). Recent interest in the Education of Cyrus suggests that the tide may be beginning to turn, but much of the new scholarship is marred by inattention to the "cutting nuances and subtlety" of Xenophon's writing (Nadon 1996, 362). The Anabasis, in any case, continues to be neglected by philosophers and political theorists alike. Thoughtful readers of this forgotten masterpiece will agree that "Xenophon is ripe for rehabilitation" (Goldhill 1998).

This article is rooted in the conviction that the Anabasis deserves to be rediscovered as a literary and philosophical classic. Xenophon invites the reader to join him in a voyage of philosophical self-discovery, the shape and meaning of which emerges most clearly when the *Anabasis* is studied as a companion piece to Plato's Republic. This approach, which to my knowledge has not been explored, is suggested by the recognition that Xenophon's text recapitulates the central themes and issues of Plato's most well-known dialogue. The parallels between the two works are further enriched by the use of Homer's Odyssey as a subtext in both. All three of these texts tell the story of a homecoming of the soul as well as the body, a homecoming that unfolds on an intellectual and metaphysical level as well as the literal level of physical reality. Although Xenophon's narrative also converses with the History of Herodotus, Aeschylus' Persians, and the *Iliad*, a full exploration of his literary debts is impossible in the present context. I propose instead to follow the most direct route into the meaning of the Anabasis, a route marked out by Plato's Republic.

The Anabasis is on one level the story of a military adventure that accidentally assumes the characteristics of the Republic's main theme: the creation of the good city. Xenophon takes up in deed the two great political problems treated in speech by Plato, namely, how a just polity can come into being and how philosophy and political power may be brought to coincide. Xenophon addresses the first problem by showing us the conditions for the emergence of an actual community, the retreating Ten Thousand, that is as nearly well ordered as a political community can be. But a well-ordered community is not necessarily a genuinely just community, such as the Republic's Kallipolis aspires to be, that is, a community in which devotion to the common good springs from the intrinsic virtue of its members. Xenophon makes it clear that the ephemeral unity of the Ten Thousand is rooted in nothing other than the urgent need for self-defense against external enemies; when this urgency fades, order inevitably gives way to factional strife. In explicating the fragility of order among the Ten Thousand, the absence of a common good that transcends self-defense, and thus the attenuated sense in which even this extraordinary political body exemplifies justice, the *Anabasis* makes explicit a lesson about the limits of politics that is implicit in the Republic. Yet, this lesson takes shape differently in each text. Plato's Socrates seems to be aware of the

limits of politics from the moment he proposes to found a city in speech, but Xenophon learns of them only through harsh experience and as a result of the actual outcome of events. His experiences, however, lead him to define these limits more sharply than Socrates does. In particular, the *Anabasis* repeatedly underscores the difference, and indeed the tension, between political order and justice, a theme only indirectly visible in the *Republic*.

The Anabasis is also, and most significantly, the story of Xenophon's intellectual and moral growth. It traces the path of his personal appropriation of the wisdom of Socrates, whom Xenophon leaves behind in Athens when he joins the expedition of Cyrus (Anabasis 3.1.4-7).4 It is furthermore in terms of his manifestation of Socratic or philosophical courage that we must understand Xenophon's response to the second great problem treated by Plato. At the center of the Republic, one encounters a seemingly intractable opposition between eros and thumos, or philosophical passion and spiritedness (especially as evinced in anger, and in the pursuit of honor and victory [Republic 586c-d]). Socrates invites us to ponder the relationship of these two dimensions of the soul when he makes it clear that the genuine philosopher will not wish to rule and, conversely, that the type of person who is attracted to ruling offices will not be inclined to philosophize. The Anabasis speaks to this problem by clarifying the critical role of philosophical passion in Xenophon's emergence as the founder and leader of a well-ordered community. Xenophon presents himself as a timely, and therefore admittedly temporary, solution to the problem of the relationship between philosophy and politics. The key to this solution lies in the fact that Xenophon's spiritedness, far from asserting itself as potentially antiphilosophical thumos, is indistinguishable from the energy intrinsic to philosophical eros. The power of the latter to overcome the partiality of thumos and to unify the soul is accordingly a central theme of Xenophon's self-presentation in the Anabasis. Xenophon emphasizes that his philosophical disposition graces his soul with a saving integrity. His political and military success is nevertheless merely the penultimate stage in this odyssey of self-discovery. His ultimate achievement of Socratic self-knowledge follows the decay of the community of the Ten Thousand and is anticipated by his eventual decision to exchange the seductions of military and political activity for the deeper pleasures of quiet reflection.

XENOPHON AND PLATO

A few preliminary observations about the relationship between Xenophon and Plato are in order. First, the existence of a Symposium and an Apology of Socrates in the corpus of both Xenophon and Plato constitutes prima facie evidence that these authors were acquainted with each other's writings. Ancient commentators took it for granted that this was the case. One

⁴ Unless another title is indicated, all subsequent citations are to book, chapter, and section numbers of the *Arabasis*.

ancient tradition holds that Xenophon wrote the Cyropaedia in response to the first books of the Republic; another maintains that the Athenian Stranger's remarks about Cyrus in Plato's Laws (694a-695b) were meant as a criticism of the Cyropaedia (Diogenes Laertius 1991, Lives of Eminent Philosophers 3.34; Aulus Gellius 1952, Attic Nights 14.3.3-4).

Second, the core of my argument is largely independent of chronological considerations. My thesis that the *Republic* and the *Anabasis* are companion pieces need not rest upon the assumption that the former was the earlier work. For example, Plato may have added passages to the *Republic*, or otherwise revised it, in the light of his reading of the *Anabasis*. It is less likely that Plato began to write the *Republic* after reading the *Anabasis*. Even so, both works can be viewed as variations on a common set of themes and issues, as meditations that are best appreciated in tandem, just insofar as one of them takes the measure of the other.⁵

Finally, although Xenophon's ancient readers do not explicitly link the *Anabasis* with the *Republic* as they do the *Cyropaedia*, the plausibility of such a connection is strengthened by the fact that all three works are essentially concerned with war, tyranny, and especially the relationship between erôs and thumos. Indeed, the latter problem stands, both literally and thematically, at the center of the *Cyropaedia* as well as the *Republic*.

The third "wave of paradox" of book 5 of the Republic, according to which political power and philosophy must coincide, breaks at the exact center of the text (Howland 1998, 633). This is said to be the "biggest and most difficult" of the three waves of paradox (Republic 472a4), presumably because it demands a reconciliation of a range of humanly fundamental oppositions, including those between spiritedness and erotic love, the longing for honor and the longing for wisdom, public welfare and private affection, the political production of civic order and the philosophical discovery of truth. Prior to the third wave's introduction of philosopher-kings, the city in speech is distinguished by the suppression of erôs and the rule of well-tempered thumos. Thus, the essential qualification for rule in books 3 and 4 is political (as opposed to philosophical) courage—steadfastness in preserving "right opinion" (orthe doxa), particularly the conviction (dogma) that one must do what is best for the city (cf. 412c-e with 429d-430c). Immediately after the third wave, however, the theme of erôs explodes into the dialogue: The philosopher is essentially characterized by an erotic passion for the whole of truth and wisdom, a passion that Socrates describes by borrowing the language of sexual attraction and consummation (474d-476d, 485a-b, 490a-b). The rulers of the just city must somehow overcome the oppositions that

come to a head in the third wave; they must, as Socrates says, be "best in philosophy and with respect to war" (543a5-6, emphasis added).

Similarly, Xenophon introduces the tension between thumos and erôs at the literal center of the Cyropaedia when he acquaints us (at the very end of book 4 and the beginning of book 5, in a text comprising eight books) with the character of Panthea, by reputation the most beautiful woman in Asia. Cyrus, whose governing desire is to rule a great empire, refuses even to gaze upon Panthea because he fears becoming a slave to erotic attraction (Cyropaedia 5.1.8, 12, 16). He wishes to avoid such slavery, in spite of its attendant pleasures, because it would prove fatal to his political ambitions. In Cyrus's view, erôs and thumos cannot cooperate within the soul: One must always be the absolute master of the other. Cyrus's manly pursuit of victory and honor therefore requires his utter rejection of the charms of beauty.

The story of Cyrus and Panthea dramatizes the issue at the heart of the problem of philosophical rule. The meaning of this story becomes clear when it is considered with a view toward the erotic philosopher of the Republic—to say nothing of a similar episode in Xenophon's Memorabilia (3.11), wherein Socrates, when told of the beauty of the courtesan Theodote, rushes off to see her. By turning a blind eye to beauty and a deaf ear to what he regards as the siren-song of erôs, Cyrus renders himself impervious to the educative power of music, which has everything to do with "love matters [erôtika] that concern the fair" (Republic 403c6-7), and so to philosophical music in particular. If this suggestion is well taken, then the Athenian Stranger's criticism that Cyrus "failed completely to grasp what is a correct education" (Plato, Laws 694c6-7) turns out to be one that Xenophon hopes thoughtful readers will have understood on their own.

THE REPUBLIC: POLITICS IN SPEECH

The Republic is narrated by Socrates and starts with a physical journey. Its first word is "I went down" (kateben). Katabasis, the noun meaning "going down," is the opposite of anabasis, or "going up"; the latter term refers specifically to a journey inland from the sea, or up country. The Piraeus, to which Socrates went down "yesterday" with Glaucon in order to observe the festival of Bendis (and from which presumably he has returned as of "today"), is the port of Athens. According to Socrates, his anabasis—his attempt to go back up to Athens from the sea—is interrupted by Polemarchus (Republic 327c-328b), and he ends up spending all night in the Piraeus in philosophical conversation with some young men. In the place of a literal ascent in deed, the Republic provides an ascent in speech to the just city and to the highest objects of philosophical inquiry, namely, the Ideas and the Good. In the course of this ascent, there is furthermore much talk of going down and going up. The theme of ascent and descent appears in the myth of Gyges' ring in book 2, in the famous cave image of education in book 7, and in the myth of Er in book 10, in which Er goes down to Hades

⁵ Xenophon's use of the imperfect tense at 5.3.7–10 indicates that at least this portion of the text was written after his expulsion from Scillos (Dakyns 1890, kvi, cxxxi). It is thus reasonably certain that Xenophon was still working on the *Anabasis* after 371 (Dillery 1995, 59 with 264, n. 1) According to the standard view of Platonic chronology, this means that Xenophon was still working on the *Anabasis* after the *Republic* was in all likelihood complete (Guthrie 1987, 437; Ross 1951, 2; but see the criticisms in Howland 1991).

and then returns to the land of the living. Finally, the theme of ascent and descent is reflected in the shuttling, dialectical movement of philosophy as depicted in the Divided Line—movement up to the Ideas and the Good and back down to concrete conclusions (*Republic* 511b-c).

As can be seen from the preceding list, the image of physical descent and ascent is meant to suggest motion on the level of the soul. It is in terms of this motion that we must understand both Socrates' katabasis at the beginning of the Republic and the action of the dialogue as a whole. Plato makes it clear that the Republic is a philosophical epic in which Socrates struggles to rescue his companions from a kind of living death, the condition of the unexamined life, within the horizons of which the practice of tyrannical injustice, whether open or disguised, seems indispensable to supreme happiness (344a, 362a-c). To begin with, the dramatic setting of the dialogue in the home of Cephalus in the Piraeus leaves little doubt that we are initially, as Eva Brann writes, "in the city of shades, the house of Pluto": in the name Piraeus (especially in the accusative phrase eis Pevaia, which Socrates employs in the first line of the dialogue) one hears peraia or "beyond-land"; Bendis is a deity of the underworld; and one ancient source indicates that Cephalus, who is said to be on the threshold of death (Republic 328e6), actually died at least twenty years before 421, the earliest dramatic date most scholars are willing to assign to the Republic (Brann 1989-90, 8-9). Furthermore, the dialogue takes place under a cloud of war and imminent tyrannical violence. Three of the ten characters who appear in the Republic—Polemarchus, Niceratus, and Cleitophon-will be murdered by the Thirty Tyrants who seize power in Athens in 404 (Krentz 1982, 79-81; Rahe 1977, 198).

Yet, although the conversation begins in the city of shades, it concludes with a return to light and life. The myth of Er ends when Er awakens on his funeral pyre at dawn (Republic 621b); as it happens, it is probably around dawn when Socrates finishes telling Er's tale at the very end of the Republic. The journey traced by the sun in the background of the dialogue as it moves below, and ultimately reemerges from beneath, the earth's surface seems to be a cosmic analogue of the movement of philosophy itself (Rosenstock 1983, 220-1). What is more, this imagistic depiction of education as the ascent of the soul from the land of the dead to the land of the living, from darkness to light, and from confusion to clarity, is well established by the time of Plato. It is the basic structure of the Greek religious rites of mystery initiation, such as those practiced at Eleusis (Burkert 1983, 248-97). It appears in Parmenides' famous philosophical poem, written perhaps fifty years before the birth of Plato. Most important, it is reflected in the Odyssey, in which Odysseus passes beyond the boundary of death and, renewed by ultimate wisdom, returns to the land of the living.6

THE ANABASIS: POLITICS IN DEED

The Anabasis, too, narrates a circular journey.⁷ As in the Republic, the motif of homecoming in the Anabasis is intertwined with the themes of a descent toward literal and spiritual death and a political and philosophical ascent. With regard to political matters, the plan of composition in the Anabasis is as follows: in books 1, 2, and 3, increasing danger and ascent from everyday political reality to the well-ordered regime; in book 4, the perfected warrior-community of the Ten Thousand; in books 5, 6, and 7, return to safety and descent from the well-ordered regime to everyday political reality. With regard to Xenophon's personal odyssey, the Anabasis describes an ascent toward self-knowledge. Let us now consider the shape of the Anabasis in more detail.

The full title of Xenophon's book is Kurou Anabasis, or Ascent of Cyrus; its subject is not Cyrus the Great, whose story is told in the Cyropaedia, but Cyrus the Younger. The title obviously has political overtones. Cyrus had hoped that his march up country to do battle with his older brother Artaxerxes, who succeeded Darius in 405, would conclude with his ascent to the Persian throne. The geographical going-up of Cyrus and his army of Persians and Greek mercenaries turned out to be a moral, political, and psychological going-down: Cyrus was killed at the battle of Cunaxa in 401; the Persians in his force abandoned the Greeks and went over to Artaxarxes; and the five main Greek commanders (including Clearchus, Proxenus, and Meno), together with twenty other officers, were murdered shortly thereafter through the treachery of the Persian commander Tissaphernes. The leaderless Ten Thousand found themselves surrounded by a vast army in the heart of enemy territory, an enemy whose hostility was intensified by two invasions of Hellas during the preceding century that ended in humiliating defeat at the hands of the Greeks. Xenophon and the Greek mercenaries had followed Cyrus straight down, so to speak, into a living hell. All of this is accomplished in the first two books of the *Anabasis*.

Tyranny

The Greeks are not, however, simply victims of circumstance. Xenophon makes it clear that the mercenary disunity of the army played an essential role in bringing about the extraordinarily dangerous situation faced at the end of book 2, and this disunity must in turn be understood in relation to the theme of tyranny. This point emerges most clearly when the *Anabasis* is compared with the *Republic*. Both books begin with the

wander about in a kind of moral and intellectual no-man's-land (Howland 1993, which develops Sogal 1978, cf. Lachterman 1990). The structure of the *Republic* also recapitulates that of the *Odyssey* in certain essential respects; see Howland 1993, 47–54.

Socrates is depicted in the Republic as a philosophical Odysseus: He attempts a homecoming of souls that have been blown off course by the stormy winds of appetite and ignorance and that consequently

⁷ Lossau (1990, 51) notes that the *Anabasis* relates a journey that begins and ends in the vicinity of Sardis, and the last words of the book, "Thibron . . . went to war against Tissaphernes and Pharnabazus" (7.8.24), recall the pretext of making war against Tissaphernes that Cyrus used to collect his army at the very beginning of the work (1.1.6).

orienting assumption that the tyrant (or the Persian equivalent, the oriental despot) is the happiest of men.⁸ This assumption is in the first place made by Cyrus, whom Xenophon represents as exercising absolute power and acting with license. Like the possessor of Gyges' ring, Cyrus sleeps with a married queen, takes what he wishes through violence, and puts to death whomever he wants (1.2.12, 19-20). As if to underscore what is revealed in these deeds, Xenophon quietly alludes to two myths that caution against hubris—the flaying of the satyr Marsyas by Apollo and the story of Midas—just before and just after mentioning that Cyrus was said to have slept with the Cilician queen (1.2.8, 13-14; on the greed of Midas, cf. Aristotle, Politics 1257b). Such allusions would find a receptive audience among Athenian readers, whose understanding of the Persians was decisively shaped by Aeschylus' depiction of Salamis and Plataea as the outcome of hubris nurtured by tyranny and luxury (Persians 800-42). Xenophon deliberately builds upon Aeschylus' politically foundational muthos, within which Greek freedom and independence come to light as the antithesis of the Persian character.9 By making it clear in his "eulogy" that Cyrus's tyrannical character was an outcome of his education (1.9), Xenophon in fact criticizes the Persian political culture as a whole.¹⁰

If Cyrus believes that tyranny is the most desirable life, he also supposes that the summit of tyrannical power, and so the apex of happiness, is achieved by the Persian emperor. To judge by its frequent appearance in the dialogues of Plato, this assumption is a commonplace in the Greek cities as well (Sophist 230d-e; Gorgias 470c; Apology 40d-e). For their part, the Greek mercenaries who join Cyrus's expedition suppose that the best things in life may come from being useful to a tyrant. Not surprisingly, in the first part of the Anabasis the Greeks act like nothing so much as a band of thieves. The army is a nest of quarreling factions, divided according to the home cities of the soldiers, and is composed of the kind of men who are ready to stone their own commanders when it seems to suit their self-interest (cf. 1.5.11-7 with 1.3.1-2).11 In these respects, the situation at the beginning of the Anabasis reminds one of the quarrelsome condition of the Greek army at the beginning of the Iliad. By the

same token, the obedience of the men in book 1—like that of the Achaean soldiers in Homer's *Iliad*—is won not by an appeal to the common good but by promises of increased personal wealth or power and by harsh discipline (or the threat thereof).¹²

In certain respects, Xenophon suggests, the army begins to undergo a transformation after Cyrus's death at the end of the first book. Yet, he also makes it clear that the army remains fragmented in a way that leads directly to even greater danger. Before Cyrus's death, the army functions simply as a mercenary band; its goal is determined by Cyrus, in whose pay it labors. After Cyrus's death, it can no longer function in the same way, for it is no longer in the employ of anyone. Unlike other armies in normal conditions, including those made up of citizen-soldiers, the Greek soldiers are now entirely independent and must henceforth determine their own goals. They are in this sense "a city on the move" (Cawkwell 1967, 55; Dalby 1992, 17), a "quasiepic warrior band" (Dillery 1995, 76), like that of Odysseus and his men on their way home from Troy. What is more, because the immediate danger to the Greeks increases with the death of Cyrus, the soldiers begin—at least intermittently—to recognize a new kind of authority. In particular, Xenophon writes that Clearchus, a notoriously harsh Spartan officer, is now willingly obeyed as the supreme commander because the soldiers "saw that he alone knew what sorts of things a leader ought to do, while the rest [of the generals] were inexperienced" (2.2.6). As the sequel makes clear, however, some men must even in these perilous circumstances be compelled to obey. Clearchus knew from experience how to employ a mixture of shame and force in motivating the troops. He shamed men into working harder by jumping into the mud himself when it was time to make bridges; at the same time, he commanded with a spear in one hand and a stick in the other, dealing blows whenever he found someone shirking (2.3.11–3).

In spite of Clearchus' leadership, "normal" mercenary disunity persists in the army even after the death of Cyrus. Most important in this regard is the perceived treachery of Meno. When Cyrus is killed, Ariaius, a Persian commander who was fighting alongside the Greeks, abandons them and flees with his men. Meno later leaves the rest of the army and stays with Ariaius (2.2.1). Clearchus, who had quarreled earlier with Meno (1.5.11-7), suspects that Meno is scheming to hand the Greeks over to Tissaphernes, a deed that would not be out of character for him (2.5.27-9; cf. 2.6.21–9). This is the bait Tissaphernes uses to trap the Greek commanders: He states that, if Clearchus will come to him, he will reveal those who are attempting by slander to undermine the position of the Greeks (2.5.24-6). Meno's apparent treachery and Clearchus'

The tyrant rules persons who regard themselves as citizens entitled to a share m political power, whereas the despot rules persons "more slavish in their characters than Greeks," for which reason "they put up with a master's rule [tên despotikên archên] without any ill-will" (Aristotle, Politics 1285a20-23). Yet, Persian rule seems to be despotism only in name: Although even Cyrus is referred to as the "slave" of Artaxerxes (1.9.29, 2.5.38), he is an unwilling one.

⁹ Compare Xenophon's remark to the assembled soldiers, "You bow down to no human being as master, but rather to the gods" (3.2.13), with *Persians* 241–2: "Who is it that is shepherd and master over their army? They [the Greeks] are slaves to no man, nor are they subjects." ¹⁰ The common assumption that Xenophon idealizes Cyrus (see, e.g., Delebecque 1947, 97) is undermined by the contradiction between his apparent praise in 1.9 of Cyrus's education and character and what he has plainly indicated in his prior account of Cyrus's deeds. ¹¹ The army is composed not so much of Greeks as of Thessalians, Boeotians, Megarians, Arcadians, Achaians, Lacedaemonians, and Athenians; as such, it is a miniature image of Hellas in its rivalries and jealousies (Delebecque 1947, 47–8).

¹² Clearchus punished his men severely, in accordance with the maxim that "the soldier ought to fear his commander more than the enemy" (2.6.10). On the use of promises of wealth and power, cf. 1.3.21 and 1.4.11–3 with 1.4.13–6 and 1.7.2–9. Note that the band of thieves furnishes the model for politics according to Thrasymachus (*Republic* 343b ff.; cf. 351c).

imprudent desire for revenge thus lead almost directly to the complete destruction of the Ten Thousand.

The mercenaries' initial assumption about the profit to be gained in associating with a tyrant is refuted practically, if not theoretically—by the death of Cyrus, whose head and hand are stuck on pikes by his brother (1.10.1), and by the subsequent betrayal, torture, and beheading of the five main Greek commanders. After these murders, at the end of book 2, the title of Xenophon's book takes on a different color. The goal of the Ten Thousand is now, first and foremost, simply to survive; with respect to the well-being of their bodies and souls they have nowhere to go, so to speak, but up. It now seems that the genitive Kurou in the title Kurou Anabasis is to be read not subjectively but objectively: the story is no longer one of Cyrus's ascent but of an ascent occasioned by Cyrus and his failed quest for power. Even more inviting is the possibility that Xenophon may mean something like ascent from (i.e., above or beyond) Cyrus, an interpretation that applies well to the story of his own development as a nontyrannical ruler.13 The ascent of the community of the Ten Thousand from disintegration and death occurs under the leadership of Xenophon, who, having watched and learned from the strengths and shortcomings of Cyrus, Clearchus, Proxenus, and Meno, rises suddenly to a position of command. Xenophon's political ascent goes hand in hand with the moral and psychological renewal of the community of the Ten Thousand. The turning point or peripeteia for the Ten Thousand occurs in book 3, in the course of the first, fitful night after the murder of the Greek commanders.

Community

Before book 3, Xenophon mentions himself by name only more or less in passing (1.8.14-7, 2.4.15, 2.5.37-42). It is at the beginning of book 3 that he chooses to speak more fully about himself and the events leading up to his journey into Asia, including in particular his consultation with Socrates. The mention of Socrates prepares us to hear the peculiar resonance of his language. Xenophon begins book 3 by observing that the Greeks were "in great perplexity" (3.1.2). The word he uses is aporia, which literally describes a condition of seeing "no way out"; it is also the hallmark of a philosophical encounter with Socrates (Xenophon 1959, Memorabilia 3.10.7, 4.4.5; cf. Plato 1979-82, Theaetetus 149a). Separated from their homeland by the impassable Euphrates and Tigris rivers and by a distance of more than ten thousand stades, surrounded by enemies, and without horsemen, a market, or a guide, all of them, including Xenophon, were demoralized or "spiritless" (athumôs: 3.1.3). Few of them ate or kindled a fire that night, and many, not even coming into the camp, just lay down by themselves wherever they happened to be. In sum, the army is at this point nothing more than a disorganized mass of individually helpless atoms.

"When the time of perplexity [aporia] came," Xenophon continues, he himself was restless but did manage to catch some sleep (3.1.11). It is in fact a propitious moment, if one considers that a confrontation with death can sometimes give birth to unprecedented insights. So it is with Xenophon, who dreams that lightning from Zeus sets his father's house ablaze. To Xenophon, this emphasizes the difficulties (aporiôn: 3.1.13) that face the Greeks and spurs him to action. He awakens and rises to give three speeches. The first is addressed to the captains (lochagoi) of Proxenus and the second to all the surviving officers. In the course of the second speech, Xenophon suggests a way to encourage the soldiers: "If we can turn their minds around [trepsê(i) tas gnômas], so that they may think not only of what they are going to suffer but also of what they are going to do, they will be in much better spirits [euthumoteroi]" (3.1.41). Xenophon then is elected a general (archôn) in place of Proxenus. The last speech, which is addressed to the whole Greek army, contains a radical plan of action and organization that is ratified by a general vote, and it proves to be the salvation of the Ten Thousand. Like the three waves of book 5 of the Republic, Xenophon's three speeches bring into being a new, saving regime (cf. Republic 497a) and introduce a leader whose claim to rule is bold intelligence.

The imagery Xenophon employs in his account of this long night is both familiar and powerful. The second speech, he tells us, takes place around midnight; the third, just as day is beginning to break (3.1.33, 3.2.1). He describes a movement from dark to light, from dreaming to wakefulness, from death to renewed life, from passivity to activity, from confusion to clarity, from perplexity to understanding. He deploys terms and images that structure the action of the Republic in general and the cave image and myth of Er in particular, terms that are themselves borrowed from the poetic tradition and the religious rites of mystery initiation.¹⁴ Xenophon's double task, moreover, resembles that of Odysseus: "to win his life [psyche, literally: soul] and the return of his companions" (Odyssey 1.5), to lead the way home from deep in hostile territory by concentrating his energies in a way that also makes possible the achievement of his own identity (on the connection between these two tasks in the Odyssey, see Dimock 1974). Not surprisingly, the adventures of this warrior band are several times explicitly compared to that of Odysseus and his men. As if to make clear the dependence of homecoming upon concentrated thoughtfulness, the first reference to the Odyssey occurs during Xenophon's first speech to the whole army, when he adverts to the fate of the lotus eaters (Odyssey 9.94 ff.), who ceased to think of their homeward journey (3.2.25). The Anabasis and the Republic share the same Homeric subtext (cf. Lossau 1990).

The regime brought into being by Xenophon's three speeches nonetheless originates in such a way as to raise important questions about the nature and extent

¹³ Cf. the example thanatou lusis, "release from death," given in Smyth 1980, § 1332.

¹⁴ On the turning of the soul to which Xenophon speaks at 3.1.41, cf. Republic 518c-519b.

of justice in any possible regime. In the Republic, Socrates suggests that the just community could come into being if everyone over the age of ten left the city, so that the as yet uncorrupted youth could be educated to justice. In practice, however, forced expulsion would be necessary, so that a massive act of injustice would be required in order for the just community to come into being (Bloom 1967, 409-10). For its part, the Anabasis supports this inference about the "Machiavellian" foundations of the ostensibly just community. In the city in deed of the Anabasis as opposed to the city in speech of the Republic, accident substitutes for deliberate action in the foundation of the community. Xenophon suggests that if men are taken deep into the uncharted territory of a vicious enemy, if they face overwhelming numbers, and if by some chance their leaders are killed, then one of two things will happen: They will either strain every nerve to work together as a team, always looking out for the common good, or they will all die a miserable death. The emergence in the Anabasis of an orderly, unified, independent community, marked by high morale and a common sense of purpose, is thus made possible only by the brutal murder of the Greek commanders—an injustice, we may note, that is itself provoked by Cyrus's wrongful attempt to overthrow a legitimate ruler. Insofar as the existence of a well-ordered community (to say nothing of a genuinely just community) depends on the prior existence of injustice, however, the Anabasis teaches at a minimum that such a community cannot deliberately be brought into being without compromising its claim to justice.

There is furthermore something Machiavellian in the way in which Xenophon seizes the opportunity presented to him by fortune. He appeals to external pressure in order to accomplish reforms akin to those brought about by the use of the Noble Lie in book 3 of the *Republic*, which helps persuade the warrior class that they must not possess private dwellings or private property beyond what is strictly necessary (Republic 416d–417b). In book 3 of the *Anabasis*, Xenophon uses the threat of imminent death—not a lie, but the truth—to convince the Ten Thousand to adopt virtually identical recommendations. His main advice to the soldiers is to throw away all the private wealth they have collected by pillage and keep only the tools of war; burn their tents and wagons; help the officers administer punishment when it is merited; and march together in a hollow square (3.2.27-32, 3.2.36-7). He explains that this plan will allow the army to move more effectively, but one suspects that these measures, like their counterparts in the Republic, are designed also to help unify the army by keeping attention focused on the common tasks of returning to safety. They may assist in doing so by minimizing the temptation and the opportunity to act for private gain or to divide into factions at the expense of the whole, by ensuring that the soldiers will be always in the public eye; and, insofar as individual soldiers, as in the Kallipolis (Republic 464e-465b), will have a hand in dispensing punishment, they will allow the troops to satisfy their aggressive instincts in a way that will least threaten the interests of the whole.

Be that as it may, it is striking that Xenophon cannot prevent the soldiers from collecting animals, women, and boys along the way, to such an extent that many men are diverted from the task of fighting and the army as a whole moves too slowly (4.1.12-4, 4.3.19). The restrictions against accumulating goods that are not essential to war are soon relaxed (if not altogether abandoned), even though the danger faced by the army has not significantly diminished; thus, the army at one point seizes a load of "fine apparel and drinking cups" from the Kurds (4.3.25). As will become still more evident in the sequel, Xenophon's attempts to subordinate the private appetites of the soldiers to the achievement of public good ultimately work no better than the Noble Lie. Even in the Kallipolis, Socrates finds it necessary to win the allegiance of the best soldiers by rewarding them with frequent sex, choice cuts of meat, and distinguished honors (Republic 459d-460b, 468c-469b; Howland 1998, 645-9). In these respects, the spirited warriors of the Kallipolis—like the Ten Thousand—never quite cease to resemble the self-interested Greeks of the *Iliad*.

In accordance with Xenophon's advice at the very end of book 3, the army goes up into the mountains of Kurdistan and through Armenia, toward the source of the Tigris and the Euphrates. This geographical anabasis in book 4 coincides with another kind of ascent: the education and seasoning, through harsh experience, of the Ten Thousand. During this part of the journey, which concludes when the Greeks reach the Black Sea, the army is compelled to make its way through snowcovered mountain passes and to fight on a daily basis against valiant foes, including Kurds, Chaldaeans, Taochians, Chalybeans, Phasians, Macronians, and Colchians. In the end, no more than two-thirds of the Greeks who began the expedition with Cyrus could endure the extreme hardships of battle, frostbite, snowblindness, disease, and starvation so as to make it to the sea.¹⁵ This extraordinarily tough regimen perhaps serves to purge the weak and disorderly from the army. At all events, it calls to mind the difficult education of the souls and bodies of the Auxiliaries in the Republic, who are compelled to confront many "labors, pains, and contests" (413d4), insofar as it seems in Xenophon's initial estimation to provide an adequate training for citizenship. It is with the surviving soldiers, all "very fit [hikanous] through long practice," that he dreams of founding a city (5.6.15-6).

Disintegration

Book 4 is the central book of the *Anabasis*, and it describes the period in the career of the Ten Thousand in which the community achieves maximum order and harmony. The unity in book 4 is sufficient to overcome, at least temporarily, the extreme suspicion and hostility between Athenians and Spartans that obtain after the

¹⁵ At 1.2.9, we are told that Cyrus's Greeks number approximately 13,000. At 5.3.3, a review at arms yields a count of 8,600.

end of the Peloponnesian War and that erupt in the subsequent books. The playful banter between Xenophon and the Lacedaemonian Cheirisophus, for example (4.6.14-7), suggests remarkable confidence and energy, a point underscored by the friendly rivalry in virtue described at 4.7.8-12 and especially by the athletic games the Greeks hold to celebrate arrival at the Black Sea (4.8.25-7). These exuberant contests put one in mind of the games in which Odysseus participates when visiting the Phaeacians (Odyssey 8.100 ff.). This parallel is confirmed by the remark of Leon the Thurian at the very beginning of book 5, who now proposes, since the Greeks have finally reached the sea at the Greek city of Trapezus, "to cease from toil...and to arrive in Hellas stretched out, like Odysseus" (5.1.2). The allusion is to the journey from the island of the Phaeacians to Ithaca, which is accomplished while Odysseus sleeps in the boat (Odyssey 13.75–124).16

In alluding to the beautiful city of the Phaeacians, Xenophon also implicitly invites us to recall the noble and beautiful city of the Republic (Howland 1993, 52-3). But these allusions ultimately underscore the evanescence of the dream of the ideal polis. First, the island of the Phaeacians is not Odysseus' final destination; like the Kallipolis, it is an unplanned detour from the main path. No less important, the athletic contests of spirited warriors in book 4 of the Anabasis bear at least as much resemblance to the games in *Iliad* 23 as to those in *Odyssey* 8. The former games provide only a brief moment of relatively peaceful and well-ordered competition in a world of chaos. In repeatedly comparing the Ten Thousand to the quarrelsome warriors of the *Iliad*, Xenophon never quite lets his readers forget what he himself seems to have forgotten for a time.

Once at the Black Sea, the Greeks no longer have a sense of immediate external threat. The beginning of book 5 marks the start of a parabasis, a "going along" the coast toward the Hellespont, which extends through book 6. This geographical parabasis, however, is concurrent with a moral and political katabasis. Leon the Thurian's remark is a sign that a decisive transition has occurred, namely, the beginning of a return to ordinary political reality from a brief period of extraordinary order and unity. The remark is itself a virtual act of rebellion: Leon announces publicly that he is tired of discharging the duties of a soldier (Delebecque 1947, 56). Additional signs of disintegration appear almost immediately. Liberated by the apparent lessening of danger, the troops feel free to shout down Xenophon's suggestion that they make contingency plans to go by road (5.1.13-4). Xenophon finds it necessary to warn them about keeping good discipline, and he repeats this warning after some soldiers are killed while pillaging (5.1.8-9, 5.4.20). He nonetheless begins to dream of founding a "great" city (5.6.15-6; cf. 6.4.1-6). Because this dream contrasts starkly with the emerging political situation, it further emphasizes for the reader

the incomplete, fragile, and ephemeral nature of the order and unity achieved in book 4.

It is in fact Xenophon's Platonic ambition to craft a city from an army that brings him afoul of the soldiers: in confiding his dream, he falls victim to the greed of a seer, Silanus, and of two rival officers, Timasion and Thorax (5.6.17–26). For the first time, Xenophon is obliged to defend himself against accusations before the assembled troops (5.6.27), and he must do so again when the soldiers, reacting to slander against him spread by another officer, threaten to stone their commanders (5.7.1-33). Book 5 concludes with a public scrutiny of the behavior of the generals, equivalent to the Athenian practice of *euthuna*, or public accounting after a term in office. For the third time, Xenophon must undertake an apologia in the face of public accusations of injustice (5.8.1-26). The three defensive speeches in book 5 seem to present a reverse image of the three speeches by which Xenophon unites and energizes the Ten Thousand in book 3.

The situation parallels Socrates' account of the inevitable decline of the Kallipolis in book 8 of the Republic, which begins, perhaps not coincidentally, with a reference to the burning of the Achaean ships by the Trojans in the Iliad (Republic 545d-e; cf. Iliad 16.112 with Bloom 1968, 467 n. 4). Socrates explains the emergence of timocracy as the outcome of a quarrel between aristocrats and oligarchs, or lovers of virtue and lovers of money; these are precisely the terms in which Xenophon presents his conflict with Silanus, Timasion, and Thorax (cf. his references at 5.6.21 to what is "noblest" [kallista] and "best" [arista], together with 5.6.18, 5.6.21, and 5.6.25-26).¹⁷ The compromise that results from this quarrel is a regime ruled by war-lovers who secretly covet gold and silver (Republic 547c-548b). This is a fairly good description of the orientation of the Ten Thousand after they have been liberated from the fear of imminent death.

The parabasis along the Black Sea continues in book 6, toward the end of which the Spartans take control of the army (6.6.5). In book 7, the story returns in more than one sense to the place from which it began. As noted above, the conclusion of the Anabasis finds Xenophon in Pergamus, near Sardis, the city where Cyrus collected his troops and from which the march up country began (7.8.8; 1.2.5). More important, a new Cyrus comes into the picture in book 7. The remaining Greek force, now numbering 6,000 (7.7.23), is for the second time employed by a barbarian prince, Seuthes the Thracian. He proposes to use Greek mercenaries for the same purpose that Cyrus had in mind, namely, to conquer a kingdom that he regards as rightfully his (7.2.31–4). Like Cyrus, Seuthes lives by plundering his own lands (7.2.34). Like Cyrus, he wins the allegiance of the Greek troops by making promises of pay and territory that he ultimately cannot (or will not) fulfill (cf. 7.2.36–8 with 7.5.4–5 and 7.7.48–57; 1.2.11, 1.3.21, 1.4.13, 1.7.5-8). Most important, he is, like Cyrus, a tyrant. Under his rule, the lot of the Thracians is

¹⁶ Lossau also identifies Trapezus with Scheria, the island of the Phaeaccians, and notes the parallel between the games in both texts (Lossau 1990, 47).

¹⁷ Xenophon is nonetheless not insensitive to the attractions of money, as is clear from his robbery of the Persian Asidates (7.8 22–3).

slavery (douleia); they are "subjects" (hupêkooi) who obey him only out of necessity and who would attempt once again to become free were it not for their fear of him (7.7.29, 7.7.32). Cyrus punishes "most mercilessly" (apheidestata: 1.9.13), and Seuthes kills "without mercy" (apheidôs: 7.4.6); both men inspire fear in those they wish to rule by burning the land (1.4.10; 7.4.1, 7.4.5).

Xenophon's narrative comes full circle: It returns in the last book of the Anabasis to a world governed by a tyrant and guided by the assumption that such a man must be the happiest of human beings. This circularity emphasizes the intransigence of political reality and places in the broadest perspective the political significance of the adventures of the Ten Thousand. Xenophon makes it clear that what came into being all too briefly in the Anabasis was, after all, neither a fullfledged city nor a truly just and well-ordered community, although it was as well ordered, and reflected as much devotion to the common good, as any political community we are likely to encounter in the annals of history. As in the Republic, the ideal city—a city in which human beings are devoted to the common good at all times, not just when exigency compels such devotion, and in which the common good transcends self-preservation—must ultimately merely the wish of good men (cf. Republic 592a-b). The Anabasis, like the Republic, lets us see the limits of politics.

XENOPHON'S PHILOSOPHICAL COURAGE

The Anabasis shows that the well-ordered community of the Ten Thousand comes into being only through Xenophon's own activity as a political founder and ruler. In the Republic, Plato makes it clear that the paradoxical figure of the philosophical ruler must be understood in terms of the relationship between thumos and erôs. It is this seemingly vexed relationship that stands at the center of the Republic and, I submit, of the Anabasis as well.

Xenophon begins to speak about himself in the first portion of book 3, immediately after describing, at the very end of book 2, three of the Greek leaders murdered by the Persians. This is a clear invitation to the reader to compare his nature with the very different natures of Clearchus, Proxenus, and Meno. In what follows, I propose to show that the contrast between Xenophon and these three companions may best be understood in terms of Socrates' characterization of philosophical and nonphilosophical souls in the Republic. Although Xenophon provides no explicit discussion in the Anabasis of the nature of the soul in itself, his description of his three companions answers to the account of the tripartite soul Socrates sets forth in book 4 of the Republic, and his actions in book 3 of the Anabasis seem to amplify and develop an important Socratic hint about the difference between political and philosophical courage (Republic 430c).

The Anabasis tells the story of the emergence of a philosophical ruler. Xenophon's leadership in the army is philosophical because it is informed by the knowl-

edge of that which lies beyond the horizons of nomos (custom or convention) and because it is oriented by a good that is transpolitical. Xenophon's saving intelligence is first and foremost expressed in the speeches of book 3, speeches that give heart to the soldiers, establish good order in the ranks, and focus the energies of the army on a well-defined goal and plan of action. Yet, it is something more than intelligence that allows Xenophon to find his voice in the first place. As he makes clear in the story of his dream "from Zeus," he is sustained in the moment of crisis by great courage. It is his courage in the face of terrifying obstacles that opens up a space within which calm and collected reflection may occur. Courage is thus the indispensable precondition for Xenophon's philosophical rule. But what is the nature of this courage?

In book 4 of the Republic, courage is "officially" presented as the virtue of thumos, which stands in between the appetitive (epithumêtikon) and calculative (logistikon) parts of the soul (442b-c). Yet, this account of the soul is deficient to the extent that it abstracts from the love of wisdom, which enters fully into consideration only with the introduction of philosophical eros in book 5. Socrates indicates there is another kind of courage that transcends the "political" courage described in book 4, and I believe Xenophon manifests precisely this higher, philosophical courage. Political courage consists in clinging fast to orthodox opinion under adverse conditions and so exists only within the horizons of nomos, whereas Xenophon's courage comes into play in response to the destruction of these very horizons. Also, Xenophon's deeds in the moment of crisis are not motivated essentially by the thumotic longing for victory or honor or by indignation against the Persians. Rather, the interior energy upon which he draws is essentially erotic; one could just as well say—following his suggestion that he is spurred to action by a light from Zeus (3.1.12)—that he is drawn by it. Furthermore, Xenophon's philosophical erôs orders and guides his soul as a whole; hence his response to the dream is simultaneously spirited and intelligent, passionate and reflective. This complexity reflects the saving integrity of his psyche, which he pointedly displays in book 3 against the backdrop of the three partial or fragmented characters he describes at the very end of book 2. Finally, Xenophon's introduction of Socrates at the beginning of book 3 is meant to clarify the philosophical origins of his exemplary wholeness. In courageously saving himself and his companions, Xenophon displays in deed the Socratic wisdom he first acquired through speech.

Xenophon could not have predicted that his journey away from home and hearth would bring him back to his intellectual father. The circularity of the *Anabasis* as a voyage of self-discovery is described in a penetrating epigram composed by Diogenes Laertius (1991) and set forth in his *Lives of Eminent Philosophers* (2.58):

Not only on account of Cyrus did Xenophon go up [anebên] into Persia,

But rather because he sought whatever path might lead up [anodon] into the region of Zeus.

For, having shown that Hellenic deeds [Hellenika pragmata]

were the outcome of his education [paideia], He recollected how noble and beautiful [kalon] was the wisdom of Socrates.

Diogenes situates Xenophon's adventures in the Anabasis in the context of a journey of the soul, and he offers a matrix of pregnant oppositions within which to consider the significance of this journey. Xenophon seeks the way to "Zeus," or to that which is highest and best. He sets out for Asia thinking that Cyrus, the political figure par excellence, may be the path to this Zeus. His experiences, however, confirm the supreme merit of the philosophical path of Socrates, a point underscored by Diogenes' rhetorical juxtaposition of the phrases "region of Zeus" and "wisdom of Socrates." At the end of his journey, Xenophon is able to see in the clear light of hindsight what was already there at the beginning: His practical experience, including moments of failure as well as success, allows him fully to appreciate the excellence of his Socratic education (Bruell 1987, 111-4; Higgins 1977, 91-2). Indeed, in looking back Xenophon sees, not Socrates' philosophia (love of wisdom), but his noble and beautiful sophia (wisdom). Paradoxically, he recognizes that his quintessentially Hellenic political deeds are the outcome of Socratic, philosophical speeches. Finally, Diogenes leaves it open whether Xenophon reaches the goal of his journey in doing great deeds or in achieving the wisdom that comes to him through reflecting on the significance of his actions. The same ambiguity is present in the image of Xenophon as a sitting eagle that comes to him as an omen from Zeus at the outset of his voyage (6.1.23). The eagle is the king of birds and the sign of Zeus, but sitting or standing still is the posture of philosophical meditation (cf. Plato, Symposium 174d–175b, 220c–d).

Diogenes' insights into Xenophon are borne out by the text of the Anabasis. Let us first consider the inception of Xenophon's odyssey as related at the start of book 3. After an introduction that is both classical and biblical in wording ("There was in the army a man named Xenophon, an Athenian"), Xenophon tells us that he received a letter from his friend Proxenus, inviting him to accompany the Greek mercenaries and offering to introduce him to Cyrus, "whom he Proxenus] himself, he said, believed to be better [kreittô, literally: stronger] than his own fatherland [tes patridos]" (3.1.4). A few pages earlier, Xenophon offers a eulogy of Proxenus that begins by stating he was someone who, "ever since his youth, longed to become a man capable of doing great deeds" (2.6.16). To this end, he studied with the sophist Gorgias, after which, "thinking that he was capable of ruling," he went to Cyrus, with whom "he believed he would acquire a great name, and great power, and many possessions" (2.6.17). Xenophon does not tell us what his own motives were in wishing to join Proxenus, but it is evident that he, too, sees in connection with Cyrus something that seems to promise more than both Athens and Socrates can offer him. He chooses to go with Cyrus despite Socrates' warning that doing so will anger the Athenians, inasmuch as Cyrus had "eagerly

joined with the Lacedaemonians in making war against Athens" (3.1.5).

It has been argued that Xenophon, unlike Proxenus, was "not interested in ruling" and joined Cyrus not out of "any love of the political life" but for "frankly utilitarian considerations," namely, "as a means of gaining wealth and, perhaps, a safe haven in a world of a considerable amount of turmoil." From this point of view, Xenophon was "more or less compelled to take an interest in political rule [after the death of Cyrus], if only to save himself along with his fellow Greeks" (Ruderman 1992, 129–30). Xenophon's motivation in joining Cyrus was not that of Proxenus, but the latter interpretation does not square completely with what is revealed in the Anabasis. Xenophon consistently manifests an interest in power that seems to be at least partly philosophical, as well as a desire to become involved in important decisions.

Just before the battle of Cunaxa, Cyrus goes out in front of his army to survey the situation, and Xenophon rides out to ask whether he has any orders to convey (1.8.14–7). Strictly speaking, this is unnecessary, since Cyrus is surely capable of giving orders on his own or of making it known that he desires to do so. What is more, Xenophon arguably oversteps his bounds: It would be appropriate for Cyrus to convey any orders he might have through an officer, whereas Xenophon is, as he tells us later, "neither a general, nor a captain, nor a soldier" (3.1.4). His gesture may indicate a desire to serve the prince after the fashion of a lieutenant, or perhaps he wishes simply to observe Cyrus more closely so as better to understand him.

Subsequent passages confirm Xenophon's interest in participating in military and political decisions (2.4.15-8, 2.5.37-42). His thinking when he is offered sole command of the army in book 6 reveals an attraction to honor as well: "Xenophon in one way desired these things, believing both that greater honor would come to be his in the eyes of his friends, and that his name would be greater when it should reach his city, and also that he might chance to be the cause of some good for the army" (6.1.20). Xenophon is tempted above all by the reputation associated with the position of plenipotentiary commander, although his special concern with how he is seen by his friends indicates a more philosophical attitude toward honor than that of the most ambitious political men (cf. the discussion in Strauss 1991, 88, of the contrast in Xenophon's Hiero [1956] between Hiero's desire to be universally loved and Simonides' desire to be admired by "the competent minority"). The "chance" that Xenophon could benefit the army occurs to him almost as an afterthought. On the negative side of the ledger, moreover, Xenophon mentions only the possibility that he might lose the reputation he has already acquired (6.1.21).

In sum, Xenophon suggests that he goes with Cyrus because he is drawn to the splendor and honor of the political life (cf. Bruell 1987, 111; Higgins 1977, 91). Following Diogenes, however, we may suppose that his attraction to politics is more open-ended or experimental than that of Proxenus, and he is motivated essential.

tially by the possibility that along this path he may discover that which is highest and best. This understanding of Xenophon's initial motivation helps make sense of his behavior with respect to Socrates. Xenophon responds to Proxenus' invitation the way any ambitious young man would: He ignores the sober advice of his "father" concerning perhaps the most important decision he will ever make. Socrates tells him to consult with the Delphic oracle, but Xenophon, having already decided to go, asks only about the god to whom he should sacrifice in order that his journey might be a success; the answer is Zeus (3.1.5, 6.1.22). As is appropriate in a story about crossing the threshold of maturity and independence, Xenophon's headstrong decision coincides with the loss, to him, of his intellectual father: Socrates is executed while he is away.18

With respect to the difference between Socrates and Xenophon, Strauss (1975, 124) observes that "Xenophon was a man of action: he did the political things in the common sense of the term, whereas Socrates did not, but Socrates taught his companions the political things with the emphasis on strategy and tactics." Strauss (p. 139) asks: "Does not knowledge of rule need some iron alloy, some crude and rough admixture in order to become legitimate, i.e., politically viable?" Xenophon's departure from Athens stems from his longing for important action, and it is the spiritedness intrinsic to this longing that adds iron to his Socratic awareness.19 Yet, Xenophon's journey follows a pattern already established by Socrates. It is no accident that his odyssey begins from Delphi-and, in a deeper sense, from the repetition (through the dream sent by Zeus) of the opportunity to encounter the sacred that he effectively avoids at Delphi—for Delphi is the origin of Socrates' own Heraclean philosophical labors or wanderings (cf. 6.2.2 and 6.2.15 with Plato, Apology 22a).

One thing Xenophon brings with him to Asia is the Socratic openness that makes genuine learning possible. He accordingly writes the *Anabasis* as if it were a record not only of events but also of his realization of the significance of these events. This is especially apparent in his description of Clearchus, Proxenus, and Meno, which is offered immediately before he goes on to speak at length about himself (2.5.1–29). These men are all warriors by nature, which is to say that they all exemplify thumos, the goals of which are identified by Socrates as "the satisfaction of honor, victory, and

anger" (Republic 586d1). Yet, each represents a distinct inflection of thumotic ambition, answering to the three parts of the soul and city distinguished in book 4 of the Republic (440e-441a). Meno's thumos is associated with *epithumia* or appetite, the lowest level of the soul, and is directed at material gain, the object of the lowest part of the city: "It was clear that he strongly desired [epithumôn] to be rich, that he desired [epithumon to rule so that he might get more, and desired [epithumôn] to be honored in order that he might gain more" (2.6.21; cf. Aristotle 1979, Nicomachean Ethics 1159a17-21). Clearchus stands at the second level of the soul and city. He was, as it were, pure thumos: A harsh and vengeful man, he was "warlike and a lover of war to the ultimate degree" (2.6.1), and his aim was victory for its own sake. Proxenus embodies the highest part of the soul as it is understood in book 4 of the Republic, or before the introduction of philosophical intelligence in the third wave of book 5; at this stage of the dialogue, the ruling part is identified simply as logismos, or "calculation" (439d). Proxenus effectively put his trust in logos, insofar as his primary preparation for rule consisted in studying with the rhetorician Gorgias (2.6.16-7). He was a gentleman who desired above all the splendor of virtuous accomplishment: He sought great honor, power, and wealth, but "he thought that it was necessary to obtain these things with justice and nobility, but without them, not [to obtain them] at all" (2.6.18; cf. Republic 443e).

It is important to reiterate that Meno, Clearchus, and Proxenus exemplify the parts of the soul or city only as these are understood before the introduction of philosophy as a ruling principle in book 5 of the Republic. Each man is therefore in his own way deficient, both with regard to the order and integrity of his soul and with regard to the insight necessary for leadership at the highest level. It is philosophical erôs that binds the parts of the soul together into a well-ordered whole (cf. Republic 485a-487a), and it is philosophical insight that alone makes possible independent and authoritative judgment in situations of the most extreme exigency.

Let us again consider each of the three deceased commanders. Meno was ruled by an appetite for wealth unconstrained by justice or piety (2.6.21, 26). He prided himself on taking advantage of those who considered him a friend, and he sought to win the allegiance of his soldiers by participating in their wrongdoing (2.6.23–7). As already noted, Meno's divisive behavior had made Clearchus suspicious and so helped Tissaphernes lay a trap for the Greek commanders. Whereas the vicious Meno was ineffective at winning the trust of decent men, the virtuous Proxenus had the opposite problem: "He was able to rule gentlemen [kalôn men kai agathôn], but was not capable of putting respect or fear of himself into the soldiers" (2.6.19).20 Proxenus relied too much on the power of speech to guide men; by the same token, he

¹⁸ His decision may also have resulted in the loss of his fatherland, as Socrates seems to have warned (3.1.5); he is ultimately exiled from Athens (5.3.7), although scholars disagree as to whether the reason was his service to Cyrus or to the Spartan king Agestlaus (Anderson 1974, 147-9; Higgms 1977, 22-4).

¹⁹ The line between eros and thumos becomes blurred insofar as spiritedness is intrinsic to strong erotic attraction. Consider the story of Episthenes, a lover of boys, who appears before Seuthes in an attempt to spare the life of a young prisoner of war. Xenophon intercedes, citing Episthenes' past bravery in the company of handsome young men; Episthenes then offers his own life in exchange for that of the prisoner (7.4.7–11). He exemplifies on the level of ordinary experience the spiritedness intrinsic to Xenophon's extraordinary, philosophical eros.

²⁰ Meno, too, had been a student of Gorgias (Plato, *Meno* 71c-d), a fact that speaks to the moral indeterminacy and incompleteness of sophistry in comparison with Xenophon's Socratic education.

cared too much what others said and thought about him. On the one hand, he believed that the obedience of the men he commanded could be won by praise and blame alone; on the other, he feared incurring the hatred of his soldiers (2.6.19–20). With regard to Clearchus, Xenophon notes laconically that "it was said that he did not very much like to be ruled by others" (2.6.15). Thumos, however, is properly ruled by philosophical intelligence, and it is the lack of such ruling intelligence in Clearchus that leads directly to the murder of the Greek generals. Like the Guardians whom Socrates distinguishes from the Auxiliaries at the end of book 3 of the Republic, Clearchus was both older than the other generals (he was "about fifty years old" when he died, 2.6.15) and was characterized by great courage (cf. Republic 412c-e). Civic or political courage is the power to hold fast to governing orthodoxy, much as well-dyed wool keeps its color even under the most adverse conditions (Republic 429d-430c). Clearchus exemplified this kind of courage both in exhibiting bravery in the face of danger and in clinging to the ways and customs of his homeland. In particular, he was very pious; he even left an important meeting with the ambassadors of Artaxerxes in order to attend to the sacrifices (2.1.9). It was precisely this piety, however, coupled with his thumotic desire for revenge against Meno, that blinded him to the trap set by Tissaphernes. It did not occur to him that Tissaphernes swore a false oath of friendship, or that he was lying when he denounced those who are willing to perjure themselves before god (2.5.3, 2.5.21).

Whereas the ill-fated generals embody the three parts of the soul in abstraction from philosophical eros, Xenophon exemplifies the power of erôs to unify and guide the psyche. He underscores the essential difference between himself and the three generals in subtle ways. At 2.3.16, in the context of a discussion of edible plants that cause headaches (kephalalges), he observes that the whole palm tree withers when its pith—the part of the tree that is "in its head" (ho engkephalos)—is removed. This observation anticipates the Persian strategy of removing the heads or leaders of the army so as to destroy the whole (3.2.29). But this remark also appears to be a pun on the name of the *Republic*'s Cephalus, the head of a family and father of Polemarchus and Lysias: The pious Clearchus, we are meant to see, is the Cephalus of the *Anabasis*. In the Republic, Socrates moves into the center of the discussion when Cephalus goes off to sacrifice (331d). A similar substitution occurs in the Anabasis when Xenophon, who ultimately becomes a "father" in the eyes of his soldiers (7.6.38), takes over the role played previously by Clearchus and the other generals. Xenophon alone is able to transcend the horizons of custom or convention (nomos) that limit the vision not only of Clearchus but also of Meno and Proxenus. Meno is utterly conventional in his worship of wealth, and Proxenus pursues the more respectable but equally conventional goal of a good name. Having spent time with Gorgias, Proxenus "straightaway believed [nomisas hêdê that he was capable of ruling" (2.6.17). The verb nomizein properly signifies belief in accordance with nomos. Proxenus accordingly puts his trust in the good opinion of other men, and he regards the study of logos not as a route to philosophic wisdom but as a mere instrument for the acquisition of reputation and power.

Xenophon stands apart from the three deceased generals in that he is able to see beyond the horizons of custom and convention and into that which is by nature. His leadership reflects a knowledge of both the nature of the human soul and the nature of political community. His political intelligence furthermore reflects an integration of the three parts of the soul that were separated in the personalities of his colleagues. He combines the nobility of Proxenus with the toughness of Clearchus, without forgetting the necessary appetites that are so forcefully represented by Meno. Xenophon's rhetorical strategy is not to dwell simply upon what is needed for survival; rather, he emphasizes the nobility of the great deeds that the Greeks are now called upon to perform (Ruderman 1992, 132). He nonetheless leads with both eyes open, so to speak, always keeping one eye on the noble and the other on the necessary.

Two examples of Xenophon's open-eyed toughness stand out. The first is his forceful suppression of Apollonides, the man who objects to the plan of action Xenophon proposes during his first speech to the captains of Proxenus. Apollonides points out the overwhelming advantage enjoyed by the Persians and recites the numerous difficulties (aponai) that confront the Greeks (3.1.26). Xenophon quickly cuts him off, implies that he is both stupid and cowardly, declares that he shames all of Hellas, and proceeds to suggest that he be demoted and made to serve as a slave. The result is that Apollonides is directly expelled from the group (3.1.27–32). In this instance, Xenophon appeals to the noble (the self-respect of his fellow Greeks) so as to accomplish the necessary (the complete silencing of negative voices), and he does it all with Clearchean ruthlessness. The political understanding displayed here is profound: The freedom of speech that characterizes the community of soldiers after Xenophon's ascent (see esp. 4.3.10) is made possible only by the prudent use of force to suppress dangerous speech at the moment of founding.²¹ The second example of Xenophon's open-eyed toughness is his comment on a guide who ran away after being beaten. Xenophon points out that Cheirisophus struck the man but did not tie him up afterward, and he quarreled with Cheirisophus about "the ill-treatment of the guide and the carelessness" (4.6.3). We may infer from this that Xenophon would not have beaten the man in the first place, but had he done so, he would have made sure to tie him up.

Xenophon's philosophical transcendence of nomos

²¹ Xenophon's harsh treatment of Apollonides and its unifying consequences are reminiscent of Odysseus' violent response to criticisms of Agamemnon by Thersites during an early assembly of the troops (*Iluad* 2.244–77, cf. Daiby 1992, 21). Compare the joke by Polemarchus about the use of force at the beginning of the *Republic* (327c) (Polemarchus jokes that if Socrates is not willing to join their party, he and his companions will use force to compel him to do so).

is also, and most important, the central theme of the dream that comes to him at the time of greatest perplexity. The dream seems to him a sign from Zeus, as if in answer to his experience of aporia (3.1.2, 11). In his own mind, the dream offers a second chance to learn from an encounter with the sacred, which Xenophon was not ready to do when he went to Delphi but for which he has perhaps been prepared by subsequent dangers and hardships. The connection between the dream from Zeus and the oracle at Delphi is strengthened by the dream's ambiguous character: Like an oracle, it responds to an (unvoiced) question ("Seeing no way out, what am I to do?"), which it "answers" indirectly and only according to the manner in which it is interpreted.²²

The dream is simply this: "There was thunder, and lightning fell on his father's house, and as a result everything was set ablaze" (3.1.11). The image conveys Xenophon's complete abandonment: He can expect help from no city, no general, no teacher, and henceforth he must rely only on himself (3.1.14). On a deeper level, the image reflects the collapse of the Greek horizons in a strange and profoundly hostile world. The very gods seem to have abandoned the Ten Thousand, as the Persians have broken their oaths with apparent impunity. Under such circumstances, the ways of one's father—the "house" destroyed in the dream—can no longer offer shelter and protection. The only hope lies in building a new political dwelling from the ground up. This task, which involves fashioning new nomoi, calls for a genuinely original or foundational intelligence. By the same token, it also calls for a kind of courage that is not defined by steadfast adherence to nomos.

Xenophon responds to the crisis in a way that leaves no doubt about his foundational intelligence. He succeeds in introducing a new goal for the community (i.e., returning to Hellas, 3.2.26), passing new decrees (including burning the wagons, tents, and superfluous baggage, 3.2.27–8), establishing a new marching order (3.2.36), and awakening a new ethos of responsibility (3.2.31). These saving deeds, however, are made possible by his extraordinary courage, the nature of which comes into view in his immediate response to the dream. One is especially struck by the optimism of Xenophon's first thought upon waking: "He judged the dream in one way good, because in the midst of labors and dangers he seemed to see a great light from Zeus" (3.1.12). But because "the fire seemed to blaze all about," he was also "afraid . . . lest he might not be able to escape from the King's country" (3.1.12). Although the dream portends death, Xenophon seizes the inherent promise of the moment. Lesser men might have been blinded and paralyzed by the dream holocaust, but this frightening vision affects him in exactly the opposite way. In his remarkable interpretation, the terrible fire becomes a meaningful sign from the highest divinity. Xenophon feels fear and is well aware of the dangers at hand; yet, his response to the dream is not courageous in any ordinary sense, because all that sustains him is the "light" coincident with the destruction of the very horizons that inform political courage. Zeus's light, furthermore, signifies nothing more than the promise of intelligibility; it is at best an invitation to reflection, not an answer in itself. On the basis of this promise alone, Xenophon regards the dream as a good omen. His courage thus seems to be a function not of thumos (as is political courage) but of his philosophical erôs: His interpretation of the dream manifests the confidence he feels in his power to find a way out of even the most challenging aporia.

As the promise of intelligibility, the light from Zeus ambiguously expresses both the power of Xenophon's intelligence and the intrinsic visibility of the solution to the problem he confronts. Understood thus, Xenophon begins to see the light from Zeus at the moment he identifies divine illumination as the central feature of the dream. It is in this light of intelligibility that he also proceeds to interrogate himself. Xenophon's intellectual training comes to the fore when he responds to his perplexity by initiating a process of Socratic questioning:

Why am I lying here?... If we fall into the king's hands, what prevents our seeing all the most cruel sights, and experiencing the most terrible sufferings, and being put to death violently? No one is making preparations or taking care as to how we will defend ourselves... What about me? What general, from what city, do I expect to do these things? What age am I waiting to attain for myself? For I will never be any older, if I give myself up this day to our enemies (3.1.13-4).

As this passage makes clear, it is through internal dialogue that Xenophon is able to arrive at an understanding of what must be done. To see in the light from Zeus is thus to reflect upon perplexity after the fashion of Socrates, calmly and courageously pushing a line of inquiry to its ultimate conclusions. This relentless dialogical process burns away all that is inessential and irrelevant so that the very heart of things is finally exposed to view. It is this clear vision of the hard truth that saves Xenophon and the Greeks.²³

In the last analysis, the light from Zeus in which Xenophon thinks his way out of aporia in the moment of crisis bears a strong resemblance to the light of what Socrates, in book 6 of the *Republic*, calls the Good. The sunlike Good illuminates the path of philosophical reflection, and it is the Good toward which the philosopher is drawn by his erôs. Xenophon's erôs for what is highest and best makes him receptive, in the dream sequence, to an inner, guiding vision, much as Socrates was guided by his daimonic sign (Xenophon, *Memorabilia* 1.1.2; Plato, *Apology* 31d, 40a). And it is Xenophon's philosophical divination of that which Socrates calls the Good—"that which every soul pursues, and for the sake of which it does everything" (*Republic*

²² The appearance of the character named "Apollonides" mmediately after the dream (3.1.26) reminds us of the god at Delphi and may further suggest that the dream is, so to speak, the mask of Apollo.

²³ If Xenophon is the "young man" of 2.4.19 (as the context may suggest: cf. 2.4.15–8), he has already manifested the ability to fight fear with logic Compare Socrates' reasoning about death in Plato, *Apology* 40c–1c.

505d11-e1)—that directs him, in the end, to ascend beyond politics by recollecting the noble and beautiful wisdom of Socrates.

CONCLUSION: XENOPHON AND SOCRATES

I have attempted to show that Xenophon's Anabasis can most fruitfully be read as a companion piece to Plato's *Republic*, and I have suggested that Xenophon's reason for writing in this manner was to engage Plato in a dialogue about the nature of Socratic philosophizing. Plato's Republic presents a paradox. It centers upon the opposition between the thumos of the political man and the eros of the philosopher, but the philosopherking of the Kallipolis is nevertheless supposed to overcome this opposition. The *Anabasis* speaks to this paradox by displaying the roots of Xenophon's saving political leadership. Xenophon showed both spiritedness and courage, qualities that are ordinarily assigned to thumos. My examination of the Anabasis suggests, however, that Xenophon's spiritedness is indistinguishable from his philosophical desire, and his courage is, by the same token, a function of his philosophical eros. The highest human souls, Xenophon suggests, are the strongest and most resourceful ones, precisely because they are most ardent in the pursuit of wisdom. In them, eros binds together the parts of the soul and guides the whole soul in such a way that the parts are transformed. The vocabulary appropriate to speech about nonphilosophical souls is no longer appropriate in the case of someone like Xenophon. Socrates' characterization in the Republic of the role of thumos within the soul does not apply to philosophical natures, in whom courage is an expression of passionate intelligence or of the energy with which the whole soul is drawn toward wisdom.

Seen in the light of the Anabasis, the conflict between erôs and thumos is not intrinsic to the Socratic philosopher; it is rather a consequence of the fragmentation of lesser psyches. This insight makes it possible to understand the relationship between Xenophon's philosophical nature and his political activity. Although his decision to go to Asia is on the surface a rejection of Socrates' abstention from what Strauss calls "the political things in the common sense of the term," Xenophon does not, as Diogenes realized, go beyond Socrates in the most fundamental sense. To "go beyond" Socrates in such a way as to become oneself is in any case, as Kierkegaard (1985, 111) has suggested, the quintessentially Socratic deed.

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Disaggregating and Explaining Corporate Political Activity: Domestic and Foreign Corporations in National Politics

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orporate political activity is usually operationalized and analyzed as financial contributions to candidates or political parties through political action committees (PACs). Very little attention has been paid to other dimensions, such as lobbying, in a systematic way. On a theoretical level we address the issue of how to conceive of PAC contributions, lobbying, and other corporate activities, such as charitable giving, in terms of the strategic behavior of corporations and the implications of "foreignness" for the different types of corporate political activity. On an empirical level we examine the political activities of Fortune 500 firms, along with an oversampling of U.S. affiliates of large foreign investors for the 1987–88 election cycle.

¬he business community, which extends beyond national boundaries, is one of the most important sources of interest group activity. Domestic and foreign corporations as well as trade associations account for the preponderance of politically active groups in the United States. There are various dimensions to corporate political activity, but scholarly research has focused almost entirely on financial contributions to candidates through corporate political action committees (PACs). Although it is acknowledged that other dimensions merit analysis, it is argued that "corporate PAC donations are important in themselves, but they also should be understood as one quantitative indicator of a range of other corporate political activity" (Su, Neustadl, and Clawson 1995, 23). Scholars of citizen participation once adopted a similar position: "Almost all measures of political involvement and participation are highly correlated with one another and for analytical purposes, interchangeable" (Berelson, Lazersfeld, and McPhee, quoted in Verba and Nie 1972, 44). But Verba and Nie (p. 45) disagree: "Our view differs. . . . There are many types of activists engaging in different acts, with different motives, and different consequences." Because PAC contributions are a convenient measure of activity and because scholars have used contributions as a general indicator, there has been very little systematic analysis of other dimensions of corporate political activity. We directly address this deficit and examine the determinants of a collection of activities that constitute business political participation.

We examine political activity directed at public preferences through sponsorship of the arts and charitable donations, at candidates in election campaigns through financial contributions made by PACs, and at policymakers through lobbying, and we explore what accounts for interfirm variation in activity. At a minimum, it is necessary for political scientists to include

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these other dimensions if they want to move toward a more complete picture of the political activity originating from corporations. Furthermore, analysis of activity at the local and international as well as national level is required.

Earlier research on business efforts to influence politics, the literature not only on PACs but also on regulation (e.g., Stigler 1971), suggests concentrating on the resources and coercive powers of government as they differentially affect the interests of firms in order to understand the varying propensities of corporations for political activity. Motivated by questions surrounding the foreign penetration of the American economy, we modify the standard profit-maximizing model to account for the institutional complexity of corporate political activity. Noting in particular the sensitivity to foreign business political activity, we draw out the implications of "foreignness" for the standard profitmaximizing model and examine the effect of domestic versus foreign ownership on political strategies. As Rehbein (1995, 54) argues, "future studies need to develop data sets consisting of both foreign-owned and domestic firms, so that similarities and differences can be tested directly." We take this step by identifying and comparing the political activities of foreign and domestic corporations.

Empirically, we examine a range of political activities, beyond PAC contributions, that originate from both foreign-owned and domestic corporations. Theoretically, we modify the standard profit-maximizing model of PAC contributions to include institutional effects (North 1997), extend the theory to new dimensions of corporate political activity, and extract the theoretically interesting properties of these new dimensions in contrasting domestic and foreign firms' propensities for political activity. We first discuss earlier research and our theoretical approach. We then describe the operationalization and measurement of our variables and the results of our empirical analysis.

EARLIER RESEARCH

The most important factors identified in the literature on business PAC formation and spending are firm size, the degree of government regulation of a firm or its industry, and the importance of government sales to a firm or its industry. In addition, the profit-maximization approach generally includes measures of the free-rider incentive (Andres 1985; Boies 1989; Grier, Munger, and Roberts 1994; Humphries 1991; Masters and Keim 1985; Wilson 1990) in an attempt to explain the formation and donations of corporate PACs.

Underlying this research is the assumption that corporate efforts to influence politics are perceived by managers to produce benefits for the firm, such as a friendlier regulatory environment or additional government contracts. The related literature that explores the consequences of PAC contributions and the links between contributions and legislative payoffs has produced mixed results (Frendreis and Waterman 1985; Grenzke 1989; Hall and Wayman 1990; Romer and Snyder 1994; Snyder 1992). The evidence suggests that when the payoff is defined narrowly in terms of committee or floor votes, PAC contributions have less influence. But voting, because of its visibility, is the most difficult of a representative's activities to influence. When the payoff is defined more broadly, analyses tend to support the assumption that businesses do get something in return for their political activities (Hall and Wayman 1990).

Political scientists have focused heavily on PAC formation and donations. Because data on lobbying activity are less accessible, corporate lobbying has not received comparable attention. Yet, some sociologists (Himmelstein 1997; Useem 1984), business scholars (Smith 1994), and economists (Navarro 1988) have taken a more comprehensive view of business political activity. Navarro (1998, 90) recognizes that charitable contributions may be part of "an overall lobbying strategy" and argues that profit maximizing is an "important motive driving contributions." Useem (1984, 116-8) maintains that charitable contributions should be considered a political activity and linked to both corporate and "classwide" considerations. Survey data suggest that corporate managers employ a political vocabulary to think about giving. In a survey of 400 heads of large U.S. corporations, the two most important reasons given for supporting the arts were "corporate citizenship" and the "business environment" (p. 125). Corporate giving to the arts is likely to have a disproportionate effect on elite perceptions of the corporation and may serve to increase access, in social settings, to policymakers.

Charitable giving doubtless enhances the corporate self-image for managers and employees, but the direct economic benefits are to a large extent intangible. Anecdotal evidence suggests that corporate donations to charity are used to influence public and elite perceptions of the institution and hence policy in relation to that institution. (In particular, regarding Japanese corporate activity in the United States, see Choate 1990, 142–3). According to Craig Smith (1994, 108), Arco, the oil company, "had been using philanthropy strategically since 1971, when it began funding and forming alliances with environmental groups."

A firm-level measure of U.S. corporate charitable donations was not available to Useem. We propose a systematic analysis of the motivations behind charita-

ble giving (see also Mitchell 1989) and argue that it is theoretically appropriate to treat this activity as a form of corporate political activity. If we find that the set of firms that contribute to charity is similar in some important respects to the set that lobbies and contributes to elections, then empirical confidence in this argument increases.

Policymakers respond to direct contacts and to electoral and public opinion pressures. The different activities that constitute corporate participation represent complementary options in a campaign to influence the policy process. We assume a decision structure that permits the coordination of these resources. Research on corporate PACs suggests that the company's chief executive officer (CEO), public or governmental affairs officers, and representatives in its Washington, D.C., office are key personnel in the running of the PAC (Sabato 1984, 34-5). According to Sabato (p. 126), "most PAC officials...will admit to having lengthy discussions or sharing information with their organization's lobbyists when it comes time to endorse candidates." Schlozman and Tierney (1986, 228) show that the great majority of corporations running PACs do so with "input from the Washington Office," which supports this conceptualization. Public affairs officers, CEOs, and senior managers generally determine the corporation's charitable contributions (Himmelstein 1997, 9; Siegfried, McElroy, and Biernot-Fawkes 1983). The location of campaign contributions, lobbying, and philanthropic decision making among this narrow group presents the opportunity for coordination.

THEORY AND HYPOTHESES

We begin with the standard profit-maximizing framework for explaining the different dimensions of corporate political activity. Specifically, we will analyze and compare models that predict PAC contributions, lobbying activities, and charitable donations. (See Grier, Munger, and Roberts 1994 for an "organizationally constrained profit-maximizing" model; but see Martin 1995 for related on work on the formation of firm preferences. Martin argues that various institutional factors, rather than economic interests alone, influence preferences on health policy.) Within this theoretical context, corporations are expected to use political activity to secure sales and avoid or modify costly regulations. Whereas Grier, Munger, and Roberts analyze aggregated industry contributions, our focus is on individual firms.

In their most basic form, corporate profits can be modeled as a function of revenues (which depend on prices and quantity produced) minus costs (which depend on the prices and quantities of inputs). Firms make choices about how much to produce and the combination of inputs to use based on their goal of maximizing revenues minus costs. In other words, to maximize profits, firms want to increase revenues and decrease costs. As we move from an ideal perfectly competitive market system to the realities of the current political economy, many factors affect a firm's revenues and costs, and in turn its profits. More

specifically, firms will engage in political activities to enhance revenues and minimize costs, and various factors will affect their profit-maximizing abilities and strategies.

On the revenue side, political activities may be targeted at procuring government contracts or price supports and tariffs. On the cost side, political activities may be targeted at reducing regulatory or labor costs or securing tax relief. As the different types of political activities are not equally instrumental in enhancing revenues and reducing costs, the significance of factors may vary across the types of activities. For example, lobbying is the most instrumental act, whereas charitable giving is at best an indirect route to securing benefits, such as a government contract.

As a first step, we model corporate political activities as a function of factors that directly and indirectly affect a firm's revenues or costs and hence its profits, and we derive a number of testable hypotheses based on the goal of profit maximization. Specifically:

Corporate Political Activity = $\beta_0 + \beta_1$

 \times Concentration + $\beta_2 \times$ Size + β_3

 \times Government Procurement + β_4 \times Foreignness

 $+ \beta_5 \times \text{Countermobilization} + \beta_6 \times \text{Visibility} + \beta_7$

× Government Regulation.

We expect that the performance of these factors, and the model as a whole, will vary in importance across the dimensions of political activity, as we show in the discussion of the hypotheses below.

Following the standard profit-maximizing model and consistent with earlier research (Andres 1985; Boies 1989; Grier, Munger, and Roberts 1994; Masters and Keim 1985), we argue that the likelihood of corporate political activity depends on the availability of the necessary resources and degree of government involvement. We hypothesize that, in general, political activity will increase with corporation size, government regulations, sales to the government, and industry concentration, but these factors may differ in their degree of importance across the dimensions. (High industry concentration lowers the incentives to free ride, assuming actors share a common and industrywide goal, and lowers information costs.)¹

We argue that this standard model may be improved by incorporating institutional influences on the incentive to participate. Drawing on insights from the participation and group literatures, we represent these influences with measures of the visibility of the corporation, the foreign or domestic source of corporate activities, and the degree of political countermobilization. We hypothesize that the likelihood of corporate political activity will increase as institutional visibility increases because the corporation is likely to attract the attention of other interests and of policymakers, which could raise (regulatory) costs and negatively affect

profits. In order to counter the threat of a public policy backlash, all forms of corporate political activity will increase as visibility increases.

Visibility is an important factor for the corporation as an institution, and it is also a theoretically important concept for the analysis of political activities. Lobbying is a less visible activity than federally monitored campaign contributions and charitable giving. Generally, corporations prefer less visible rather than more visible methods of influence (Schattschneider 1960, 40). Visible activity may generate opposition and stockholder concerns, and unlike membership-based interest groups, these institutions do not have to rely on widely publicized successes to recruit members and raise funds (Gais and Walker 1991). Moreover, the visibility of political activities is a particular consideration for foreign firms.

The conventional wisdom is that foreign corporations commit more resources to political activities than do domestic corporations (Choate 1990; Prestowitz 1988; Tolchin and Tolchin 1988). We theorize that foreign affiliates adapt to the host political economy (Mitchell 1995; Mitchell, Hansen, and Jepsen 1997; Pinto-Duschinsky 1981; see also Rehbein 1995). The forces that produce political activity by domestic firms will do so for foreign firms (for an alternative theoretical argument, see Pauly and Reich 1997). If foreignness does make a difference, we hypothesize that it will affect PAC activity and to some extent charitable giving, rather than lobbying. Given the greater visibility of PAC contributions, foreign firms will be, ceteris paribus and contrary to the conventional wisdom, less likely to contribute because of concerns about the perception of interfering in the politics of another country. Although PAC contributions from U.S. affiliates of foreign corporations are formally permitted under campaign finance regulations, legitimacy questions arising from foreign ownership constitute an informal institutional constraint, which amounts to observing a norm of behavior (North 1997, 6). This institutional factor should be less salient for less visible political activities, such as lobbying. Charitable giving is not very obviously a self-interested activity, but it still may be perceived as interference and so is more risky for foreign firms; again, we hypothesize that foreign firms will be less likely to contribute to charity than domestic firms.

Another theoretically important characteristic of political activity is its instrumentality in communicating a narrow goal. The greater the visibility of an activity, the greater is the difficulty of tying it to narrow, "private," interests. Lobbying conveys most information to policymakers concerning policy preferences, is most instrumental, and is most likely to result in an exclusive benefit, such as regulatory relief or a government contract (analogous to citizen contacting; see Leighley 1995). The expected benefits of PAC contributions are broader, as the noncontributing corporation cannot be excluded from the benefits of electing a probusiness candidate. If the contribution is targeted at a candidate who has the prospect of an important committee assignment, then it may result in improved policy

¹ For additional insights into understanding economic approaches to corporate political activity, particularly lobbying, see Alt et al. 1999.

access and a narrower benefit for the particular corporation. Yet, both giver and receiver must at least pretend a broad not narrow benefit. Charitable contributions convey little information about policy preferences, have an indirect effect on policymakers, and are not likely to influence firm-specific contracts and regulations. Because lobbying is the least visible and the most instrumental of the different activities, foreignness should make the least difference with respect to lobbying.

An additional difference among these activities is that, in contrast to lobbying, PAC contributions and charitable donations may be solicited from the corporation as well as initiated by it. Solicitation should affect which candidate or charitable organization receives a contribution but not the overall resources the corporation allocates to these activities.2 We are modeling the overall amount the corporation contributes, not the choice among candidates or causes. For example, AT&T's PAC conforms to this dual process: Separate committees are responsible for the PAC funds and their disbursement (Mutch 1994). For charitable contributions, Smith (1994) describes changes in corporate allocations for philanthropy as a function of internal decisionmaking and market conditions, not solicitation pressures. He illustrates the importance of those pressures for disbursement decisions, however: AT&T "caved in to demands from the Christian Coalition to deny funding to Planned Parenthood" (p. 110).

We also consider the degree to which there is potential or actual political countermobilization. In an analysis of the Bork nomination, Austen-Smith and Wright (1994) show that lobbying, even by "friendly" legislators, will increase to counteract the efforts of opposing groups. Mobilization of competing interests may differ across types of industries (e.g., according to degree of unionization, type of products, environmental effects, or the like). Corporate political activity is likely to increase to offset the potential costs of mobilization (Martin 1991; Truman 1951, 79; Vogel 1989, 293; Wilson 1990).

DATA AND MEASURES

We focus on large firms potentially interested in national politics. These may vary considerably in size, from sales of \$91 million to \$121 billion (\$500 million for the smallest Fortune 500 company). Data on Fortune 500 corporations were used in earlier analyses of domestic corporate political activity (e.g., Boies 1989; Humphries 1991; Masters and Keim 1985, 1986). We examine financial contributions, lobbying, and charitable donations of Fortune 500 companies for 1988. In addition, we include the largest U.S. affiliates of foreign firms listed in Forbes (1989), which covers the financial and service sectors as well as manufacturing. For our purposes, we limit foreign firms to those in manufacturing to ensure comparability with the Fortune data. Corporate PAC contributions to federal election candidates is for the 1987-88 cycle (Almanac of Federal PACs 1990; Federal Election Commission 1989; Makinson 1992). Data on PAC contributions by domestic affiliates of foreign corporations are available from the Congressional Research Service, but only for the 1987–88 cycle (Walke and Huckerbee 1989). The Congressional Research Service report provides a useful means of verifying the identification of politically active affiliates.

Appendix A provides a detailed description of the data. More specifically, Table A-1 lists each variable, how it is measured, and the sources of data. Table A-2 gives statistics for all dependent and independent variables:

For lobbying, we collected 1988 data on the number of corporate representatives in Washington, D.C., offices as well as the number of consultant or counsel offices retained by the corporation in the capital. These data measure the potential for lobbying by each firm.³ We also collected data on corporate testimony for congressional hearings. Annual financial data on charitable contributions by company are available in the *Corporate Giving Directory* (1991). Major charitable contributions (more than \$500,000) include both company and foundation giving as listed under the "How much they give" category. For entries that state a range (e.g., \$500,000 to \$1,000,000), the lowest figure was used.⁴

For the independent variables in the explanatory model, we use firm size as an indicator of political visibility. The larger the firm, the more likely it is to attract the attention of government regulators as well as other institutions and groups, and regulation can affect a firm's costs. More generally, and particularly for large data sets that include firms smaller than the Fortune 500, firm size is an indicator of resources. Firm size is measured as the log of dollar sales and is available in the annual "Fortune 500, The Largest U.S. Industrial Corporations" (Fortune, April 24, 1989, 10-30) and "The 100 Largest Foreign Investments in the U.S." (Forbes, July 24, 1989, 313-8). In addition, we use the number of company citations in the Wall Street Journal and the number of negative articles, available through WESTLAW and Lexis-Nexis, as measures of visibility. Specifically, we use the log of the number of times the company name appeared in the story and title of an article during 1983–87 and the log of the number of negative articles for 1987. The effect of free-rider incentives is measured using industry concentration ratios. Higher concentration lowers organization and information costs and should have a positive effect on political activity.

For corporation sales to the government, which

² We thank a reviewer for drawing this process to our attention.

³ Ideally, we would like to use actual lobbying expenditures by firms. Although these data are available under the Federal Regulation of Lobbying Act of 1946, evidence suggests that there is gross underreporting. A recent General Accounting Office study compared lobbyist registration under the 1946 Act and the modified Lobbying Disclosure Act of 1995; it found 10,612 organizations and individuals registered in 1996 under the later act who had not been registered in 1995 under the earlier act (GAO, Comparison of Lobbyists' Registrations B-274543.)

⁴ Since charitable giving is highly skewed, we use the log of dollar contributions.

enhance firm revenues and hence should be positively related to political activity, we use the log of the dollar amount of defense contracts and, at the industry level (based on U.S. Bureau of the Census four-digit SIC codes), the log of the dollar amount of industry sales to the federal government weighted by total industry sales. Regulations generally raise a firm's costs, which induces political activity to counter those costs. Regulation is measured at the firm level by corporate interactions with the federal courts in regulatory cases and with major regulatory bodies (EPA, OSHA, CPSC, FERC, NLRB, FDA, FTC, and the Antitrust Division of the Department of Justice). The SIC pollution abatement expenditures for 1988 are used to measure regulation at the industry level.

There are 31 foreign firms among the *Fortune* 500. In addition, we oversampled for foreign firms by collecting data on 70 U.S. manufacturing subsidiaries listed in *Forbes* (1989). Foreign firms are identified in the analysis with a dummy variable.⁶

Countermobilization typically increases costs for a corporation, 50 firms have an incentive to counter with increased political activity of their own. The mobilization of countervailing interests is measured at the firm level by union PAC and lobbying activity, matched to firms by examining bargaining agreements. Because of multicolinearity problems, we combined the PAC and lobbying measures in a countervailing power index that used each firm's share of total union PAC money and each firm's share of total union lobbying. At this stage we do not have data on environmental and consumer groups (Vogel 1989, 293). Finally, for charitable donations, the number of plant locations is included as a control variable; philanthropy is not necessarily focused on the national level, in contrast to PAC and lobbying activity, and it is likely that corporations with facilities in multiple communities will make more donations.

ANALYSIS AND RESULTS

Of the 565 firms in our data set, 72.6% engage in some form of measurable political activity at the national level. Lobbying is the most common—54.6% have representatives in Washington or retain counsel there. Roughly half (292 corporations, 51.6%) have PACs, and 215 firms (38%) gave \$500,000 or more to charity. Restricting the analysis to the domestic Fortune 500, 56.4% lobby, 54.6% have PACs, and 42.2% give to

6 The 495 Fortune 500 companies (data are missing on five) plus 70 foreign affiliates total 565 corporations in the analysis.

charity. Because earlier research did not examine the other modes of participation, only the PAC figure can be compared, and it is in keeping (Boies 1989, 821; see also Andres 1985, 213). Contrary to the popular view, foreign firms are not hyperactive politically. Compared to domestic firms, foreign-owned affiliates lobby less (38% versus 56%) and are less likely to have PACs (32% versus 55%), and fewer give to charity (13% versus 43%). Foreign-owned firms are less likely than domestic firms to engage in any of the three activities, with the differences more pronounced for PAC and charitable giving, the more visible activities.

Scholars have commented, particularly with regard to PAC formation, that it is surprising how many Fortune companies are not politically active. Sabato (1984, 32) terms these the PAC holdouts. In addition to noting the variation in government involvement across industries, Sabato reports the results of a telephone survey of Fortune 500 companies without PACs and identifies a mix of motives, including concerns about perceived corruption, hostile public reactions, the belief that business should not be involved in politics, and reliance on trade associations for such activity (pp. 32–3; see Makinson 1992 for data on trade association PAC contributions).

Although there is considerable variation in size among the Fortune 500, almost all the 200 top ranked are involved in some form of political activity. The nonactive firms are concentrated in the bottom threefifths of the list. Among the top 100, not until number 67, Hanson Industries, does one find neither PAC, nor lobbying, nor charitable contributions. Hanson is foreign-owned, has no defense contracts or shipments to the government, and has no measured interactions with federal regulators. The next total "holdout" is Agway, at Fortune rank 124, which also has no defense contracts or government shipments and no measured interactions with federal regulators. Both operate in industries with relatively low concentration and low expenditures on pollution regulation compliance, and both had relatively few Wall Street Journal citations.

Among the politically active firms, General Electric and Philip Morris are near the top on all dimensions. The former, which ranks fifth among the Fortune 500, is very visible as measured by Wall Street Journal citations, is highly concentrated and highly regulated, and has large defense contracts and shipments to the government. The same is true for Philip Morris, which ranks tenth, in terms of visibility, concentration, and regulation. It was the largest of the PAC contributors in the 1987–88 election cycle, distributing a total of \$623,380. General Electric has the largest number of lobbyists

⁵ In creating this variable, we used WESTLAW's online electronic database to gather information on the total number of interactions of each corporation with the federal courts, the Antitrust Division, and each of the seven major regulatory agencies: Environmental Protection Agency, Occupational Safety and Health Administration, Consumer Product Safety Commission, Federal Energy Regulatory Commission, National Labor Relations Board, Food and Drug Administration, and Federal Trade Commission. By "interaction" we mean court actions and a number of different types of regulatory agency actions, including adjudicative decisions, consent orders, interlocutory orders, and opinion letters regarding requests, such as legal exemptions.

⁷ We also compared the means of our independent variables for active, inactive, and partially active firms. For example, for size and regulation, corporations that engage in only one activity lie in between those active on all dimensions and those that are inactive. Average sales for firms active on all three dimensions are \$9.5 billion, inactive firms have average sales of \$980 million. Firms giving only to PACs have average sales of \$1.4 billion, those that only lobby have average sales of \$1.8 billion; and those that only give to charity have average sales of \$1.7 billion Results are similar for the regulation variable.

TABLE 1. Top Ten Most Politi Corporations among Fortune 5 Affiliates, 1988	
PAC Contributions	
Philip Morris	\$623,380
GM	\$555,469
Mid-Am Dairy	\$458,650
Lockheed	\$441,834
Textron	\$ 411,431
Northrop	\$383,517
FMC	\$382,345
Rockwell	\$379,768
GE	\$370,445
RJR Nabisco	\$364,025
Washington Lobbyists GE	34
Philip Monts	28
Ford	27
Westinghouse	26
McDonald-Douglas	23
GM	22
Boelng	.21
Chrysler	17
Arco	17
IBM	16
Charltable Contributions	•
IBM	\$135,400,000
Hewlett Packard	\$55,000,000
GM	\$54,500,000
Exxon	\$48,985,000
RJR Nableco	\$48,952,000
GE	\$38,824,000
Ford	\$31,589,400
Dupont	\$31,000,000
Proctor and Gamble	\$29,619,000
3 M	\$29,089,52`1

(34 representatives in the Washington office, consultants, or counsels retained); Philip Morris is second with 28. IBM is the largest of the contributors to charity.

Even when the focus is restricted to the top ten most active corporations in each category, as shown in Table 1, there is the suggestion of overlap among the forms of activity. Including all the firms in the analysis reveals that those active in one category are likely to be active in the others.

To examine the overlap among the forms of political activity, Table 2 presents cross-tabulations of firms with PACs and those with lobbyists, firms with PACs and charitable contributions, and firms with lobbyists and charitable contributions. As can be seen in Table 2A, many more firms with PACs have a lobbying presence in Washington than do firms without PACs, and the 42% difference is easily large enough to reach statisti-

TABLE 2. Cross-Tabulations of Lobbying, PAC Formation, and Charitable Giving by Fortune 500 and Foreign Affillate Corporations, 1988 A. PACs and Lobbying No PAC PAC No lobby 67% 25% Lobby 33 75 100% 100% N = 292N = 273 $\chi^2_{1df} = 97.95$, significant at .001 level B. PACs and Chartty No PAC PAC No chartty 80% 45% Chartty 20 55 100% 100% N = 273N = 292 $\chi^2_{1df} = 75.32$, significant at .001 level C. Lobbying and Charity No Lobby Lobby No charity 80.5% 47% Chartty 19.5 53 100% 100% N = 257N = 308 χ^2_{1df} = 68.62, significant at .001 level

cal significance. Firms with PACs are also more likely to make charitable donations—55% compared to 20% for firms without PACs (Table 2B). A similar, statistically significant relationship holds between lobbying and charitable contributions (Table 2C).

To "understand" one activity as a "quantitative indicator of a range of other corporate political activity" (Su, Neustadl, and Clawson 1995, 23) would be mistaken. Although the activities are not perfectly correlated, however, the forms of activity are clearly associated. The results for charitable contributions provide empirical support for the claim that this should be considered a form of political activity. We are interested in the relationships among these activities both as components of a campaign to influence politics and policy and as outcomes of the economic and political forces at work on the firm.

Through multivariate analysis, we investigated the amount of participation in the three forms of political activity. The regression models in Table 3 display the findings.⁸ For each dependent variable, a substantial number of firms decided not to participate politically. These nonparticipants present a problem of sample selectivity. A solution is to use the Heckman (1976, 1979) correction, as suggested by Grier, Munger, and Roberts (1994). The procedure adds another regressor, known as the inverse Mill's ratio, which is obtained

 $^{^{8}}$ N=555 because ten foreign firms had missing values on the sales data and were excluded.

TABLE 3. Determinants of Corporate PAC Contributions, Number of Washington Lobbylsts, and Chartrable Contributions (with Heckman Correction for Selectivity)

Corporate PAC Number of Lobbylsts In Contributions Washington, D.C.		Charltable Contributions			
Coefficient (S.E.)	t- Statistic	Coefficient (S.E.)	t- Statistic	Coefficient (S.E.)	t- Statistic
-577,203 (68,573)	-8.42***	-37.3 (3.44)	-10.85***	5.63 (1.50)	3.76***
392 (214)	1.83	0.019 (0.009)	2.05*	0.0003 (0.003)	0.10
6,452 (1,034)	6.24***	0.16 (0.04)	3.75***	0.02 (0.01)	1.54
126,700 (43,152)	2.94**	6.15 (1.85)	3.32**	-0.36 (0.63)	-0.58
60,022 (6,497)	9.24***	3.72 (0.33)	11.14***	0.91 (0.13)	6.81***
-52,692 (15,159)	-3.48***	0.12 (0.54)	0.22	-0.52 (0.35)	-1.50
14,780 (6,111)	2.42*	1.05 (0.25)	4.22***	0.13 (0.0 9)	-1.49
3,798 (5,069)	0.75	0.9 5 (0.21)	4.49***	0.19 (0.10)	2.00*
17,544 (6,050)	2.89**	0.43 (0.20)	2.12*	0.11 (0.07)	1.45
9,554 (3,016)	3.17**	0.47 (0.11)	4.16***	-0.05 (0.04)	-1.49
_	_	_	_	0.01 (0.07)	0.07
120,676 (27,322)	4.42***	7.36 (1.01)	7.26***	0.83 (0.38)	2.21*
289		306		215	
.55		.64		.54	
	Coefficient (S.E.) -577,203 (68,573) 392 (214) 6,452 (1,034) 126,700 (43,152) 60,022 (6,497) -52,692 (15,159) 14,780 (6,111) 3,798 (5,069) 17,544 (6,050) 9,554 (3,016) — 120,676 (27,322)	Coefficient (S.E.) t- Statlstic -577,203 (68,573) -8.42**** 392 (214) 1.83 (214) 6,452 (1,034) 6.24**** 126,700 (43,152) 2.94*** 60,022 (6,497) 9.24**** -52,692 (15,159) -3.48**** 14,780 (6,111) 2.42* 3,798 (5,069) 0.75 17,544 (6,050) 2.89*** 9,554 (3,016) 3.17*** - - 120,676 (27,322) 4.42****	Coefficient (S.E.) t- Statistic Coefficient (S.E.) -577,203 (68,573) -8.42**** -37.3 (3.44) 392 (214) 1.83 (0.009) 0.019 (0.009) 6,452 (1,034) 6.24**** 0.16 (0.04) 126,700 (43,152) 2.94*** 6.15 (1.85) 60,022 (6,497) 9.24**** 3.72 (6,497) -52,692 (15,159) -3.48**** 0.12 (0.54) 14,780 (8,111) 2.42* 1.05 (0.25) 3,798 (5,069) 0.75 (0.21) 0.95 (0.21) 17,544 (6,050) 0.43 (6,050) 0.43 (0.20) 9,554 (3,016) 3.17** (0.11) - - - 120,676 (27,322) 4.42**** 7.36 (1.01) 289 30	Coefficient (S.E.) t- Statistic Coefficient (S.E.) t- Statistic -577,203 (68,573) -8.42**** -37.3 (3.44) -10.85**** 392 (214) 1.83 (0.009) 0.019 (0.009) 2.05* (0.009) 6,452 (1,034) 6.24**** 0.16 (0.04) 3.75**** (1,034) (0.04) 126,700 (0.04) 2.94*** 6.15 (1.85) 3.32*** 60,022 (6,497) 9.24*** 3.72 (0.33) 11.14**** -52,692 (15,159) -3.48**** 0.12 (0.54) 0.22 (15,159) 14,780 (6,111) 2.42* 1.05 (0.25) 4.22**** (6,111) (0.25) 3.798 (5,069) 0.75 (0.21) 4.49**** 17,544 (6,050) 2.89** (0.20) 2.12* 9,554 (3,016) 3.17** (0.11) 4.16**** - - - - 120,676 (27,322) 4.42*** 7.36 (1.01) 7.26**** 289 306	Coefficient (S.E.) t- Statistic Coefficient (S.E.) t- Statistic Coefficient (S.E.) -577,203 (68,573) -8.42**** -37.3 (3.44) -10.85**** 5.63 (1.50) 392 (214) 1.83 (0.009) 0.019 (0.009) 2.05* (0.003) 0.0003 (0.003) 6,452 (1,034) 6.24**** 0.16 (0.04) 3.75**** 0.02 (0.01) 126,700 (43,152) 2.94*** 6.15 (1.85) 3.32*** -0.36 (0.63) 60,022 (6,497) 9.24*** 3.72 (0.33) 11.14**** 0.91 (0.43) -52,692 (15,159) -3.48*** 0.12 (0.54) 0.22 (0.54) -0.52 (0.35) 14,780 (6,111) 2.42** 1.05 (0.25) 4.22**** -0.13 (0.09) 3,798 (5,069) 0.75 (0.21) 0.43 (0.20) 0.19 (0.10) 0.11 (0.10) 17,544 (6,050) 0.89** 0.02 (0.20) 2.12* (0.07) 9,554 (3,016) 0.17** 0.11 (0.07) 4.16**** -0.05 (0.07) 120,676 (27,322) 4.42**** 7.36 (1.01) 7.26**** 0.83 (0.38) 289 306 215

from probit models. We define the new regressor, λ , as $f(Z_i)/F(Z_i)$, where $Z_i = X_i\beta$ from probit models. As Table 3 shows, λ is significant in all three models, which suggests the need to correct for self-selection.⁹

Consistent with earlier research on PAC contributions, the variables that constitute the standard profitmaximizing model influence all forms of political activity. Firm size (Size), operationalized as the log of dollar sales, is a significant positive influence on all three forms of political activity. This is similar to the PAC contribution findings of Andres (1985), Boies (1989), Humphries (1991), and Masters and Keim (1985). In addition to size, measures of government

involvement with a corporation are included in the standard profit-maximizing model.

Corporation sales to the government, measured at the firm level by the log of the dollar value of defense contracts (*Defense*), have a significant positive influence on decisions about how much to give to candidates and on how much to lobby, but they are insignificant in predicting charitable contributions. When government procurement is measured at the industry level by government shipments as a ratio of total shipments (*Government Shipments*), it is also significant in the PAC and lobbying models. Government procurement offers the most individualized of benefits to firms, encouraging them to focus on the more "instrumental" forms of political activity, rather than charitable giving.

There is evidence to support the influence of government regulation, whether measured at the firm or the industry level. Company interactions with regulatory agencies and courts (*Regulation*) and the industry-level measure (*Pollution Expenditures*) are positive and

⁹ Heckman's two-step correction is most appropriate for the model on PAC giving, but it could be argued that Tobit is more appropriate for charitable giving, which is consored data (we only observe contributions of \$500,000 or more). Green (1993, 697) suggests that Heckman's two-step correction is a consistent alternative to maximum likelihood estimation. We also obtained the Tobit results for each model for comparison. These were largely identical to the Heckman models, so for brevity and ease of comparison we present only the Heckman results.

significant in explaining both PAC giving and lobbying. Neither regulation measure is significant in the charity model, contrary to expectations. This may again reflect the difference in instrumentality among the different types of political activity.

An interesting mix of results for industry concentration emerged in earlier research. Andres (1985), Grier, Munger, and Roberts (1994), and Masters and Keim (1985) found a significant relationship between industry concentration (or number of establishments in an industry) and PAC activity. Boies (1989), Humphries (1991), and Rehbein (1995) found that industry concentration does not have a significant effect on PAC formation and activity. Even though there is a severe measurement problem because firms in our sample are often active in multiple industries but are constrained to one industry concentration value, we find that industry concentration (Concentration) is significant in the lobbying model but not for PAC contributions or for charitable giving. ¹⁰

The combined effect of union PAC contributions and lobbying efforts (Union Activity) has a significant positive influence on PAC contributions and amount of lobbying, but it is not significantly related to charitable giving. Visibility, as reported in Table 3, is measured by the log of the total number of Wall Street Journal citations (Wall Street Journal). This variable is significant for lobbying and charitable donations but not for PAC contributions (see Appendix B for a discussion of the potential for endogeneity, particularly regarding PAC giving). Firm size, too, can be interpreted as a measure of visibility as well as resources. Given the nature of our sample, all the firms are likely to have the resources to become politically active. There is a strong correlation between Wall Street Journal citations and firm size (.58).

Foreign ownership, a dummy variable with the value of 1 for U.S. affiliates of foreign companies and 0 for domestic firms (Foreign Ownership), significantly decreases the likelihood that a firm will give PAC contributions to candidates for federal elections, but it has no effect on lobbying. The insignificant difference between foreign and domestic corporations regarding lobbying fits with our hypothesis that foreign-owned firms adapt to the local political environment in the less visible forms of activity.

For charitable donations, the number of plant locations is included as a control variable (*Number of Plants*). It is likely that corporations with facilities in multiple communities will make more donations. In the regression analysis, number of plants is not significant

in explaining charitable contributions (although it is significant, $p \le .001$, when a Tobit model is used).

In disaggregating the dimensions of business political participation, we isolated the theoretically interesting properties of visibility and instrumentality and expected the importance of the different variables to vary across these dimensions. The results support this expectation. Overall, our modified profit-maximizing model explains most variance $(R^2 = .64)$ when the dependent variable is lobbying, the most instrumental and least visible activity. For the least instrumental activity, charitable giving, the "narrow" interests of the firm measured by defense contracts, government shipments, regulation, concentration ratios, pollution expenditures, and countermobilization have no significant influence. The varying effect of foreign ownership is another component of this argument, based on the view that foreign firms prefer to avoid visible activities. They are as likely to lobby as domestic corporations but are less likely to contribute to campaigns, and there is some evidence that they are less likely to give to charity.

Finally, we test and correct for endogeneity in three of our independent variables. Specifically, our measure of countermobilization, the *Wall Street Journal* citations, and regulation variables all pose a potential problem of endogeneity. Appendix B describes the various techniques that we use. The *Wall Street Journal* and regulation variables are affected in our PAC model, but overall the correction generally improves the performance of our model and lends support to our theoretical argument (see Appendix B and Table B-1).

One possible weakness in our results stems from the fact that we measure corporate readiness for lobbying activity rather than actual lobbying. We do not have a dollar measure of lobbying comparable to that of PAC and charitable contributions. As a further step to confirm our findings, we looked at one other measure, the number of appearances by firms at congressional hearings to lobby for or against legislation. These results largely substantiate our other findings and further support our theoretical expectations.

The model of congressional hearing appearances in Table 4 performs comparably to our other models. Size remains important. Government procurement at both the firm and industry level is significant. Both firm and industry measures of regulation are significant, as is our measure of countervailing power. Most interesting is the negative and significant coefficient for foreign ownership. We have argued that foreign firms will behave in a similar way to domestic firms in terms of lobbying, which is a narrow benefit, low visibility activity. For other modes of political participation, which might give the appearance of interfering in the politics of the host country, foreign firms are less likely to be active. This measure of lobbying provides results that support the general argument about the importance of institutional factors. Because appearing at a congressional hearing is a far more public and visible form of lobbying than hiring a Washington lobbyist, foreign firms are less likely to engage in this type of behavior. Lobbying is itself a collection of activities.

¹⁰ This problem is also present for government shipments and the pollution expenditures measure, as they rely on SIC codes.

¹¹ It is plausible to expect that negative publicity, rather than the total number of articles, is what drives business political activity, but when we included visibility measured by the number of negative articles in the same models, it had no significant effect. The correlation between the number of citations and the number of negative articles is .50. If we only use our measure of negative publicity, it is also insignificant in all models.

¹² In the Tobit model, foreign ownership also significantly decreases the likelihood of charitable contributions $(p \le .001)$.

TABLE 4. Determinants of Congressional Hearing Appearances					
	Number of Appearances at Congressional Hearings (Using Heckman Correction) Coefficient				
Varlable	(S.E.)	t-Statistic			
Constant	-3.03 (0.60)	-5.02***			
Concentration '	0.002 (0.002)	0.81			
Defense (lóg dollars)	0.03 (0.01)	2.86**			
Government shipments	1.23 (0.45)	2.71**			
Size (log dollar sales)	0.41 (0.06)	6.42***			
Foreign ownership	-0.57 (0.14)	-4.04 ***			
Union activity Index	0.16 (0.06)	2.76**			
Wall Street Journal (log citations)	0.03 (0.04)	0.77			
Regulation (log regulatory Interactions)	0.19 (0.06)	3.22**			
Pollution expenditures (log dollars)	0.07 (.02)	3.19**			
Lambda (Heckman)	0.72 ~ (0.21)	3.38**			
Number of cases	2	74			
Adjusted R ²		48			
"p ≤ .01, ""p ≤ .001					

CONCLUSION

We began by suggesting that corporate political participation includes various dimensions of activity, one dimension of activity is not necessarily a good proxy for all forms, and the different dimensions have differences in the theoretically interesting properties of visibility and instrumentality. We argued that firms engage in political activities to exploit political opportunities to enhance their revenues and reduce their costs. But other interests also mobilize to influence the use of government resources and coercive powers that influence firm profits. The size (and therefore the visibility) of the firm increases the likelihood of public attention, mobilization, and policy activity. Our examination of

the amount and type of political activity by corporations was motivated in particular by questions surrounding foreign penetration of the American economy.

Lobbying, PAC activity, and charitable donations linked to foreign corporations are politically significant issues that have attracted considerable attention in recent years (Choate 1990); there are even fears of a "fifth column" (Graham and Krugman 1989). Although it was a key issue in the 1996 presidential election, such foreign activity has not attracted the scholarly attention it deserves. We have presented systematic data on corporate political activities that permit direct comparison between domestic and foreign-owned corporations and allow us to reach firmer conclusions about the dimensions, particular motivations, and relative significance of foreign-based political activity within the United States. Including foreign firms in the analysis helps illustrate some real differences among the forms of political activity. Contrary to conventional wisdom, this evidence suggests that foreign corporations are less likely to contribute to candidates and are less likely to engage in public lobbying by appearing at congressional hearings, but they are about as likely as domestic firms to have a lobbying presence in Washington. The evidence on charitable contributions provides some support for a lower propensity toward donations by foreign-owned firms. The comparatively low level of political contributions by U.S. affiliates of foreign corporations is consistent with Pinto-Duschinsky's (1981, 232) observation that foreign firms in Britain are less likely to make political contributions. We found evidence that foreign firms adapt to local behavior but at the same time try to avoid the appearance of influencing the politics of another country.

The data set that we used is more extensive than in earlier studies, but further measurement refinements are needed. In terms of the independent variables, we improve on the earlier, cruder, industry-level measures of regulation, and we incorporate an ownership measure, a countervailing power measure, and press measures of visibility. Yet, our dependent measures for lobbying do not include dollars spent, comparable to our measures of PAC and charitable contributions, and our PAC measure needs to be expanded to stay current with the shifting patterns of electoral finance, including soft money. Also, associational political activities are not covered in the firm-level models, and the issue of whether associational activity is a substitute for or complement to political activity at the firm level remains to be addressed. Further analysis and better and more comprehensive data may help clarify some of the contrasts and similarities among the forms of political activity and may support the argument that corporate political activity, whether domestic or foreign, is complex and multidimensional.

APPENDIX A

	Measures and Sources	<u> </u>
Variable	Measure	Source
PAC contributions	PAC's total contributions to candidates in dollars	Federal Election Commission 1989; Almanac of Federal PACs 1990; Makinson 1992
Representation	Washington office staff, counsel/law firm(s) retained	Washington Representatives 1990
Charitable contributions	Donations by firm in thousands of dollars	Corporate Giving Directory 1991
Size	Sales in millions of dollars, logged	Fortune, April 24, 1989, pp. 10–30
Foreign ownership	0 = U.S. company 1 = foreign parent	Angel various years; Walke and Huckerbe
Defense.	Defense contracts in thousands of dollars, logged	U.S. Department of Defense 1987
Government shipments	Shipments to the federal government divided by total industry shipments at the 4-digit SiC code level in millions of dollars, logged	U.S. Bureau of the Census 1987a, 1987c
Union activity Index	Union PAC expenditures and lobbying, for firms with bargaining agreements	American Directory of Organized Labor 1993
Pollution expenditures	Expenditures by 4-digit SIC in millions of dollars, logged	U.S. Bureau of the Census 1988
Concentration	4-digit SIC concentration ratios	U.S. Bureau of the Census 1987b
Wall Street Journal	Logged number of Wall Street Journal citations, includes number of times the corporation is mentioned in the title or body of an article	WESTLAW
Regulation	Logged number of Interactions between firms and the EPA, OSHA, CPSC, FTC, FDA, FERC, NLRB, the Department of Justice, and federal courts on regulatory matters (see note 5)	WESTLAW
Hearings	Number of appearances by firms at a congressional hearing	Congressional Information Services Inc. 1987–88
Plants	Logged number of plants with 100+ employees	Marketing Economics Key Plants 1986-87

Varlab les	N	Mean	Std. Dev.	Minimum	Maximum	Mean $(N = 555)$
PAC contributions	280	83,120.5	106,576	105	623,380	41,69
Washington lobbylets	308	4.4	4.98	1	34	0.5
Charitable giving	215	6,062,970	12,681,900	75,000	13,540,000	2,348,72
Congressional hearings	276	6.8	10.3	1	68	3.30
Concentration ratios	555	40.8	20.5	· 6	98	40.8
Defense contracts	64	1,278,930	1,747,870	2,306	7,715,240	147,420
Government shipments	133	3,753.1	5,914.6	22.9	22,493	899.4
Size	555	3,788.8	9,028.9	91	121,085	3,788.8
Union Index	58	1.6	1.14	.001	5.05	0.17
Wall Street Journal citations	555	174.6	305.6	1	2,996	174.6
Regulatory Interactions	419	9.26	18.4	1	277	6.92
Pollution regulation compliance expenditures	457	61.1	119.9	.10	470.3	49.5
Plants	555	12.0	15.75	1	145	12.0

APPENDIX B

We are grateful to anonymous reviewers for pointing out potential endogeneity problems in three of our independent variables—union activity, regulation, and Wall Street Journal citations—and for the suggestion of using an event count model for the analysis of hearing appearances.

We recognize that union activity, as a measure of countermobilization, is endogenous by definition. Regulatory contacts and visibility, measured by Wall Street Journal citations, may be a function of past or present firm activity. We test for endogeneity in three ways. First, we created instrumental variables for our potential endogenous independent variables using size, government procurement, and industry dummy variables at the two-digit SIC level as explanatory variables. Although our results overall are not significantly different, due to multicolinearity, size (a major factor in predicting

TABLE B-1. Results of Endogeneity Test for the PAC Model Heckman-Corrected Regression Model for Corporate PAC Contributions Coefficient Statistic Varlable (S.E.) -690,527 -7.78*** Constant (8,810)631 3.02** Concentration (208)11.350 8.99*** Defense (log dollars) (1,263)Government 204,294 4.42*** shipments (46,201)8.61*** 139,053 Size (log dollar sales) (16, 160)-117,113-6.43***Foreign ownership (18,208)5.27*** Union activity index 34.324 (6,513)-6.69******* Wall Street Journal -117,563 (log citations) (17,567)2.28* Regulation (log 37,345 (16,384)regulatory Interactions) 3.03** Pollution expenditures 9.523 (log dollars) (3,145)6.94*** 133,018 Residual from WSJ model (19, 162)20,675 1.17 Residual from (17,639)regulation model 4.93*** 127,235 Lambda (Heckman) (25,811)289 Number of cases .60 Adjusted R2 *p ≤ .05, **p ≤ .01, ***p ≤ .001.

TABLE B-2. Event Count Model for Congressional Hearing Appearances (Negative Binomial Regression)

[1109BU14 Dillottikii 110910001011]						
	Number of					
	Appearances at					
	Congression					
/ }/	Coefficient	Z- Statlatia				
Varlable	(S.E.)	Statistic				
Constant	-5.19 (0.67)	−7.7 9***				
Concentration	0.001 (0.003)	0.36				
Defense (log dollars)	0.042 (0.02)	2.44*				
Government shipments	0.64 (0.74)	0.86				
Size (log dollar sales)	0.67 (0.10)	6.62***				
Foreign ownership	-0.95 (0.22)	-4 .26***				
Union activity Index	0.16 (0.11)	1.46				
Wall Street Journal (log citations)	-0.0005 (0.06)	-0.009				
Regulation (log regulatory Interactions)	0.29 (0.08)	3.71***				
Pollution expenditures (log dollars)	0.06 (0.04)	1.62				
Number of cases	555	5				
χ² (9 df)	299	9.9				
$p \le .05, p \le .01, p \le .001.$						

political activity) becomes insignificant; in particular, the correlations between size and the instruments are .94 for the Wall Street Journal, .64 for union activity, and .92 for our regulation measure.

Second, we substituted industry dummy variables at the two-digit SIC level for the three potential endogenous variables. These dummy variables pick up some of the differences captured by the three variables left out of the models, and the results for the other variables do not change, which gives us some confidence that our models are otherwise sound.

Third, in order to include the potentially endogenous variables and test and correct for endogeneity, we used the method suggested by Smith and Blundell (1986). We regressed each of the potentially endogenous variables on all the other exogenous variables in the model along with the two-digit SIC industry dummy variables, and saved the residuals from each of the three models. Then we included these residuals in our original models. If the residuals are significant, endogeneity is present. Due to multicolinearity, this test does not work for the union activity measure. Specifically, 497 out of 555 firms (90%) have no union activity by our measure. The large number of zeros in the union activity variable makes the error term highly colinear with the original measure (.87), but the small union response suggests limited endogeneity. In other words, if unions were responding to corporate political activity, we would be unlikely to

observe 90% with no activity, when more than 70% of the firms are politically active.

The results for the inclusion of the residuals for the Wall Street Journal variable and the regulation variable do confirm an endogeneity problem in our PAC model. In Table B-1 we report the results of the Heckman-corrected regression model with the included residuals. As the results show, one measure of visibility, Wall Street Journal citations, becomes negative and significant, and the residual from the Wall Street Journal model is significant, which indicates an endogeneity problem. In testing and correcting for endogeneity in this way, the significance of the other variables in the model is generally improved.

Interestingly, the endogeneity result in some ways supports our theoretical argument regarding visibility. We view PAC giving as a more visible political activity, so visibility being a function of PAC activity, as opposed to other types of political activity, indirectly lends support to our claim that the activities differ along this dimension.

For the analysis of hearing appearances, we test our model using negative binomial regression since it is a count variable. The results in Table B-2 differ slightly from the Heckmancorrected regression in Table 4. In particular, the two industry-level measures, government shipments and pollution expenditures, are no longer significant. Overall, our results appear fairly robust across the different methods of estimation.

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Recovering the Political Aristotle: A Critical Response to Smith

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Thomas Smith presents an Aristotelian view of the common good that resembles much contemporary political theory in that it focuses on ethics rather than politics. Smith contends that Aristotle is a potent remedy to a society in crisis due to its unconcern about the common good. Against Smith's apolitical reading of Aristotle, we examine how Aristotle's views of common advantage, the multiple needs of citizens, and political friendship support neither harmonizing conceptions of the good nor a personal "radical conversion" that makes the common good our primary political concern. In engaging the political Aristotle, we find instead that he is concerned with the necessary conflict that resists attempts to arrive at the common advantage, with the material basis of good citizenship, and with the institutions and practices that foster a good deliberative politics.

homas Smith's (1999) recent account of Aristotle is puzzling in that it seems to neglect almost entirely the messy business of politics and focuses instead on "a radical conversion" (p. 628) of disparate, inwardly focused individuals into citizens who have attuned themselves to a harmonized conception of the good. Aristotle is made into something he is not in Smith's oversimplified readings of the common good as well as his tendency to collapse (rather than integrate) the distinctions between the Nicomachean Ethics and the Politics and between moral and political friendship. This is done in such a way that the Politics and political friendship are largely withdrawn from Aristotle's political thought. Aristotle is reduced to a purely ethical thinker rather than a nuanced political theorist who has much to tell about our attempts at good politics. By addressing these problems in Smith's reading, we find an Aristotle who tells us not that we must harmonize our thinking to some shared conception of the good or be simply less overreaching, as Smith thinks. Rather, Aristotle is concerned with the constitution of the polis and the formation of crucial institutions and practices that support deliberative citizens and the public sphere. We also find that Smith's Aristotle is indifferent to why citizens, with all their differences, can become good citizens and why the institutional arrangements of the good republic are essential to its survival and flourishing.

According to Smith (1999, 628), we need to recognize that Aristotle wants to remove injustice and that "the cure of injustice is personal reorientation." On Smith's reading of Aristotle, family and friends can serve as a foundation for such a reorientation (p. 629), and "the common good in the family and politics" are analogous (p. 632). This we dispute.

We argue that Smith makes Aristotle's politics static and misses the disputes that mark his democratic politics. Our reading focuses on matters that are essential to both Aristotle's good politics and his good polity, matters that Smith largely ignores. One piece missing from his interpretation is the economic security and political autonomy of citizens in Aristotle's good polity, goods that Aristotle thinks provide them with stakes in their republic and its well-being. Because Aristotle's politics reflects the diversity of citizens and their possessions, any agreements about the common good and justice are matters of negotiation and always subject to revision in the good republic. Finally, we show that, although Smith wants to separate himself from liberalism, he reveals an affinity to a liberalism that emphasizes moral individualism and is indifferent to the institutional foundations that Aristotle thinks are so necessary to human flourishing and good citizenship.

THE COMMON GOOD

For Smith (1999, 626, 628), Aristotle sees injustice as what stands in the way of the common good and is pessimistic that injustice can be easily overcome. Smith leads us through an account of this central problem (which we take up later) and of remedies that ask us to reorient our thinking in moderate, just ways (p. 628). The shift from an account of the common good, with injustice as its paramount obstacle, to Aristotelian solutions for overcoming the obstacle moves too quickly over Aristotle's more complicated understanding of the dilemmas of a politics that aims at some common advantage.1 Smith's conception of the common good in Aristotle largely misses the ways in which Aristotle problematizes it. Although Smith does not believe the common good is unproblematic, he underrepresents its problematic nature and in doing so removes much that is crucial to understanding Aristotelian politics. For Aristotle, the "common good" is at best a paradoxically unattainable goal rather than something that requires us to overcome one tenacious obstacle. The difficulty is not (just) correcting injustice; as we will show, justice, equality, and the ends of politics (happiness of citizens) necessarily resist the possibility of a common good in any city.

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¹ We use this term to characterize Aristotle's concern with the good of the whole, despite necessary conflict, so as to distinguish it from Smith's use of the "common good," with its flavor of homogenization. For a view of what might be considered the common good that departs significantly from Smith's view and is congenial to our presentation of a common advantage, see Nussbaum 1990, 209.

Smith does not contend that Aristotle tells us what the common good is, but he fails to explore how Aristotle's examination of the good life, happiness, the virtues, justice, and equality all point to necessarily conflicting conceptions of these goods when we move beyond the individual to the city. There is certainly no ideal form (eidos) as there is in Plato (NE 1096a11-1097a14).2 Instead, Aristotle begins each investigation in a manner similar to his treatment of happiness: "Most people virtually agree . . . both the many and the cultivated call it happiness, and suppose that living well and doing well are the same as being happy. But they disagree about what happiness is" (NE 1095a17-21). As in all his investigations, Aristotle is not satisfied with allowing conventional belief to stand untested; he subjects it to the scrutiny of reason in order to move us beyond our uncritical assumptions. What he finds, time and again, is not that convention is wholly wrong but that "it is reasonable for each group to be not entirely in error, but correct on one point at least, or even on most points" (NE 1098b28-9). Aristotle does not provide a way to bridge entirely the initial conflict of convention through reason and, as his critique of Platonic forms shows, does not believe it either possible or prudent in the realm of the study of human action and ends (NE 1096b30-5, 1097a8-14). As long as differences in perspective remain, the good remains politically contested.

What is the source of these different perspectives? Some difference clearly comes from the poor habituation of some individuals,³ but for Aristotle some of the sources of difference that resist better habituation, and thus resist resolving disagreements over the good, derive from our material lives. Not only are some of us rich, some poor, and some middling, but also our occupations give us certain interests, certain capacities, and certain perspectives.⁴

Smith (1999, 633) notices that the political community may be a site for conflict but believes the way to reduce the conflict (and thereby increase the common good) is to inculcate the virtue of equity, which combines justice with the awareness of when unjust laws must be "straightened out." The problem is that so

long as the fundamental differences are not resolved, and Aristotle thinks they will not be, there cannot be a collectively held sense of what that "straightening" should look like.

Focusing on justice in Aristotle, we find that in political associations the mean of the virtue of justice is proportionately based on desert (NE 1132b21-1135a15), but desert is an inherently contestable concept. Aristotle states: "While there is agreement that justice in an unqualified sense is according to merit, there are differences, as was said before: some consider themselves to be equal generally if they are equal in some respect, while others claim to merit all things unequally if they are unequal in some respect" (P 1301b35-9). Because neither arrangement is wholly right nor wholly wrong, a good regime will attempt to see the legitimacy and limitations of both views and attempt to arrange politics so that these factions enter into constructive negotiation rather than risk intense civil conflict (P 1301b39-1302a15, 1318a27-b1). Aristotle further encourages regimes to foster the growth of the middle classes, who he thinks better see the merits and limitations of the two extremes (P 1295a33-1296a21).

Smith's neglect of these Aristotelian concerns seems largely to assume that a middle-class society is already in place and that the constitutional arrangements for the regime are unimportant. For Smith's Aristotle, a good politics rests almost entirely on people reorienting themselves away from injustice and at the same time refusing "to push their 'just' claims too far" (p. 633). Certainly, Aristotle stresses the importance of moderation in both the personal and political and believes that politics can be improved for the better habituation of citizens to virtuous views and actions. Yet, Smith's account suffers two critical problems: (1) It constructs Aristotle's political thought without his Politics and (2) it neglects Aristotle's larger concern with moving politics and ethics away from mere belief to something more reasoned.

As for the first problem, in the History of Animals Aristotle states that humans pursue both common and individual ends, and he implies that human communities are about the common good (H 487b33-488a10).5 This seems to correspond to Smith's reading of communities but not to the account we get from Aristotle's Politics. Here we find that "man is much more a political animal than any kind of bee or any herd animal... man alone among the animals has speech [logos]" (P 1253a8). Humans pursue both individual and common ends in their communities, and the human political community distinguishes itself from animal communities by reasoned deliberation "rather than on simple devotion to the common good" (Yack 1993, 52).

When Aristotle turns to how to improve the regime in order to help its citizens live well, he does not concern himself with tutoring personal reorientation but focuses instead on what conditions will best sup-

³ We use this term for lack of a better alternative, although "habituate" has an unfortunate connotation of passivity. Included in the habituation to the good life, as Aristotle conceives it, is the desire to be political, to listen and engage one another

² To remain consistent with Smith's style, references to Aristotle's works will be incorporated into the text with the Bekker pagmation preceded by an abbreviated reference to the work: Eudeman Ethics (EE), History of Animals (H), Nicomachean Ethics (NE), Politics (P), and Rhetoric (R) We use the translations listed in our references (Aristotle 1984a, 1984b, 1991, 1992); of the two editions of the NE listed (Aristotle 1985, 1990), we use the Irwin (1985) translation.

³ We use this term for lack of a better alternative, although "habit-

⁴ For the role of occupations, see Aristotle's discussion of the different types of democracy (P 1290a30-1292a39). Stephen Salkever finds that Aristotle not only sees diverse individuals comprising the polls but also holds that each individual has a complex, unique set of needs. These competing needs or interests cannot be distilled into some brew called the common good Rather, their integrity is best protected with a democratic politics that refuses to make one judgment dominate the rest and recognizes that "moral and political problems" are characterized by "their indefiniteness" (Salkever 1990, 138).

⁵ We are drawing from the nuanced description of Aristotle's political community in Yack 1993, 51-87.

port citizens in a reasoned, deliberative politics to settle their differences. In his explicit political theory (found in the *Politics*), he turns not to ethical solutions but to constitutional arrangements, concerns about power, and the material and educative conditions necessary for citizens to engage in better deliberative politics. He believes political communities are characterized by inevitable conflict, so attempts to arrive at agreements for the common advantage require much more than a change of thinking; in attending to politics we must develop the institutions, citizen capacities, and citizen stakes (discussed later) that will move us beyond disagreement and unexamined belief.

This leads us to the second problem with Smith's reading of the common good in Aristotle. Smith almost entirely neglects the necessary conflict built into politics and the political arrangements that as a result are necessary to develop agreement about collective ends. Consequently, Smith's Aristotle lacks the capacity to rise above unexamined belief. Aristotle constructs all his investigations into human affairs by posing what is conventionally held and then using reason to move beyond belief and convention. Practical reason (phronesis) is about what is open to deliberation (NE 1141b9-13), which is about neither what is known nor belief (NE 1142a32-b16, 1143a1-7), and requires the development of nous (NE 1141a2-8), the faculty of rational intuition about undemonstrable first principles, which for Aristotle connotes being sensible or having common sense (NE 1110a8-11, 1112a19-21, 1115b7-11). Practical reason cannot be taught through traditional schooling; it is developed through selfgovernance in one's household affairs and through participation in political deliberation in institutions arranged so that belief and convention can be challenged (NE 1141b23-1142a30).

Smith's account of developing good citizens looks to the family, education, and to a lesser extent living according to laws. These are certainly critical elements of good politics for Aristotle, but they cannot by themselves deliver it on Aristotle's account. One missing component—participation in deliberative politics not only helps citizens develop the skills to apply the laws well to particular cases but also helps ensure that the laws that habituate us are good laws. It is through deliberation that belief and narrow interest are at least potentially exposed to reasoned examination in a democracy. The common advantage is not something we develop prior to politics; it comes to a significant extent from acting politically.6 By neglecting almost entirely the conflictual nature of politics and the resulting need for constructing good deliberative practices in Aristotle,7 Smith's conception of Aristotelian politics lacks the capacity to rise above convention and belief to attend to what the good might be and how it might be

⁶ Frank (1998, 795) has a similar account of how Arustotle's conception of excellence (areie) in the polis draws on a reflexive contrast between habit (hexis) to "being-at-work" (energeia) and action (praxis).

developed politically. In this regard, it is antithetical to Aristotle's entire political project.

MULTIPLE NEEDS, NECESSITY, AND STAKES

In his discussion of the common good, Smith (1999, 626) introduces us to citizens with the disposition to grasp for more and more. His treatment of individual goods captures the side of Aristotle that warns about the dangers of a lack of moderation but ignores the many positive features Aristotle attaches to the many goods that individuals desire. Smith's partial portrait of humanity misconstrues Aristotle's view of what it means to be human. Aristotle holds that part of our humanity comes from caring about ourselves in the right ways. This does not mean leaving the world but living in it in ways that lead to our happiness. We see some of this in Aristotle's critique of Socrates' views regarding shared property and families for the philosopher kings in the Republic. On Aristotle's account, Socrates takes the natural attachments of human beings to be forces of gravity that pull them to the earth, denying them the chance to become wise and virtuous $(P \, \Pi, \, 1-5).$

Some of what Socrates takes as impediments to human flourishing is what Aristotle finds is a natural aspect of the human condition. Part of our humanity comes from the right kind of tangible attachments because they contribute to our happiness. Aristotle provides an inventory of some of the components of happiness: friendships, good children, economic security, a satisfying old age, health, reputation, and virtue (R I.5.4; also see NE bk. 1). His listing shows that a variety of goods is essential to the good life, which Smith seems to overlook in his effort to focus on the one obstacle that must be overcome if we are to become just. As Salkever (1990, 137-9, 153) shows, attending to these multiple needs in the right way is part of what makes us human. One who fails to attend to her multiple needs is defective in some significant ways. We have needs that revolve around our households, work, friends, politics, and the like, and we cannot, according to Aristotle, understand ourselves apart from each of these needs. The multiple needs of different individuals are distinctive and vary from person to person. Aristotle maintains that when we make one good or need dominate the rest, we undermine our happiness.

For this reason, Smith's common good is far too static for Aristotle, and his call for a "radical conversion" (1999, 628) is too problematic for Aristotle. Aristotle recognizes that the best politics is fluid and negotiable, reflecting the changing conditions of the members of society. He reaches for a cosmic harmony that is always on the horizon; it is an aspiration rather than a solution to impose on everyone. His political cosmos appears in public (i.e., common) space where discrete citizens work out their differences politically. Aristotle's cosmological politics provides a structured space where politics holds the parts together within a whole that is changing. All this disintegrates if one part

⁷ Although Smith acknowledges conflict, it seems to be almost entirely missing from the workings of his version of Aristotelian politics.

imposes its conception of justice and dominates all the other parts.

Aristotle looks at both the whole and the parts. Although he states that the "whole must of necessity be prior to the part" (P 1253a20), he argues that neither stands independently of the other. To degrade the parts is, for Aristotle, to degrade the whole; the whole then can no longer be said to seek what is to the common advantage. For him, it is necessary to consider both the integrity of the polity and the integrity of its many households with all their complexity, and he finds a good politics will challenge the efforts of some to dominate the rest. This is something Smith misses.

Aristotle's political cosmos not only reflects the different needs, ideas, strengths, opportunities, and vulnerabilities that reside in each particular community but also rests on the ability of all citizens to secure their basic needs. To achieve this, Aristotle has his citizens in the good state own their own property, which enables them to attend to the biological needs of their household and not be dependent on others.9 Property figures prominently because of what it does, not as an end in itself. For him, property ownership is an essential characteristic of good citizens who do not have to please others to meet their needs. This material foundation allows citizens to move beyond satisfying basic needs in order to attend to their multiple needs, including politics and the common advantage.10 This stake provides citizens with an interest in living under the laws [nomoi] as well as attending to their making. It is not, as Smith believes, that simply overreaching impedes attending to the common advantage. The inability to attend to one's material needs and security limits a citizen's horizon to self-referenced material concerns just as much as overreaching does.11

There is another reason property ownership is important to Aristotle: It provides citizens with a tangible stake in their republic. He wants them to see not only that their own good depends on their own efforts and perseverance but also that their good is intimately tied to the good of the republic. When this occurs, citizens have a stake in their own property and in their republic as well. For Aristotle, the stakes of citizens must be

more than procedural or cognitive; they must be substantive. 12

Aristotle finds that private property ownership as a citizen stake fosters a connection between the unavoidable concerns people have about themselves and a concern about their republic. They recognize that their ability to meet the multiple needs of their household depends not only on their own labor but also on the protections and security provided by their republic. In Aristotle's good regime, the good of the whole is entangled with the good of each citizen and does not stand in opposition to it when citizens experience the goodness of their republic in their everyday lives. For Aristotle, a stake is not a gift but an essential requirement for full citizenship. To deprive people of their stake is in effect to strip them of their citizenship. For this reason, private property, or whatever assures the political autonomy of each citizen,13 does not stand apart from the political but is related to it. Even if Smith is correct about teaching people not to overgrasp, such a strategy cannot work if citizens are without economic security and therefore lack a stake in their republic.

FRIENDSHIP AND POLITICS

Like his account of the common good and overreaching, Smith's account of the relationship of politics and friendship gives an incomplete and problematic reading of Aristotle's more complex view on friendship. Smith focuses on what friends have and think in common, to the neglect of a more political side of friendship in Aristotle's view. By collapsing the distinctions between true (or moral) friendship and political friendship, Smith develops a reading of Aristotelian friendship that informs his Aristotelian politics by drawing almost entirely from the Nicomachean Ethics to the exclusion of the Politics. This leads to serious problems with how to develop friendship and concord (homonoia) in the regime.

Smith (1999, 629–31) focuses almost completely on the "being of the same mind" aspects of Aristotelian friendship to the exclusion of a more political conception of true friendship and concord. True friends deliberate on the good life and correct each other's errors of reasoning (NE 1172a10–4). As we know from Aristotle's discussion of the intellectual virtues, we deliberate over that which can be otherwise (NE 1140a31–b12) rather than over beliefs we already share (NE 1142a31–b35). True friendship is like politics: We both share and disagree, and we deliberate over our disagreements with the aim of improving ourselves and

m trade or farming" (P 1320a33-40).

⁸ Salkever (1990, 51) finds that Aristotle's universe is "composed of interdependent parts which are themselves wholes" When one part seeks to disturb the harmony of the whole and uses its power to serve its interests, it destroys any conception of common advantage and denies the other parts their integrity.

⁹ For Aristotle, a citizen cannot be someone who is used by another for the latter's purposes, which defines a slave (see P 1253b33-5).
¹⁰ This is why Aristotle wants to eliminate destitution. We "should see to it that the multitude is not overly poor; for this is the reason for democracy being depraved. Measures must therefore be devised so that there will be abundance over time. Since this is advantageous also for the well off, what ought to be done is to accumulate what is left over of the revenues and distribute accumulated [sums] to the poor. This should particularly be done if one could accumulate enough for the acquisition of a plot of land, or failing this, for a start

¹¹ In this light, Pocock (1975, 203) finds that Aristotle's citizen has a "household of his own to govern so that he may not be another man's servant, so that he may be capable of attaining good in his own person, and so that he may apprehend the relation between his own good and that of the polis."

¹² For an extended discussion of stakes in Aristotle, see Terchek 1997, chap. 2; for a general discussion of stakes in contemporary society, see Ackerman and Alstott 1999.

¹⁵ We use "autonomy" in a very specific and limited way. Aristotle's good citizens are interdependent and simultaneously not dependent on (and therefore beholden to) another for their economic security. In this way, his politically autonomous citizens bring their own voice when they deliberate and participate in the regime. We do not intend for the term to carry the individualist baggage that most modern uses connote.

our relationship. Aristotle tells us that because we are political by nature we have a need for friendship (NE 1169b18-22), which implies that we have an ingrained need to deliberate on the good life, the pleasant and unpleasant, the advantageous and the harmful. If we only agreed, friendships would remain incomplete. As an external good, true friendship is pursued for its own sake and should not be reduced to a means to the common good, as Smith does. As political animals, we need to engage in politics and friendship (i.e., figuring out how to live well together despite our differences) to be fully human.

This said, we must not carry the analogy between true friendship and politics as far as Smith does. Whereas Smith wants to make political friendship into true (or moral) friendship, Aristotle wants to draw sharp distinctions between the two: "Certainly it is possible to be friend of many in a fellow-citizen's way, and still to be a truly decent person, not ingratiating; but it is impossible to be many people's friend for their virtue and for themselves. We have reason to be satisfied if we can find even a few such friends" (NE 1171a18-20). If true friendship is impossible other than among very small groups, then what kind of friendship is political friendship? Aristotle clearly considers it to be a matter of utility rather than a true friendship. "[Political] friendship... is constituted in the fullest degree on the principle of utility, for it seems to be the individual's lack of self-sufficiency that makes these unions permanent" (EE 1242a6-7).14 Political friendship, then, "looks to equality and to the object, as buyers and sellers do" (EE 1242b33).

This conception of political friendship stands in sharp contrast to the harmonious, other-regarding citizen-friends that Smith develops, but it is not incompatible with Aristotle's conception of homonoia among citizens, from which Smith builds his case. In the Nicomachean Ethics, Aristotle states: "Concord [homonoia], then, is apparently political friendship, as it is said to be; for it is concerned with advantage and with what affects life [as a whole]" (NE 1167b2-4). Because it is concerned with advantage, it is a friendship of utility. Aristotle goes on to tell us that homonoia agreeing about what is advantageous (NE 1167a24-8) and having "the same thing in mind for the same person" (NE 1167a35). This does not mean that citizens lose their self-regarding interests, however. As we see in the discussion in the Politics, the few and the many come to agreement through deliberation, a process in which both sides must address the concerns of those with whom they are in conflict in order to reach a compromise that is mutually advantageous to a majority forged from the ranks of both factions (P 1291b29-1292a37). On this account, homonoia is a political negotiation of interests that is supported by institutional arrangements rather than by a moral agreement or the harmonization of interests.¹⁵

By substituting true for political friendship, Smith overemphasizes the harmonious, moral nature of political friendship and misses the complex dynamics of Aristotle's conception. Contrary to Smith's view, Yack (1993, 35) rightly points out that friendship (philia) for Aristotle is a generic term describing "a much broader range of relationships than we are accustomed to using the word 'friendship' to describe." Instead, it describes "all expressions of human sympathy and mutual concern" (p. 35) and is akin to "friendly relations" that we have "with a much larger and broader range of individuals than those with whom we have the intimate and noninstrumental ties that we ordinarily associate with friendship" (p. 35 n. 29). Political friendships, being friendships of utility among equals, are analogous to business relations in which we have "friendly feelings" toward the partner in a mutually beneficial exchange as well as a sense of holding each other to an obligation of receiving "what is due" (EE 1242a6-13).

For Aristotle, political friendship presents the possibility of bitter conflict,16 and because of this there is also the seemingly paradoxical possibility of developing bonds with both our fellow citizens and the institutions of our political community. Because factional conflict arises from small disputes (P 1303b17), it is in political friendship itself that we find the very seeds (in the hurt and recrimination of betrayal) that can grow into intense conflict and the collapse of the regime. In these conflicts among citizens, however, there are also the seeds of civil bonds and the nurturance of political friendship itself. As we seek to enlist the aid of our fellow citizens to enforce the justice we feel is due us,17 we engage in the activity of deliberating over what justice is, which in turn develops practical reason (NE 1141b23-1142a30) and the virtue of justice in the citizenry.

Smith's (1999, 629-31) characterization of political friendship as true or moral friendship—we think good thoughts about one another and rely on moral character—is according to Aristotle one of the most dangerous beliefs we can bring into the political community. In a passing remark, Smith (p. 631) acknowledges the danger. Yet, given that the rest of his discussion of friendship is entirely contrary to that acknowledgment, we must conclude that he depicts a very un-Aristotelian place. Aristotle seems to be speaking directly to Smith, when he warns:

When, therefore, [political friendship] is based on a definite agreement, this is [political] and legal friendship; but when they trust each other for repayment, it tends to be moral friendship, that of comrades. Hence this is the kind of friendship in which recriminations most occur, the reason being that it is contrary to nature; for friendship

¹⁴ Rackham (Aristotle 1992, 415–7) uses the term "civic friendship." We substitute "political" to keep it consistent with the rest of our discussion. Aristotle uses *politike* to describe this friendship (*EE* 1242a1–9).

¹⁵ We all may agree to submit to the decision because we recognize the process as being fair, that is, there are no consistent winners and losers, and all have a fair chance of entering the deliberation and swaying others to their cause.

¹⁶ Friendships of utility, as political friendships are, are by their nature prone to complaints and disputes (NE 1162b5-21).

¹⁷ See Yack 1993, 42, for a more thorough discussion.

based on utility and friendship based on goodness are different, but these people wish to have it both ways at once—they associate together for the sake of utility but make it out to be a moral friendship as between good men, and so represent it as not merely legal, pretending that it is a matter of trust (EE 1242b35–1243a2).¹⁸

Aristotle's discussions always point out that the greater the feeling of intimacy between partners, the greater is the potential for recriminations. For political community to be stable, which Aristotle frequently raises as a basis for good politics, it is imperative that relations among citizens be based on institutional and legal arrangements rather than on presumptions of another's morality and upon an intimacy that does not exist among citizens qua citizens.

This is not to say that Smith's concern with improving the quality of political friendships is not both laudable and in line with the spirit of Aristotle's thinking; it is dangerous not to attend to the conditions of the political friendliness of citizens. Inequality breeds recriminations in any friendship (NE 1161a30b10, 1163a24-35), just as does confusing political friendship with true friendship. Providing a solid material foundation for citizens may reduce accusations, since "most men pursue what is fine only when they have a good margin in hand" (EE 1243a37). Finally, it is clear from Aristotle's critique of politics as usual in Book VII of the *Politics* that he wants to move away from citizens bashing other citizens and seeking honor to a deliberative politics that better approximates the deliberations between friends. Although Aristotle finds education important,19 his focus is on institutional arrangements in the republic, such as those mentioned above and in our discussion of common advantage.

LIBERALISM, ARISTOTLE, AND SMITH

Smith (1999, 625) offers his account of Aristotle's common good as an alternative to liberalism. He makes the not uncommon assumption that liberalism is a unified theory, but as Gray (1989) has cogently shown, not all liberals are of the same stripe. Ironically, Smith's conception of the common good features a strand of liberalism that emphasizes moral individualism, 20 something alien to Aristotle's political thinking.

Most recent scholarship on the canonical texts of pretwentieth-century liberalism emphasizes a moral content to this literature.²¹ It finds that the very freedom canonical liberals prize not only is necessary for a fully flourishing life but also can lead to the opposite. Free men and women can mistake their liberty to mean they should pursue anything they wish.

To counteract this prospect, liberals such as Locke want individuals to surmount their passions in ways that will lead to their moral development, as Locke discusses in *Thoughts Concerning Education* (1968) and *The Reasonableness of Christianity* (1823). What is arresting in these accounts is how little attention is devoted to the material or economic conditions of individuals. Rather, Locke makes individuals morally responsible for their choices and their character. To assist them in this moral quest, Locke and many liberals for some time afterward turn to religion learned in a church one freely chooses or to a moral education.

In Smith's "reorientation" (1999, 628) and the moral individualism of Locke, the focus is on how people cultivate virtue and thus change themselves. The latter wants us to deny ourselves (Locke 1968, par. 33), and the former wants us to forgo grasping. In each case, the effort concentrates on individual dispositions. With Locke and Smith, we move away from Aristotle's concerns about politics and the multiple needs and stakes of citizens, and little attention is paid to institutional arrangements. Locke's moral individualism is unconcerned about the economic security or well-being of citizens, and it turns out that Smith does not vary much from this version of liberalism.²²

Contrary to Aristotle but similar to liberals such as Locke, Smith ignores the social constructedness of individual identity and interests. The focus of Smith's attention is abstract individuals: how they overgrasp and how they need to reorient themselves.²³ With Smith as well as with Lockean liberals, there is no concern that citizens have stakes in their republic. For his part, Aristotle takes account of the social and political aspects of persons and focuses on their institutional arrangements, which is missing in Smith.

We are also troubled by Smith's discussion of liberalism. Although Smith (1999, 625) sees some problems in communitarians, most of the problems he finds in modern life are tied to liberalism, with its penchant for neutrality and individual ends. His argument simplifies matters too much by ignoring not only Aristotle's view of the complexity of politics but also the many ways that the common advantage can be subverted. In order to meet the challenges that face us, we need more than a one-dimensional explanation that hides too much to be helpful.

CONCLUSION

Smith's account of Aristotle is a political minimalism that overlooks the themes of power, conflict,²⁴ and

¹⁸ See note 4.

¹⁹ Although Smith concentrates heavily on education, his focus is not on improving the quality of deliberation, such as is found in *Rhetoric*. In Aristotelian politics, a knowledge of ethics and rhetoric as well as the educatory practice of participation in deliberative politics are all important to the development of good deliberations and judges of deliberative arguments.

²⁰ See Terchek 1997, chap. 4, on Locke as a moral individualist ²¹ For some works showing the moral content in major writers in the liberal cannon, see Dunn 1969 and Colman 1983 on Locke; Raphael 1985 on Smith; and Semmel 1984 and Ryan 1974 on Mill.

²² Smith (1999, 630) holds that "Aristotle's is an ethics of being not having." This makes Aristotle into something he is not. Being, as in being virtuous and philosophical, is obviously central to Aristotle, but he also thinks that being happy is essential, and Aristotle's happy person also possesses certain things.

²³ As pointed out earlier, this contradicts Aristotle, who clearly states that "most men pursue what is fine only when they have a good margin in hand" (EE 1243a37).

²⁴ For Aristotle, conflict is a necessary and unavoidable part of the good regime because people are different. He sees two critical

possible reconciliation that are at the heart of Aristotle's politics. Whatever else may be said about Aristotle, he does not want people to pretend that power is irrelevant in politics or that ethics can stand in the place of politics.

Contemporary political discourse often takes on a minimalist dimension, and we are often told that to make things better, citizens must change their outlook, regardless of who they are and what they have. Political minimalism seeks to settle individually problems that once were considered collective, or it ignores them. It also largely ignores power and the inequalities that reside in any society. This political minimalism animates Smith's account of Aristotle's "common good," a rendition that relies on ethics, not politics, and that avoids the hard stuff of political disagreement in favor of making friendship the foundation for civic thinking. However admirable such dispositions appear on the surface, they ignore the potential danger that Aristotle sees in any kind of political arrangement, namely, that some will attempt to use public power for their private advantage. To counter it, Aristotle teaches us to be attentive to politics and to the constitutional arrangements that assure the political presence of all citizens, not just a few. He also reminds us that citizens do not stand apart from their multiple needs and require material stakes to tie their good to the good of the republic.

Smith presents a story that is only minimally concerned with power and politics and how the troubles that pursue us can be largely solved by thinking the right thoughts. Whatever else such a line of reasoning may be, it is not that of Aristotle, who in the *Politics* shows a persistent attentiveness to a world of inequality and power and who asks how individuals can learn to share a time and place in ways that respect the integrity of each and that enable moral flourishing. At the heart of Aristotle's politics is a quest to protect the integrity and political autonomy of each citizen in a political

political issues: how to keep the conflict within a constitutional framework and how people live well together as citizens despite conflict.

cosmos that can also challenge the centrifugal forces inherent in any society.

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Ethics and Politics: A Response to Terchek and Moore

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respond to Terchek and Moore by investigating the interrelationship of ethics to politics in Aristotle's thought. To this end I defend two claims. First, doing ethics requires understanding politics. Second, politics requires practical wisdom and ethics. Since even the politics wanted by Terchek and Moore requires these virtues, they are wrong to claim that my account of the common good, resting as it does on similar virtues, is impractical and dangerous.

thank Terchek and Moore for their interesting and thought-provoking response.¹ In one way, they raise issues important to the growing debate over the meaning of Aristotle's political thought. In another their criticisms recall those in Aristotle's time who faulted the classical political philosophers for their apparent inability to comprehend the "real world" of power (NE 1095a25, 1141b4-10; Plato's Crito 44b-d; Gorgias 483d-e, 485a-d; Republic 336aff).²

In general, their presentation of my work suffers from an interpretive mistake common to many contemporary Aristotelians. Terchek and Moore read me as many interpreters read Aristotle: as a didactic theorist interested in promulgating abstract political and moral doctrines to an indeterminate audience.3 But this approach overlooks any pedagogical or zetetic concerns and thus conflates exploration and advocacy. My critics assume that in pointing out the conditions for the common good I am advocating policies to advance it, which allows them to neglect or distort my argument that the conditions for the common good point to its limits. Because they overestimate my estimation of the possibility of the common good, they underestimate my assessment of the reality of narrow self-interest in political life and the limits this imposes on reform (Smith 1999, 634). Thus, they miss my argument that sketching the conditions of the common good can be read either as an injunction to pursue it or a warning not to, depending on circumstances (p. 634).

DISTINGUISHING ETHICS AND POLITICS

Underlying the various criticisms of Terchek and Moore is the general claim that I have run together ethics and politics. The problem is that they never articulate a distinction themselves. They merely claim ethics and politics are distinct without telling us how. Related to this is their failure to refer to a slew of

excellent research devoted to a careful parsing of the distinction they casually assert.4

A brief reconstruction of their implicit account goes as follows. Ethics concerns individuals acting morally. Politics concerns the relationships among people who have irreconcilable needs and interests and who therefore come into conflict. Thus, politics is about the ways citizens struggle to protect their interests through legal and institutional frameworks (Terchek and Moore 2000, 905, 906–7, 909). Politics is primarily instrumental and should be hesitant in making substantive claims about the good life, since all such claims are contested and contestable (p. 906). This is not how Aristotle thinks about the relationship between ethics and politics. His view is more complex, and I will sketch it by defending two claims: Doing ethics requires understanding politics, and political wisdom requires ethics.

DOING ETHICS REQUIRES UNDERSTANDING POLITICS

Contrary to the bifurcation of ethics and politics by Terchek and Moore, Aristotle characterizes his inquiry in the *Nicomachean Ethics* as an investigation of politics (1094a30-b12). How is an investigation into ethics an account of politics? Aristotle writes his ethics to help people do what is good (*NE* 1094b11, 1103b26-30, 1179b1-4; cf. 1194a22-3). A problem immediately intrudes, however. Aristotle thinks that every context is formative. If his goal is to help us do what is good for us, he must somehow deal with the fact that our minds are already made up on this score. That is, doing ethics entails realizing that our regime forms us comprehensively; it affects our tastes, habits, sense of justice, and whom and what we respect and emulate. Terchek and

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¹ I appreciate the valuable suggestions of V. Bradley Lewis, Walter J. Thompson, and Kevin L. Hughes.

² Aristotle responded to these critics partly by wondering why they did not have power, since they claimed to understand it so well (NE 1180b31-1181a3). I will refer to Aristotle's works within the body of the text according to the following scheme: Nicomachean Ethics (NE); Politics (P). Translations are my own. All references to Plato can be found in Plato 1961.

⁵ For an account of these approaches and an attempt at correction, see Smith 1993

⁴ There are several dimensions to the problem of the relationship between ethics and politics in Aristotle's thought. One is textual. Aristotle presents the last chapter of the Nicomachean Ethics (NE X.9) as a prologue to a discussion of politics, but in several ways this prologue does not seem to correspond to the structure of the Politics as we have it (see Lord 1981; Rowe 1991; VanderWaerdt 1984). Another dimension is the relationship of ethics to politics in Aristotle's thought generally. Perhaps partly because of the textual difflculties, many scholars read Aristotle's ethical works as autonomous moral tracts intended to be considered apart from his political writings. See, for instance, Gauthier and Jolif 1959, ii.1-2; 10-2; Nussbaum 1986, 237, Voegelin 1957, 279 Others argue for various reasons that Aristotle's ethical thought is inseparable from his political thought (Adkins 1991; Bodéus 1993; Rowe 1991; Simpson 1998; VanderWaerdt 1984). I have addressed both the substantive and textual problems (Smith 1998).

⁵ I make a similar argument elsewhere (Smith n.d., chap. 1).

Moore fault me for failing to understand Aristotle's political thought, but they fail to see that Aristotle treats political things not abstractly or universally but always with reference to specific regimes. To him, a regime is not merely an arrangement of political offices or a formal legal order, as my critics assert. It includes offices (P III.6, e.g.), and understanding law is crucial for understanding regime, but Aristotle emphasizes that these structures serve to foster or promote the end of the political organization and the society as a whole; they seek to inculcate a way of life (Strauss 1953, 136–7).

For Aristotle, this way of life is a matter of affection. Terchek and Moore argue that by basing politics on affection (philia) rather than considerations of power, I inject a dangerous moralistic element into politics.⁶ Yet, this claim rests on a deficient understanding of Aristotle's treatment of friendship. They "dispute" that Aristotle examines philia analogously (Terchek and Moore 2000, 905), despite the fact that the Greek term analogia recurs in his treatment (NE 1158a35, 1162a15, 1163b32), and their subsequent analysis shows they do not understand analogy or its importance for Aristotle's account of human communities. This makes their account excessively moralistic because it implies that "morality" cannot be in our interest.

Terchek and Moore's reading of philia is typical of those who view Aristotle's political philosophy through Kant's notion of morality as an autonomous realm of conduct unconnected to beauty, nature, or self-interest (MacIntyre 1984, 43-7).7 This approach implies a "sharp" distinction between "true" or "moral" friendship, on the one hand, and "useful" friendship, on the other (Terchek and Moore 2000, 908), but this distinction is foreign to Aristotle's treatment of philia; he does not bifurcate morality and utility. For instance, he thinks that healthy useful relationships are in truth kinds of friendship. Furthermore, the best kinds of friends find each other extraordinarily useful in a variety of ways (NE IX.9; 1172a12-5). Analogous terms resist sharp distinctions because they resist both definition and the application of a single account that does not vary from one context to another, yet still demand employment in diverse contexts in spite of acknowledged differences in meaning (Burrell 1973, 26). In

short, analogical language is necessary for realities that are both different and alike. Terchek and Moore are right to say that for Aristotle political friendship is different from other kinds of friendship, but they are wrong to overlook their similarities. Contrary to their reading, the principle of distinction among the kinds of philia is not whether morality is present, and thus whether the friendship is somehow "true," but what kind of affection or love animates the philia in question (NE 1155a33ff). That is, for Aristotle the real question to ask when distinguishing different kinds of relationship is: What kind of love binds it together?

Aristotle thinks that political regimes are more than instrumental arrangements of laws and institutions (P III.9.1280b5-12, 1280a31-4, to take but a few examples) because they cultivate more or less determinate ways of life to which the citizenry is dearly attached. In his view, when a regime arranges its offices; passes laws against running away in battle, public drunkenness, swindling, or discriminatory housing policies; or issues tax breaks to encourage certain economic or religious activities, it is making broad claims about what kind of life is good for citizens of that regime. One way of getting at the affection that binds citizens to their regimes is to ask for a list of what they would be willing to die for. This would typically include family, friends, and what the French call patrie, which encompasses one's territory, political structures, history, and way of life generally. These are clearly useful relations: The land gives life; political arrangements protect and govern; fellow citizens make social life possible. But our political relationships are both useful and matters of intense affection. To argue that people should love their homeland instrumentally is to ignore their deep and abiding affection for it, which is based on more than a rationally calculated assessment of the worth of legal and constitutional arrangements meant to protect them from power.

Philia is an analogous term because people typically say they love their families, friends, and regimes, even though they mean different things in each case. Aristotle is realistic enough to know that the question for most citizens is not: "Should we love our regime?" Because affection is already present, the real questions are: "How should we love it? Do the affections animating a particular polity tend to make its citizens flourish or not?" Terchek and Moore are correct to point out that it is dangerous to run together political and filial affection, but they go on to argue that because affection is dangerous, we should avoid it in politics. Yet, if

⁶ Terchek and Moore (2000, 909-10) rely on a passage from Eudemian Ethics to argue that Aristotle thinks it dangerous to rely on "moral character" in politics. Yet, they completely misread it. In that text Aristotle differentiates the relations between citizens and business partners. His warning does not concern the relationships among citizens, which aim at equality and are protected by law. Instead, he warns business associates not to mistake their partnership for a relationship of trust unbound by legal rules. In these relationships, there is no legal protection, and recrimmations are rife. A careful reading of the passage shows it is not about politics at all. Rather, Aristotle is urging us to protect our commercial dealings with legal arrangements (EE 1243a5-7; cf. the corresponding passage at NE 1162b26).

⁷ For example, according to Terchek and Moore (2000, 910, n. 22): "Being, as in being virtuous and philosophical, is obviously central to Aristotle, but he also thinks that being happy is essential." This bifurcation of happiness and virtue is utterly unAristotelian because, for Aristotle, happiness is virtuous activity (NE 1098b31-1099a7, 1098a16, 1101a14, 1177a12-b26).

Strangely, despite the criticism that injecting affection into politics is dangerous (Terchek and Moore 2000, 909), elsewhere Terchek (1997, 48) acknowledges that people typically love their political communities: "Patriotic citizens have a normative or ideological investment in the vitality of their republic. They take it to be good and reach this conclusion on the basis of their civic education as well as from their experiences in society." They "value" their republic for a variety of civic and private reasons.

⁹ I make this point explicitly (Smith 1999, 631), and my critics acknowledge this (Terchek and Moore 2000, 909). Yet they dismiss my caveat, saying that it is "entirely contrary" to the rest of my analysis (p 909). An interpretive approach that dismisses an author's explicit claim is uncharitable and makes rational conversation diffi-

the authors are right that any intense affection in a useful relationship is so dangerous that it should be avoided, we then ought to ban marriage and familial childrearing as well (cf. Plato's *Republic* 457d, 460c).

Aristotle insists that politics is architectonic, for regimes affect every aspect of human life, including our individual formation (NE 1141b21, 1141b24-8; P 1252a1-8, 1260a17-9). The regime manages the overall shape and direction of the constituent parts of the political community because its citizens are always acting on some conception of what is good when they pass laws or engage in typically respectable practices. For example, politics determines the kinds of science practiced, the kinds of arts permitted and encouraged, what sort of education citizens receive, which social activities will be forbidden and which fostered, what sorts of goods can and cannot be sold, what counts as a marriage, and so on (NE 1093a27-1094b12).

The problem is that regimes deform more or less. Most regimes are defective because the ruling element in each foists its own interests on everyone else and calls this "justice" (e.g., P 1301a37-40 and generally V.1, III.9, III.12). Aristotle thinks that all regimes skew their politics to the interests of the ruling element rather than the interests of the whole or the common good, so all existing regimes are more or less unjust (P 1252a7-9, 1324b32-3). It follows that he believes most human beings are not virtuous (NE 1150a15-16) precisely because they are formed by the habits, customs, and laws of regimes that are more or less defective (NE 1796b16-20). Indeed, the situation is worse than it initially appears: All regimes necessarily form us, but most do not reflect on the problem as such.

Aristotle says that only in Sparta and a few other cities has the legislator thought about formation (NE 1180a24-9). His famous criticisms of Sparta (P II.9) force us to realize that, in the only places where citizens reflect on the issue, he thinks it is done so badly. Yet, although the formation our regimes give us is by and large defective, it is also necessary. Nature has underdetermined us. More than any other animal, human beings desire things as a result of habituation and reflection, rather than simply as a result of inherited biological factors or instincts. The actualization of these uniquely human qualities, however, requires socialization (NE 1180a4-5; Salkever 1991, 12). Thus, we require an education we cannot supply.

Some prominent interpreters argue that the *Nicomachean Ethics* has a political dimension because it provides a kind of basic training for politicians (Bodéüs 1993, 123; VanderWaerdt 1985, 77–89), but that is only part of the story. Aristotle thinks that any attempt to inquire into ethics must take into account the way one's character has been formed by one's regime. His pedagogy has a political dimension because the effort to make people good is inseparable from a resistance to their deformation. The *Ethics* is a semipublic act of countervailing persuasion aimed at reforming personal

and social life through the reorientation of the souls of Aristotle's students.

Aristotle argues that this countervailing resistance requires learning about politics. Right after his sobering discussion of the ways regimes and the households within them fail to form rightly, he states: "Our next task is to consider from what source (pothen) and how (pos) we can become practitioners of the lawmaking art (nomothetikos)" (NE 1180b28-9). Due to widespread misunderstanding, it is crucial to be clear about this passage. The traditional account glides over the distinction between learning about law and learning where and how to learn the lawmaking art. Consequently, most scholars assume that Aristotle in this passage means that he is going to speak about what legislation is in the *Politics* (Lord 1981; Rowe 1991, 70-2). This passage baffles many interpreters because it seems to promise a discussion of legislation that never appears in the Politics. Yet, Aristotle never says he will discuss legislation in the Politics. Rather, his task is to inquire where and how to learn about it. Why?

The point of the *Politics* is not merely to learn how to enact laws, as Terchek and Moore argue (2000, see e.g., 905, 910). Rather, Aristotle says we should study these questions so that "each one will be better capable of attaining virtue and leading loved ones to virtue" (NE 1180a33-4). This is a reiteration of his earlier point that one who wishes "to make people better through his care, whether many or few, must attempt to become expert in the lawmaking art" (NE 1180b23, emphasis added). Unlike Terchek and Moore, I believe we cannot read these passages as a recommendation to know the art of lawmaking so that "reasonable people," upon grabbing the reins of power, can "protect people's interests" through law. Aristotle believes that human beings tend to be unreflective about the ways they have been deformed. Most of us tend to selfrighteousness by overestimating our own capacity for virtue. Yet, if most of us are deformed more or less by defective regimes, our first task would be to be on guard, lest we identify our own narrow interests with something like the common advantage.

Contrary to Terchek and Moore (2000, 908–10), I do not think that grabbing power and balancing interests through law is sufficient. The problem is that most people have a skewed understanding of their interest, and this makes political reform very difficult. From an Aristotelian perspective, any political reform runs the risk of mistaking one's own interest with the interest of society. Moreover, even if the reformers are correct, the defective formation of the larger regime entails that they cannot hope their decrees can resolve the fact that many will not see the sense in reforming policies.¹⁰

The main reason for learning the art of lawmaking is to become more reflective about how the regime's laws deform us in order to help ourselves and our loved ones resist that deformation more successfully. Only by achieving a sense of the ways we have been deformed by our surrounding culture can we achieve some mea-

cult, since in principle any claim can be reinterpreted according to the interpreter's whim.

¹⁰ I make this point in Smith 1999, 631, 634, n. 14.

sure of thoughtfulness and genuine direction of our lives and the lives of those we love. That is, since politics forms us decisively, knowledge of politics is necessary to bring people a bit closer to more genuine human excellence (P 1259b18-9). This process has strict limits because any community within a regime is not self-sufficient. We cannot hope to withdraw from political life because our households and friendships rely on larger structures for resources and protection. For Aristotle, our task is to become more thoughtful about our regime's assumptions and how they affect our lives so that in very practical, local ways we can do something about them. Understood in this way, for an Aristotelian there is a political dimension to most of our daily undertakings, from rearing our children to engaging in friendship, religion, or economic life.

In my article I articulated a motive for this care in formation by sketching an example of a common good in the family.¹¹ This motive flows from our interdependence. In the family, we depend on our loved ones' differences for life, sustenance, and socialization. Analogously, in politics, we depend on our fellow citizens to participate in civic life reasonably, socialize us and our children, defend us in battle, pay their taxes, obey the laws, help us in hurricanes or earthquakes, and so on. Because we also divide up tasks in political communities, we depend on differences in this as well. The fact that we depend on fellow citizens means that we have a motive to be responsible with and to them. By mistaking my account of motive for an account of habituation, Terchek and Moore overlook the issues I raise. Differences within families can lead to conflict. but they also can be opportunities for loving cooperation. In politics, differences can be an occasion for conflict and might require balancing competing claims and interests (P III.13). But can diversity lead to a convivial interdependence in which citizens set aside distrust and competition for mutual benefit? Indeed, might a common good precisely depend on our diversity and interdependence insofar as homogeneity renders us less useful to one another?12

Politics differs from other architectonic practices. Bricklayers need not know anything comprehensive

about architecture to do their job; in this case, knowledge of the part does not necessitate knowledge of the whole. In contrast, the formative dimension of political regimes entails that knowledge of the part necessitates knowledge of the whole. Our roles as mothers, fathers, friends, accountants, lawyers, members of religious communities, or participants in an economy are so decisively affected by our larger regime and the way of life it fosters that in order to do these tasks well, we need to know a great deal about our politics. For Aristotle, genuine knowledge of how to form ourselves and our loved ones requires comprehensive knowledge of politics, because the regime is the overarching context that decisively affects each of its parts. In this way, ethics is not separable from politics in the way Terchek and Moore imply.

PRACTICAL WISDOM REQUIRES ETHICS

I have shown that doing ethics requires knowing politics. Yet, are Terchek and Moore correct to point out that the Politics is all about institutional arrangements and balancing competing diverse interests rather than getting people to be good? Ironically, given their criticism of my work, their account is abstract and apolitical. Above all, they miss the way Aristotle analyzes legal and institutional arrangements in the Politics. Again, he examines these with reference to particular regimes. Contrary to Terchek and Moore's interpretation, the *Politics* is not about legal or institutional arrangements abstracted from any and all circumstance; it is about the politics of different kinds of regimes, and the ways different regimes make claims about the good life through the practices they encourage and forbid. It follows that any arrangement of offices or legal structures must pay attention to the unique circumstances of the regime for which they are intended. Laws and offices that are perfectly reasonable in South Africa might be disastrous in Peru and vice versa.

Contrary to what Terchek and Moore assert, my account is consistent with Aristotle's account in the *Politics* of the causes of civil strife (*stasis*) and the ways to overcome it in particular regimes. Aristotle says that civil strife occurs because all regimes are only more or less just (*P*1301a27; cf. Smith 1999, 626). Factional conflict "everywhere" is the result of some perceived slight, when a citizen feels s/he is not being treated fairly. These disaffected citizens overreach for an excess of profit or honor when they feel that their regime has not given them their fair share (*P* V.2; cf. Smith 1999, 626-7).

Terchek and Moore argue that I read Aristotle's political thought through the *Ethics* and so miss his advice in the *Politics*, which does not focus on virtue but on political arrangements. What does Aristotle recommend in the *Politics* in order to avoid civil strife? He provides different recommendations for each regime he discusses, but all have one theme in common: Since the cause of civil strife is injustice, the best way to avoid it is to moderate the regime in the direction of

¹¹ Terchek and Moore misread this account of motive as an account of habituation, which leads them to claim that my argument is individualistic. Yet, I provided no account of habituation. In fact, they ignore the only statement I made about habituation, perhaps because it flatly contradicts their claim that I construct an individualistic account limited to the family: "In contrast to those who extol family values" in our time, Aristotelians would insist... that good households are a necessary but not sufficient condition for the proper formation of human beings" (Smith 1999, 631).

formation of human beings" (Smith 1999, 631).

12 Terchek and Moore (2000, 905, 907-8, 910, 911) are wrong to say that I favor a "static" "homogenized good" to be imposed "from the top down." Shareable goods pursued in common, such as friendship, are not static. Indeed, I claim these goods are satisfying because they result from and are productive of shared dynamic action (Smith 1999, 629-30). Furthermore, I consistently argue that the common good flows from interdependence and thus requires human diversity and heterogeneous communities (pp. 631-2, 655). I point out that Aristotelians insist on a "variety of good households" (p. 631) as well as a "variety of common goods," depending on political circumstances (p. 633). Finally, I say explicitly that the common good cannot be imposed from above (p. 634).

justice. 13. For example, he tells both democracies that tyrannize over the minority and oligarchies that tyrannize over the majority to moderate their rule in the direction of justice by mixing their deficient regimes together. He tells tyrants to moderate their rules in the direction of stewardship, for by caring for their subjects justly they can make their reign more stable (P V.12). In every case, regimes are made more stable by drawing them more toward moderation (P 1313a18-9). Terchek and Moore say the focus of the *Politics* is on laws and institutions, not virtue. But Aristotle's point in the Politics is that knowing how to arrange laws and institutions for stability precisely requires virtue. Even a politics that aims at the common advantage for the sake of security and stability requires striving for the virtue of justice. He thinks that the commonwealth can only flourish when citizens actively care for it, and he gives people a motive to do so: Their regimes will be more harmonious and stable if they do.

It follows that Aristotelian citizens have a twofold task. First, they must have "affection toward the established regime" (P 1309a33) so that they can preserve it. Second, to preserve their regime, they must have the critical capacity to know when and how to moderate it in the direction of justice (VanderWaerdt 1984, 244ff). Just as the virtue of an individual in a household demands a countervailing attitude, so the virtue of Aristotelian citizens demands it so they can bend the regime toward justice. Leven the kind of stable politics Terchek and Moore want cannot be had apart from practical wisdom's evaluative-critical dimensions.

Finally, politics requires ethics because practical wisdom cannot be had apart from virtue. On Aristotle's account of action, human beings are always motivated by some object or action experienced as good (NE 1094a1-3; P 1252a1-6). In this sense, the beginning (arche) of action (in the sense of its originating motive) is the end or purpose (telos) of the action. For Aristotle, the disposition of our characters influences the way we perceive actions and goods. For this reason, he argues, vice destroys the initiating principle or motive of right action (NE 1140b15-20) because it truncates our experience and so our perception; a vicious person has not tasted the attractiveness of certain goods and so does not see them as choiceworthy. Stable politics requires both practical wisdom and ethical character. 15

CONCLUSION

The passionate response by Terchek and Moore stems in part from their healthy skepticism about abstract virtue-talk. In particular times and places we may know what a good family is, or a good home economy, neighbor, or friend. And we may know what habits and activities are required to sustain these. But what is virtue in the abstract? For an Aristotelian, virtues are understood with reference to the specific practices and communities they sustain. Otherwise, they are complicated forms of moralistic self-hating propagated by holy rollers afraid of the communal sky falling. Elsewhere Terchek (1997, 7, 48-55) argues forcefully that those who promote self-denying republican conceptions of virtue do not pay adequate attention to the ways ordinary citizens live; ordinary citizens both care about their regimes and are self-interested individuals who need stability and property. My critics believe my account is apolitical because I have not taken this into account.16 They go on to argue that in my enthusiasm for virtue I overlook the legal and institutional structures necessary for a stable common advantage.

Their critique misses the fact that even the politics desired by Terchek and Moore requires virtue, so it overlooks at once our need for practical wisdom, its difficult requirements, and why it is in our interest. Let us assume for the moment that Terchek and Moore are correct to insist that a common good is impossible because it requires individuals to be good, and agreement on the good is impossible in politics because of irreconcilable multiple needs and desires. Let us limit our discussion of politics to what they call the common advantage, which is a politics structured around legal and institutional frameworks. This common advantage is a human problem, and it requires a human solution; particular men and women have to foster it politically. Yet, how are the people involved in politics supposed to know what institutions and legal structures are appropriate for their particular time and place? Knowing how to acquire power is not the same as knowing how to use it well. Politicians require practical wisdom even in order to know how to use their authority to

¹³ For reasons of space I will limit my examples to two recommendations. It should be noted that even when Aristotle does not specifically recommend policies to moderate injustice, his account of the origins of civil strife implies them. Once we know the things that destroy regimes, we also know the things that preserve them (P 1307b25). It is also worth noting that Aristotle's discussion of regime preservation does not necessarily mean he is a stooge of the status quo. It follows that if we know what preserves regimes we also know what undermines them.

¹⁴ This seems to be one reason Aristotle insists that *phronem* is a single virtue with several different manifestations, such as the practical wisdom required to do politics and household management (NE VI 8)

¹⁵ Terchek and Moore are right to point out that citizens within regimes often engage in heated disputes over the meaning of the good life. But they overestimate the need for some deep consensus on the good in order for statesmen to act well. For instance, Terchek (1997, 216–25) holds up Lincoln as an example of a good politician.

Yet, Lincoln acted virtuously amid the disagreements of the Civil War in the absence of a plebuscite or joint resolution of Congress on the meaning of the good. Terchek and Moore insust that I overlook the fact of political disagreement in my desire for citizens to act well, but this kind of shining public action does not require some deep overlapping consensus, only wisdom and courage.

¹⁶ I argue in my article that the demand side of mjustice stems from our tendency to take too much of what Amstotle calls the "goods related to fortune." I go on to give Aristotle's list of these goodshonor, power, and money. Terchek and Moore claim that I think we should avoid every external good, but in doing so they conflate the goods related to fortune with all other external goods. Since they miss my distinction between different kinds of external goods, they mischaracterize me as an impractical Puritan who devalues children, health, property, marriage, and friendship for the sake of some self-denying conception of virtue. This leads them to claim that I will not give citizens a stake in my apolitical regime, since I will not let them have property and stability. That overlooks my argument that by overgrasping for the nonshareable goods of honor, money, or power we lose sight of the value of shareable goods, such as friendship. Indeed, my examples of shareable goods are the very ones that the authors say I spurn (Smith 1999, 632-3).

achieve what my critics want them to. Where does this practical wisdom come from? Terchek and Moore are right to say that experience in politics is a necessary condition for its development, but is it a sufficient condition? Can a politician single-mindedly obsessed with advancing her own power or honor acquire practical wisdom? Or is her overgrasping ambition a clear sign that she has left the path of wisdom? Can a money grubber obsessively concerned with his material wealth be said to possess wisdom? Or is excessive love of money a tangible manifestation of foolishness? My critics ignore Aristotle's argument that overgrasping for honor, power, and profit are inimical to the wisdom and justice that even a clearheaded devotion to the common advantage requires.

Can the common advantage Terchek and Moore want be sustained indefinitely in the face of a regime full of narrow self-promoters who skirt the institutions and laws intended to keep them in their proper bounds? Can communities survive through competition and mutual suspicion of differences? Or is it the privilege of human beings to need interdependence, justice, forebearance, and mercy?¹⁷ If our regimes require these qualities, then it is in our interest to develop them, as difficult and subject to dispute as they are. And if such virtues are needed to sustain the political communities that sustain us, we cannot bifurcate virtue and self-interest simplistically. If politics demands these virtues, we cannot say an argument for them is apolitical and impractical. Proceeding from these reflections, it is eminently practical to wonder about the conditions of a political community in which each is valued because each contributes through diverse talents and perspectives to shareable goods all can enjoy. For Aristotle, all regimes form the characters of the citizenry, so all cultivate determinate conceptions of virtue. To him the question is not whether virtues will be cultivated through politics but what kind they will be. It may be a dangerous kind of thoughtlessness to refuse to wonder about the virtues a common good requires in the name of a narrow conception of "practicality."

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¹⁷ I use mercy as a synonym for equity. For this, see Nussbaum 1993, 92-6.

BOOK REVIEWS

Political Theory

Democracy, Real and Ideal: Discourse Ethics and Radical Politics. By Ricardo Blaug. Albany: State University of New York Press, 1999. 229p. \$65.50 cloth, \$21.95 paper. Democracy in Dark Times. By Jeffrey C. Isaac. Ithaca, NY: Cornell University Press, 1998. 250p. \$45.00 cloth, \$16.95 paper.

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These are two deeply pessimistic books about the current prospects for democracy. The authors valorize civic or popular struggles over and against the state and nation, but they do not exaggerate the potential to transform the present political order or to transcend the limits of liberal democracy. Isaac writes: "At the level of the nation-state the 'rule of the people' must always be more or less metaphorical, channeled, diluted, and corrupted by mass political organizations and bureaucratic structures" (p. 120). He stresses the value of imperfect forms of political response and oases of civic initiative, such as ACORN, the Algebra Project, Planned Parenthood, Amnesty International, and Charter 77. He argues the need to replace the populist vision of massbased reform movements with localist initiatives; his is a modest and pessimistic progressivism. Blaug is more blunt and more radical: "That we could ever learn to be citizens on such a scale as to be able to rule ourselves must be considered very close to impossible" (p. 156). Nevertheless, he concentrates on the possibility of extending the life of "democratic breakouts," or social movements, beyond their initial mobilization to the point at which they can take on the revolutionary task of building democratic institutions.

Isaac, in particular, delivers a compelling, if dark, portrait of the present. His title, Democracy in Dark Times, echoes Hannah Arendt's collection of essays, Men in Dark Times (1968). Isaac refers to the meaning of 1989, to the opacity of democracy in the post-Soviet era. Far from triumphal, the liberal democracy emerging from 1989 (in the East as well as the West) is nearer a state of barbarity, and civic initiative can just barely resist the corruption endemic to the large-scale bureaucratic politics of the nation-state. The initiatives to which he refers—Solidarity, Industrial Areas Foundation, and battered women's shelters, among others—aim to amplify public outrage at the horrible injustices and mass slaughter darkening the present as well as at the cover-ups, the ways people are inured to horror by the media, bureaucratic dissimulation, and cynicism. Isaac is certain that the prospects for democracy in the present are limited to such initiatives, "oases in the desert" of democratic politics.

Rather than turn to theory for a vision that could transcend the limits of liberal democracy, both authors attempt to make political theory speak directly to the present. They do not abandon or reinvent political theory so much as they attempt to reshape our expectations of it to fit the limitations of the political present. It is on this score that readers will be rewarded, not with new approaches or answers to old questions, but with a glimpse at what democratic theory might accomplish once, in Blaug's awkward phrasing, we "change the object domain of application." That domain, in both books, is at the level of association or organization where individuals struggle to practice democracy under imperfect if not horrifying conditions. Blaug abandons the terrain of the ideal speech situation in order to apply the discourse ethics of

Jürgen Habermas to the real problems of judgment encountered by political actors. Isaac attempts to shift the focus of political theory to first-order political questions and away from the problems of interpretation and epistemology. Both eschew any analytical effort to reconceptualize the political; their efforts are directed only to the forms of democratic action now available.

For all their shared focus on democratic participation, however, the two disagree sharply on the question of whose participation matters. Despite his own pessimism, Blaug struggles to retain some hope for mass democracy, whereas Isaac's politics, local and voluntarist as they may be, are elitist. Noting that Arendt was no friend of mass democracy, Isaac plays her elitist and democratic sides off one another to tease out what he believes is a truly democratic elitism. These Arendtian elites "are distinguished by their insulation from the many, not by their rule over them" (p. 104). As selfselected, such elites are distinguished only by their extraordinary civic mindedness and love of freedom, not by their wealth, political experience, or power. Isaac attempts to convince the reader that Arendt's argument is unconventional but not antidemocratic, that it is possible to be both elitist and democratic in the same breath. He asks: "But again, what about the masses" (p. 107)? Yet, he cannot give an adequate answer. Although he holds political theory to the fire, Isaac nevertheless concludes that even a more relevant and engaged mode of inquiry is not likely to find an audience in a world "in which political illiteracy and public attention deficit disorder are endemic" (p. 56). Pessimism about the prospects for mass democracy are requisite for Isaac's version of a political theory attuned to the present.

Whereas Isaac's collection of essays revives (some might say "invents") Arendt's democratic theory, Blaug finds few sources for a theory of radical democratic politics in the work of Habermas. He concludes: "There is no Habermasian politics" (p. 82). Blaug's project is to explain why. The first three parts of the book offer an arduous account of why Habermas fails to offer much to political actors and how the theory of deliberative democracy reduces radical democratic politics, at best, to a temporary counterfactual. Blaug then gestures too briefly toward the Italian Renaissance language of political action to bridge the divide between the real and the ideal, or the empirical and normative in democratic theory. On the subject of participation, in a chapter entitled "Changing the Object Domain of Application," Blaug leaves the problem of ideal speech behind, and his work comes alive. He startles the reader with a dramatic change of writing style and by turning to the empirical problems of sustaining a "democratic breakout." He offers a vital study of a generic social movement to explore the problems participants face in order to sustain its democratic legitimacy beyond its own institutionalization.

A key problem for both authors, treated as a contradiction by Blaug and a paradox by Isaac, is that the sources of genuine democratic participation and legitimacy under liberal democracy are to be found only in the most fragile political practices. Any incorporation or institutionalization of these democratic practices saps their legitimacy and deliberative strengths. Although neither cites the work of Frances Fox Piven and Richard Cloward (*Poor People's Movements*, 1979), both revive the problematic of movement mobilization versus organization. Isaac argues: "This tension between civic initiative and mass political organization is the terminal condition of modern democratic politics" (p. 122). The paradox is that any attempt to bolster civic initiatives will prove corrosive because they must remain small, participa-

tory, and nonbureaucratic. For Blaug, the tradeoff between participation and efficiency leads to a contradiction that follows from "the entropic nature of legitimacy" (p. 144), when the collective and discursive processes of democratic breakouts become normalized. Neither author offers a solution for this problem. Like Piven and Cloward, they attempt to reorient political analysis to the fragile, even ephemeral practices of democratic participation.

Isaac gives a passionate account of how constricted various national democracies are, not only by state and ethnic violence but also by the "aversion to first order inquiry" of most contemporary democratic theory. He scrutinizes the treatment of 1989 events in American political theory and finds a "strange silence" on the subject. (His indictment is debated in several essays in Theory & Event [http://muse. jhu.edu/journals/theory_+_event/vool/1.1wolin.html]). Much in Isaac's account of contemporary political theory rings true, especially its tradition-laden and overly academic character, but he roundly refuses to engage the attempts by cultural theorists, poststructuralists, queer theorists, postcolonialists, and feminists to reinvent political theory, favoring "less culturally radical forms of political association" (p. 15). Unfortunately, he does not thoroughly explain his preference despite the fact that these theories so deeply influence many of the civic initiatives he so values. Similarly, Blaug limits himself to the works of Habermasians, with a brief appearance by Wittgenstein. Both of these books would be improved by first-order engagements with theoretical movements that are also searching for sources of democracy in the present.

Although they are generally critical of the current state of political theory, both authors remain firm believers in the idea that political theory can and should inform political life. (Of the two, Isaac has the better chance of reaching a politically engaged audience; his essays are eloquent and readable. Blaug writes in a style familiar only to those versed in discourse ethics, with the clear exceptions of his introduction and the last two chapters, which are more readable.) Isaac holds that the role imagined for political theory in the Enlightenment is hopelessly antiquated in the postmodern polis, yet he hopes to revive the humanistic, antimodern, and action focus of Arendt to secure a guarantee of human dignity as a new universal foundation for democratic politics. He wants to sustain the humanistic strains of political theory in the struggle against injustice, but he abandons the idea that a single grand theory or struggle will procure justice. Although still wedded to the Enlightenment view of political theory, Blaug attempts to reorient theory away from the grand scheme to the practical concerns of political actors. Both authors favor the provisional over the visionary and prescriptive conventions of political theory. For all their pessimism, their approaches will surely inspire others to follow more provisional pathways to political theory and democracy.

Democratic Devices and Desires. By Geoffrey Brennan and Alan Hamlin. Cambridge: Cambridge University Press, 2000. 267p. \$59.95 cloth, \$22.95 paper.

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The central argument of this book is that the standard rational actor model is an impoverished representation of individual motivation. To replace it, the authors use a modified form of rationality, one that allows individuals to be

guided by moral and other considerations that are not "instrumental." Part 1 focuses on the nature of individual motives ("desires"), and Part 2 asks how different democratic institutions ("devices") can be expected to function from a normative standpoint under the modified rational actor model. The primary conclusion is that popular elections, representation, political parties, separation of powers, and division of powers all better serve the public interest if citizens are represented by a model of "expressive" rationality rather than by the standard model.

As the authors make clear, their purpose is primarily normative. Their model of modified rationality is never mathematized (with a brief exception in chapter 6), and their arguments are almost entirely verbal. The few examples scattered throughout the text are of some interest, but the use of modified rationality is ad hoc, generally represented by an arbitrary constant added to the standard model (as in chapters 6 and 8). Still, the arguments are appealing, and it would be interesting to see how standard results in public choice theory might be altered by a fully developed model of modified rationality. The problem, of course, is that one does not exist.

It is possible to take exception to some of the characterizations of the standard model. The authors argue for expressive voting because instrumental voting makes little sense in mass elections. This fails to note that politicians, not voters, are the strategic actors in this setting. Also, the authors appear to be unfamiliar with several standard voting models that answer some of their concerns. Probabilistic voting is a case in point. Models with candidates who have nonpolicy characteristics as well as issue positions are another. Also, a number of works using the standard model allows voters to abstain from alienation, contrary to the authors' belief (p. 135).

Brennan and Hamlin argue that the standard model takes a negative view of such institutions as representative government, political parties, and the rest. I find this argument understandable but somewhat forced. Standard public choice analysis typically takes political institutions as given rather than ask such questions as whether the public is better served by representative than direct democracy. Normative questions of this type are typically outside the scope of public choice theory, both because most researchers are positively (i.e., scientifically) oriented and because almost any set of normative criteria can be expected to be mutually inconsistent (à la Arrow). So, forcing public choice theory to "take sides" on the question of whether institution A is normatively preferable to institution B is somewhat contrived and, given the complexity of the literature, probably impossible.

As long as the normative purpose of the authors is kept in mind, the book makes many telling points. A common-sense rationality characterizes most of the arguments, but I think the authors generally miss the point that rational choice theory is designed to do scientific analysis rather than answer normative questions at the societal level. The rationale for homo economicus is not that he realistically represents human motivation but that this assumption allows the deduction of a mathematical set of empirically testable propositions. If "moral man" can be given the same mathematical foundation as "economic man," then empirical testing will tell which is the better model. Without such a foundation, morality is simply an "add on" to the basic model.

Republican Theory in Political Thought: Virtuous or Virtual? By Bill Brugger. New York: St. Martin's, 1999. 211p. \$59.95.

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At a time when communitarians are moderating their once radical critique of liberalism by an embrace of individual rights and liberals are calling for more robust community, Bill Brugger offers to satisfy both groups with "republicanism." The title refers to an old paradox of republican self-government: The people are both sovereign and subjects, rulers and ruled. Since immoderate and partisan appetites sometimes move us toward domination and license, we must somehow rule over ourselves. We can do so in a classical or communitarian manner, by cultivating self-sacrificial virtues informed by a notion of an exalted common good. Or we can do so by delegating rule to representative lawmakers who will determine by cool deliberation what a rational collection of individuals would choose for its security and comfort. This second and characteristically modern or liberal solution is indirect government, or what Brugger calls "virtual republicanism." But Brugger argues that the division drawn between the two kinds of self-rule is overdone; modern republicanism always intended to provide the best elements of both virtuous community and individual freedom secured by liberal institutions. Properly updated, it can still do so.

The pressing need to do so is found in the face of democratic wave theorists, like Francis Fukuyama, whose view of democracy ignores and therefore abets "half-understood structural domination" (p. 20; see also pp. 118, 120, 126-7, 152, 163, 175, 181). Brugger thus appeals to the Left's long-held suspicion that liberal democracy is quietly, structurally oppressive—that its official neutrality or tolerance on questions of the good life permits and fosters domination or marginalization in the private realm. In particular, he stresses that globalization has seriously weakened popular sovereignty, permitting a corrupt, monied, power elite to eclipse the people's formal representatives and make those outside of corporate organizations "in effect second-class citizens, victims of a process of social and political marginalization" (p. 62; see also pp. 75, 122, 131-4, 164-5, 175). Virtuous republicanism seeks freedom from this and from "all dominating relationships." It would extend nondomination "to children, women, and many others. . . . [It] must appeal to socialists, environmentalists, feminists, and advocates of multiculturalism" (p. 42; see also pp. 127, 131, 137, 141). Republican nondomination would extend the liberal state's concern for freedom into the social or private realm.

Liberals, Brugger knows, will worry that his concern for virtue and freedom will yield not freedom but totalitarian oppression (p. 16; see also pp. 20, 29, 60, 134). And they would be right, he stresses, if they were speaking of states with premodern foundations for virtue, such as Aristotlean logocentric theory, or daoism in China, or purity in Hindu India anywhere "fixed functions based on an overriding principle . . . are the primary source of identity" (p. 175). But the modern republican tradition is different. It "was capable of bracketing off logos and developing practical reason from which a distinct view of political liberty and individual political efficacy could emerge" (p. 175). It attempted to distill "explicitly civic virtues" out of the moral or ethical cardinal virtues elaborated by classical writers (pp. 3, 17). A "fundamental rupture" from an "inflexible logos" was able to "open up the possibilities for a this-worldly civic liberty." Modern republicanism replaces an "extravagant faith in spiritual leaders" with "a separate realm of civic virtue which centres on the notion of a citizen who is a potentially active

participant in politics" (pp. 166-7). Modern republicans reject "imposing a particular form upon society," one based on a notion of human fulfillment. They see human nature as plastic, as "created in the political process." The modern republican telos is therefore cast "in terms of an absence—nondomination—rather than in terms of fulfillment" (p. 15).

The achievement of this new republicanism began, Brugger argues, with Machiavelli, who changed the meaning of virtue (p. 116), and it now finds its completion in Rawlsian and postmodern pragmatism. Machiavelli's new virtue "owed nothing to any divine order and consisted simply in a willingness to do whatever was necessary for civic greatness and, if that involved qualities more appropriate to the beast than the man, so be it" (p. 33). Virtue was no longer self-abnegating but instead compatible with individual interests. Machiavelli also encouraged us to view religion as a mere means in the promotion of civic order and thus to be unconcerned with its truth (pp. 36, 51, 71, 188 n. 2). The consequent destruction of religion secured the "autonomy of," the political." After Machiavelli, republican thinkers moved still farther from "a focus on a divine cosmos" and produced a healthy weakening of natural law or "universality of values?" (p. 16). Justice as "political and not metaphysical" is what "four centuries of republicans... have been groping for" (pp. 10-1).

Brugger builds the case for this understanding of an evolving pragmatic republicanism by constructing "four variations on the ideal-type of republicanism—early modern (Machiavelli, Sidney, Milton); Enlightenment (Montesquieu, Rousseau, Kant), mechanistic or American (Madison, Jefferson) and contemporary (Pettit, Sunstein, Barber)." Each is accorded a chapter in which Brugger presents the variation's respective view of popular sovereignty, its understanding of history (cyclical or progressive), the place it accords to virtue and law; and its conception of liberty. In a fifth chapter, he examines the (dim) prospects of republicanism in the developing world.

The republicanism that emerges from this narrative "affirms popular sovereignty, is sensitive to [structural or systemic] corruptibility, asserts the value of civic virtue and upholds a view of liberty as non-domination" (p. 181). It satisfies liberal neutrality by "having the state acknowledge only the ecumenical or non-sectarian good represented by the freedom of its citizens" (p. 9). Yet, it is republican and not liberal in that it promotes not merely noninterference but nondomination. It entails vigorously defending the right to "define our own private sphere." In short, modern republicanism lies somewhere between the autonomous self of liberalism and the collectivist or "holistic" ontology of communitarians (pp. 139–42).

Brugger's is a rich and subtle account of modern republican political theory and of its enduring relevance, but the book is not without difficulties. Being derivative and eclectic, it is obese with names and shorthand summaries of argument. Readers must wade through passages like the following:

It would be interesting to speculate on the virtual virtue of virtual republicanism, noted by Ronald Grimsley (1961, 77) in Rousseau, or what Kant's enlightened philosophers, which, in Gramscian terms, were to be "organic" to his prescribed state, would have done with it. Kant, after all, was noted for what Gareth Stedman Jones (1994, 171) described as the "pedagogic state" backed up by philosophers (p. 72).

More substantively, Brugger's account of a progressive and liberating battle toward a postmodern pragmatic republicanism is not convincing. He assumes, although he does not spell out or begin to defend, the fashionable claim that "logocentric" reflections on justice in classical political thought were born out of a fear in the face of flux. But as he is compelled to imply in his survey of nonliberal regimes, the view of justice as categorically or rigidly demanding self-sacrifice is not limited to the "logocentric" pre-modern West. It belongs rather to political life as such. Even early modern republicans saw the need to make substantive arguments about "natural rights"; Brugger too easily dismisses their arguments as "rhetorical" rather than "deontological" (p. 43). And while freedom for Kant may have been "freedom from domination" (p. 78), in The Metaphysics of Morals Kant spells out very strict universal moral imperatives that are to free us from our baser selves, imperatives that Brugger, if he had not ignored them, would have to call "domination."

Above all, the "fixed" demand of a universal and categorical justice—the demand that we serve a common good even against our interest—is felt by Brugger himself. He makes it a part of his republicanism. He asks us to accept the "belief" that "politics has the potential to generate disinterested deliberation in the search of a common good," a belief that will itself "further that common good" (pp. 130, 133). But his account of a modern republicanism leaves no room for devotion to such a common good. Machiavelli made virtue compatible with self-interest by rejecting justice understood as a high, common good requiring sacrifices of us (pp. 22, 69-70, 106 with 34, 40, 68, 103, 168, 170). Virtue must no longer mean choosing correctly but "grasping" against fortune; it must mean being out for oneself and by oneself, through leonine force or foxy fraud, in a hostile world (pp. 34). But if this is what it means, then one who is devoted to a common good is merely a sucker. Brugger never explains how virtue can be both self-interested, even fraudulent, and yet entail a devotion to the common good. But he is not bothered by contradictions. Unlike the modern political philosophers that he has read, he fails to see why we cannot have contradictory things (pp. 181–2).

Serious political consequences can arise from failures of this sort. Brugger's republicanism of nondomination might appear to be little more than a radicalized version of liberalism. But the following clear and honest statement about "civility," the one virtue that the new republicanism truly requires, shows that it will not secure the freedom it promises: "Civility is not just a matter of denying the personal self. It is also a matter of letting other identities take over in your person" (p. 134). How letting this happen constitutes nondomination is a great mystery. In fact, it seems to be the very essence of domination, especially when we consider that "certain "normative measures are needed" to foster the desired "flexible identity" or "multiple patterns of identification" (p. 134). These normative measures would require in turn various "constraints on democracy," both within existing institutions and through the creation of new ones (pp. 144-8,

These constraints are chilling to liberals. They make it hard to avoid the impression that Brugger's republicanism, in attempting to do away with the quiet domination of the neutral liberal state, would bring about a more complete, official domination of individuals. Whether the postmodern pragmatist misology that lies behind this prospect grows inevitably out of modern political philosophy is, however, a serious question, and it is to be hoped that Brugger will bring his considerable talents to bear on it in future work.

Rousseau, Nature, and the Problem of the Good Life. By Laurence D. Cooper. University Park: Pennsylvania State University Press, 1999. 264p. \$45.00 cloth, \$18.95 paper.

(Un)Manly Citizens: Jean-Jacques Rousseau's and Germaine de Staël's Subversive Women. By Lori Jo Marso. Baltimore, MD: Johns Hopkins University Press, 1999. 192p. \$38.00.

Instinct and Intimacy: Political Philosophy and Autobiography in Rousseau. By Margaret Ogrodnick. Toronto: University of Toronto Press, 1999. 238p. \$50.00.

Rousseau's Republican Romance. By Elizabeth Rose Wingrove. Princeton, NJ: Princeton University Press, 2000. 272p. \$60.00 cloth, \$19.95 paper.

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Lori Jo Marso combines a discussion of parts of Rousseau's works on political theory and literature with a study of Stael's novels, Delphine and Corinne ou L'Italie, in order to point to an "alternative vision of citizenship" (p. viii). This alternative is urgently required because our politics has at its core the ideal of the "manly," or alternatively "rational," citizen, whose very conception of the common good or general will is constructed through the systematic "philosophical and political exclusion of women" (p. 2). This ideal, Marso argues, has its roots in "the 'primordial error' of Western philosophy," that of "ontologiz[ing] universals and reduc[ing] particulars to mere exemplars" (p. 75). Marso turns to the portrayal and celebration of "subversive women" in Stael and Rousseau for the purpose of examining their "dissatisfaction with a society that is above politics," that is, with a politics that suppresses difference, contradictory goods, and particularity, a politics that is unable to embrace loyalties to friends, family, and loved ones as well as loyalty to the community (p. 75). Whereas all Rousseau's male characters represent attempts to construct "manly citizens" who necessarily take "the reason and sentiment of one man . . . [as the] general will of all," and who as such "inspire nothing but contempt," these women both represent and champion the "excluded and marginal" (pp. 41, 50, 56-7, 69, 72, 74).

Marso's book constitutes a refreshing departure from much of feminist scholarship on Rousseau by considering in detail what the author describes as "Rousseau's sympathies with his women characters and the delicacy and strength he puts forth to portray them" (p. 6). She argues that through his female characters, Rousseau reveals his own "ambivalence about constructing gender differences in terms of the male model." The "feminine presence" in his works both "acknowledges radical difference" and points to a politics "unable to be consumed by the male imagination" (p. 7). Inspired by this alternative perspective in Rousseau, Stael portrays the heroines of her novels as defying the "constraints of women's presumed identity in denying the framing of a choice between public and private life" (p. 126). Staël further shows us that masculinity is deficient in creating "moral citizens," because the "manly" citizen's reason cannot comprehend "passion, engagement, and interdependence (with certain identifiable others) as integral to moral political action" (p. 106).

The "alternative model for citizenship" that arises from a consideration of Rousseau's and Staël's women presents "love for family and intimate friends [as] a model for the care, respect, and justice desired by all" (p. 137). The alternative model leads us to consider those "situated on the margins" because it automatically "integrates individual passions and goods with the larger good of the whole" (p. 105). Rousseau's careful explorations of the possibilities, difficulties, and limits of justice and of love, however, repeatedly

require us to confront the realities of power and clashing wills, consideration of which realities is notably missing from Marso's book. It is likely that care, justice, and respect are desired by all, but in Rousseau's view that desire does not guarantee its own satisfaction in the face of "the incommensurability of human goods" (p. 106). Hence, for him, our need and desire that women, as well as men, move from self-love, through love for family, to care and just regard for all constitute a problem, not an answer.

Because the following claims are pivotal to Marso's argument, it would have been helpful to clarify, for example, how "basic tenets of Enlightenment thought" give rise to the model of "'manly' citizen"; how "mysogyny and xenophobia" necessarily have their "deep roots" in "philosophy"; in what way Rousseau's politics is a "communitarian dream of the transparent and unified whole"; or how his texts represent the "triumphal moments of Enlightenment reason" (pp. 129, 131, 136, 132). These claims may ultimately be true, but their bare statement without significant elaboration suggests self-evidence, which is surely contestable.

Wingrove's confident, complex, and provocative book takes up the central Rousseaun questions of power and will. Using narrative analysis of Rousseau's philosophical as well as literary works, Wingrove seeks to flesh out "the political work that gender does for republican theory" (p. 14). Understanding how gender and politics are mutually constitutive is crucial, in her view, to understanding how Rousseau reworks, but does not resolve, the "dilemma of conventional authority," a dilemma articulated by the famous paradox in the Social Contract that citizens are "forced to be free" (pp. 243-4). The problem of how to harmonize obedience and liberty is generally understood to be resolved by him through the doctrine of submission to the general will, by which each citizen sovereignly commands the law that in turn subjects him or her. Wingrove, however, seeks the tangible forces that invigorate this abstract form. She argues that Rousseau points to erotic desire as "the passion to be reckoned on" (p. 244). If we look to Rousseau's account of sexual relations, and of the mutual constitution of political and sexual orders, the heart of "consent" will be revealed as a "consensual nonconsensuality, meaning the condition in which one wills the circumstances of one's own domination" (p. 5). Consensual nonconsensuality describes an interplay of coercion and cession, force and will, which is the fabric of relations between bodily beings who find that the satisfaction of desire necessarily entails both domination and submission. This interplay of power and will, she argues, is the stuff of romance, and romance the stuff of democratic self-rule.

Wingrove argues, contrary to Rousseau's firm statement that "man" and "citizen" are contradictory ideals, that these are simultaneously formed through induction into a heterosexual and political order. The very possibilities of "authenticity" and "identity" are produced through physical or bodily conditions that are always "organized" politically: Self-consclousness arises with, and is therefore constituted through, hierarchies of ruler and ruled, strong and weak, desired and desiring, all which hierarchies emerge from the ways we first experience ourselves and others as masculine and feminine objects. Heterosexuality itself, however, and the binaries it produces masculinity and femininity, power and submission, sensual passion and reason—are all "a function of Rousseau's politics" (p. 51). Since the "possibilities of being and seeming to be emerge coincidentally," no "unmediated identities" are possible: We are the creations of relational amour-propre. As such, juridical, political, familial, and sexual practices above all condition the "factual and fictional positioning" that structure "identity" (p. 41). Hence "submission and domination are the natural concomitants of human *perfectibilité*, inasmuch as an awareness of will, world, and self entails the recognition, which is the experience, of coercion and consent" (pp. 36–7).

It is this awareness and recognition, produced through rhetorical and "performative" civic and sexual practices, that animate the "imaginative dimension of political consciousness" (p. 216). Wingrove develops this account by detailing how the core of Rousseau's democratic politics is the gendered spectacles in which citizens celebrate nation, family, and love as the images animating and constituting the "pleasures of their mutual bondage" (p. 204). She is led to conclude, therefore, that "autonomy" or "political and moral agency," in both men and women, is "possible only in the context of, through the enabling and constraining structures" of, a "social system organized by sexuality" and "a political order that is always, in the final instance, coercive" (pp. 77, 230).

Wingrove's confidence in the nullity of Rousseau's claim that man and citizen are distinct ideals seems, in part, to arise from the presuppositions, derived from her reading of Emile, that a sense of self only arises with sexuality, and that the "natural man's" "dominant passion" is to "live in a world...peopled by catizens" (p. 197). Her claim that amour-propre, will, and power, necessarily "mediated" by our encounters with the world, are the constitutive dynamics of the Rousseauian self is more than plausible. Still, their necessary identification with sexuality seems less so, since these dynamics are described as fully present prior to puberty. Although sexuality potently serves to give them further form, it is not required to set them in motion. This suggests the necessity of a reconsideration of "asexual humanity," or self-love (p. 70). Furthermore, it is not too much to say that Rousseau would not be worth reading if he were not keenly alive to the subtler varieties of force and fraud in the world and in democratic politics; nevertheless, unlike Wingrove, the issue for him does not seem to be simply how political "possibilities are imagined and practiced" (p. 152). These practices, in his view, have real consequences that we cannot help but judge in light of a concern for well-being. For Rousseau, the question of nature remains a question, one that Wingrove's interpretation may not sidestep as easily as her "antifoundationalist commitment" might suggest (p. 9).

Cooper wades right into the middle of this most complex and problematic of questions in Rousseau's thought, the difficulty of which is reflected in the disagreement within Rousseau scholarship over its character and status. Cooper's ambitious and impassioned book, which takes up the cause of the "sublime" in Rousseau, examines "nature" in light of the character of morality or conscience and happiness. In Cooper's account, Rousseau, sharing as he does "the general principles of modern science," deepens modernity by developing its reflections upon nature necessarily understood in terms of origins (pp. 186, 12, 39, 70, 73). He "rejected the naturalness of any kind of moral or psychological dualism" (p. 117). In light of this understanding of nature, health is understood as the actualization of a "formal" standard of nature, the condition of "psychic unity and balance" that characterizes the human animal in the state of nature (pp. 183, 180). Significantly, however, Cooper disagrees with the view that Rousseau found "no place for the sublime in the modern scientific explanation of man," and he goes on to argue that Rousseau "did not need to postulate the derivation of the sublime from strictly nonsublime sources" (p. 82). A "substantive" standard of natural fulfillment, a "post-stateof-nature-naturalness," is manifested by ends embedded in our nature, which in turn are revealed to us through the

conscience, that is, through an "innate" love of the "order" that is expressed by "the harmony of the cosmos" or "creation" (pp. 11-3, 92, 59).

Cooper states that "Rousseau never calls conscience a principle of soul." (Rousseau means by such a "principle" a fundamental inclination "anterior to reason" from which all other passions are derived: In the Second Discourse, he identifies these as "self-love" and "compassion.") According to Cooper, it "suited Rousseau not to bother identifying principles of soul in Emile." Nevertheless, if he had "used the Second Discourse's scientific methodology on Emile, he would have identified the love of order—that is, conscience—as a principle of the civilized natural man's soul" (p. 94). Conscience is the "one thing that is sublime from the start" and "is natural in the strongest possible sense: it is an innate principle in the soul whose awakening from latency is automatic and irreversible in civilized man" (pp. 82, 11). It includes "aesthetic," "philosophical," and "scientific sensibilities," as well as constitutes "the innermost meaning of goodness, virtue, and justice" (p. 95). Upon this basis, Cooper engages in detailed discussions of Rousseau's texts in order to fashion an account of the development of civilized naturalness through the "developmental template" of conscience, a development in which the formal and substantive natural standards in Rousseau's thought are made to coincide (pp. 11, 100).

This development confronts, however, the central problem of amour-propre: Although it necessarily transforms the "fundamental needs" experienced by civilized human beings, amour-propre always threatens to destroy any harmonious development of self-love guided by the conscience. Therefore, in what is perhaps the best part of the book, Cooper justifiably seeks to correct prevailing accounts of this fundamental Rousseauian concept by putting forth an interpretation of the characteristics of amour-propre and self-love. His interpretation is based in part on the postulate that, since Rousseau describes himself as not being "dominated" by amour-propre, Rousseau's characteristics necessarily serve as a template of self-love and therefore cannot be ascribed, as they sometimes are, to amour-propre (pp. 139-43). This interpretation leads to an account of how to govern the extent and character of amour-propre in order to realize the end of "civilized naturalness."

Cooper concludes that Rousseau's "dual-level conception [of nature] allows him to reestablish the naturalness of the sublime and thereby redeem human dignity from the clutches of modern scientific naturalism" (p. 186). His book ends with a critique of Rousseau's attempt to construct "an understanding of the good life" upon "nonteleological" foundations, suggesting that Rousseau is dangerously mistaken in teaching us that amour-propre is unnatural, for that teaching undermines "longing and aspiration" (pp. 15, 207).

Drawing upon Rousseau's philosophical and autobiographical works, Ogrodnick engages in a psychoanalytical analysis of Rousseau's account of the modern self as the "primordial base" of his political and philosophical thought (p. 17). "Conceiving philosophy as in part an unconscious memoir," Ogrodnick attempts to dig down to the "psychocultural" roots of the modern self as it is expressed by, and through, Rousseau (p. 46). She therefore analyzes Rousseau's autobiographical works as the most immediate expressions, and the philosophical works as reflections, of Rousseau's unconscious. The unity of his thought arises from the "primordial vision" of Vincennes, in which the "archaic residue embedded in our deepest psyche," erupted to reveal to him "humanity's primal beginnings" (pp. 36-7). This vision is filtered through a pattern of "psychocultural dichotomies" that are not idiosyncratic to him but are reflective of the "modern Western epoch" (p. 15). These unconscious dichotomies are threefold: masculine versus feminine; desires for absolute self-sufficiency versus total union; and good and evil. By teasing out the workings of these manifestations of the unconscious through a "phenomenological" investigation of the "parallel personal and theoretical engagement[s]" in Rousseau's texts, Ogrodnick seeks not only to clarify and revitalize Rousseau's vision but also to validate the psychoanalytical approach she employs (p. 42). Although mindful of its difficulties, she claims that its "credibility" depends upon its ability "to resolve textual complexities and interpretative controversies . . . [that] it would have been difficult to see and configure" without it (p. 44).

In a detailed analysis, Ogrodnick argues that Rousseau's project was to retrieve "instinct" as the "emotional foundation" of a "politically intimate polity," thereby challenging accepted distinctions between the private and public realms (p. 19). Beginning as he does with the radical "atomism of liberalism," Rousseau must reconcile the conflict between duty and interest concomitant with citizenship by "deriv[ing] civic attachment from the development of private sentiment (pp. 187, 172). This reconciliation is accomplished through the "catalytic effect" of intimate familial and romantic ties. These, "extending the self" by channeling "instinctual compassion and self-love" into "civic friendship," cause a "fusion between self-care and other-care that turns harm to a fellow citizen into a self-infliction" (pp. 172, 160, 171, 138). Rousseau thereby accommodates "both the autonomy of the rights-bearing individual and the attachment of the communitarian self" (p. 163). Ogrodnick's account correctly identifles such a project as central to Rousseau's prescriptions for citizenship, but it encounters a difficulty similar to that encountered by Marso's account. Rousseau himself, as Ogrodnick notes (e.g., pp. 160, 187), shows repeatedly that links between civic virtue and individual flourishing are difficult to forge. Much the more salient portion of a physical being's "self" is its physical existence, which is the condition of all goods and cannot be shared. The relationship of an individual to the others who comprise "community" can never be understood as simply "constitutive" for this reason. In light of this difficulty, the absence in Ogrodnick's work of a treatment of the fundamental role of religion and of the limits and difficulties of compassion in Rousseau's thought prove to be problematic.

Ogrodnick may not fully succeed in demonstrating decisively her claim to resolve contradictions in Rousseau's political and philosophic thought that cannot otherwise be resolved than through the kind of psychoanalytic approach in which she engages. She does, however, perform an important service and raises many useful questions in highlighting a problem that Rousseau so forcefully and artfully asks us to confront, namely, the basis and motives of philosophy. It is intriguing to wonder what results Ogrodnick would have arrived at if she had attempted to elaborate Rousseau's thought on this matter not only in terms of the psychology of Jung, Merleau-Ponty, or Benjamin but also in terms of Rousseau's own psychological account.

Fugitive Theory: Political Theory, the Southern Agrarians, and America. By Christopher M. Duncan. Lanham, MD: Lexington Books, 1999. 244p. \$65.00.

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The American South is unique. As Christopher Duncan notes in this provocative volume, only the South inspires true love or true hate (p. 4). He addresses the ideas of one set of ardent defenders of the South, the agrarians, who arose in the wake of the Scopes trial and sought to defend what they saw as a unique southern culture against the forces of materialism and centralized capitalism.

In examining the work of Allan Tate, Donald Davidson, John Crowe Ransom, Robert Penn Warren, and others who contributed to the classic statement of the southern agrarian position, I'll Take My Stand (1930), Duncan argues that they provide a useful alternative vision of American political and social life. They spoke (and for Duncan continue to speak) against the dominant paradigms of possessive individualism and liberalism that control our cultural universe and our intellectual horizons as Americans. But they were not solely negative. The agrarian idea seeks to avoid the effects of huxury or ease through hard work (p. 13). In Duncan's view the agrarians fought for a vision of civic republicanism informed by the ideal of "true republican independence" and "social bond individualism" based on land ownership (p. 141). For them, the agricultural South most closely approximated the conditions necessary for virtuous, uncorrupted life and therefore for effective political and social participation. They saw themselves as defending a unique and valuable form of life, a true community, against the atomizing forces of modernity. Understanding the southern agrarians as communitarians before their time, Duncan does not seek to obviate the problems raised by their thought. But he asks us to take them seriously as thinkers and as prescient observers of the effects of modern economic and social change. In presenting the South as a community, Duncan asks the reader to drop any academic communitarian notions about the meaning of the term (pp. 2-3). Communities are not mere "lifestyle enclaves" (p. 5). Instead, he asks us to consider the agrarians' idea of community as a political and social organism in the Aristotelian and classical republican

This volume is really two books in one. The first part (introduction, chaps. 1–3) is a discussion of the history of property rights and their relationship to civic republicanism. In this section Duncan attempts to create a linkage between classical (Aristotelian) and early modern (Harringtonian) visions of the linkage between property and citizenship and the vision of the agrarians. Unfortunately, the agrarians disappear from large sections of the argument, which left me with the feeling that Duncan forces the connection, relying too much upon analogy and assertion and too little upon textual evidence from the agrarians themselves.

In the second part (chaps. 4-6), Duncan takes on the central question for the agrarians, the survival of a specifically southern form of community in the face of industrialization, individualism, and advanced capitalism. He traces the forces that led to the publication of works such as *I'll Take* My Stand. This is the more effective part of the book. In these chapters the tensions that underlie the agrarians' worldview are clearly delineated. Questions of progress, industrialism, and race all receive a hearing. He asks us to understand the agrarians not as romantics nostalgic for the ante-bellum past but as rebels against the future, seeking a means to escape a seemingly preordained world of industrialism and centralized government. Duncan suggests the agrarians sought a choice between "triumphalism," or the mindless celebration of progress, and "fatalism," or the passive acceptance of the tendencies of the age and our place in it (p. 221).

This book is old-fashioned but in a good way. It sets out a forthright position. As such, and like the southern agrarians he defends, Duncan opens himself to strong criticism. His argument rests on a dangerous assumption. He seems to

believe there is (or was) a specifically southern way of life and culture that stands in opposition to challenges and demands from the rest of the country. This leads him to accept uncritically the agrarians' vision of the South and, by extension, the North. He notes that in effect this radically limits our political and social possibilities. "Once again the choices unfortunately seem to be dichotomous ones: virtue or luxury, redemption or corruption, life or death" (p. 23). Any political theory that relies on such stark choices can be suspected, with good reason, of setting up strawmen. This can be seen in the tendency of the agrarians to blame the North for the South's problems (p. 210). The North becomes the evil oppressor and the South the noble victim. As propaganda this is excellent, but as political theory it is rather weak.

A second problem is Duncan's acceptance of both moral and practical claims for the superiority of farming and a land ethic. (The southern view of the moral superiority of farming over other forms of endeavor explains why William Faulkner always listed his occupation as farmer.) But does the fact that the agrarian idea was most often espoused by academics and not by Jeffersonian yeomen suggest a disconnection with reality that undermines the main premise of agrarianism? Duncan addresses this point but in a manner that begs the question. Throughout the book, and especially in the conclusion (pp. 207-8), he compares the agrarian program as a theoretical model with the reality of industrial civilization. This is unfair, as he compares the ideal with the real. Indeed, we might ask whether building a coherent theory on which to base practice from agrarian thought is possible. As Paul Conklin notes (The Southern Agrarians, 1988), the agrarians disagreed among themselves on the proper political organization of society.

Finally, Duncan also overstates the "daring" of the agrarians in calling for a unique southern tradition and for demanding northern respect for the South. He ignores the fact that by the 1920s and 1930s the southern interpretation of the Civil War and Reconstruction had become practically the orthodox view, both in academic circles and popular culture. One need only point to such films as "Birth of a Nation" and "Gone with the Wind" as examples of the triumph of revisionism. Or consider that the use of the term "the Great Rebellion" for the war had been replaced in the North by "Civil War" or "War between the States." By the 1920s the North was quite willing to leave the South alone. Indeed, the South was seeking to become like the North through industrial development. Duncan notes this, but because he accepts the agrarians' model of a virtuous South and corrupt North, he misses an opportunity for a more complex analysis.

No discussion of the agrarians can be complete without examining their positions on race. Duncan seeks to give them the benefit of the doubt. He claims that they were segregationists but not white supremacists (pp. 210-1). Some might say this is distinction without a difference, but Duncan strongly defends its meaningfulness within their historical context. His analysis will not convince anyone who feels that that agrarians were irredeemable products of a racist culture, but it does give a sympathetic reader a means to get past race to address the more contemporarily useful parts of their argument. There is a danger in doing this, however. The danger is not that we will absolve these writers of any complicity in segregation and racism but that we will unconsciously allow them to define southern values in purely white terms. In many ways blacks are tangential to the southern agrarian project. We risk losing sight of this if we accept the agrarians solely on their own terms. We also risk losing some of the richness and tragic complexity of issues of race in the South and in the United States.

This attempt to retrieve the ideas of the southern agrarians faces a final challenge. Duncan suggests that by bringing their demands forward they faced two risks: cooptation to the northern view of industrialism and unrestrained individualism and destruction in the face of the uniformity and sameness imposed by modernity (p. 155). There is a third possibility, however, and it endangers the whole project of the agrarians and Duncan's efforts at reconstruction; this is the danger of being ignored by a triumphant opponent. Duncan's book represents an effort to avoid that third danger, to inject into our modern debates over community, globalization, and diversity the vision brought forward by the southern agrarians. As one might surmise from this review, I do not think Duncan fully succeeds in that effort, but it is surely worth making.

Anyone looking for a general introduction to the ideas and theory of the southern agrarians will be disappointed (and probably confused). But if you want to plunge headlong into a lively and idiosyncratic debate about republican virtue and sectionalism, combined with a vigorous defense of untimely men and a "road not taken," this book will provide plenty to think about and plenty to engage the emotions.

Law as Politics: Carl Schmitt's Critique of Liberalism.
Edited by David Dyzenhaus. Durham, NC: Duke University Press, 1998. 318p. \$54.95 cloth, \$18.95 paper.

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This is an excellent collection of articles, both from German and Anglo-American commentators, on one of the most controversial and influential critics of liberalism. Focusing largely on Schmitt's critique of liberal politics and his politicization of constitutional law, the essays tend to rotate around two interrelated questions. First, should we defend the reasonableness of liberal constitutionalism against Schmitt's well-known strategy of seeking to find beneath liberal institutions the primordial "political" moment when an individual or a people willfully and without rational guidance decide to impose a unified political and constitutional order? Second, can Schmitt's existential concept of the political as the definition of the self through the definition of the enemy be used to justify political forms he would never have embraced, that is, to defend constitutional liberalism, radical democracy, or natural right? An answer to the first question (according to Ronald Beiner and David Dyzenhaus) is the explicit aim of this collection, but the provocation of these essays is in the variety of answers they give to the second question.

Quite a number of the contributors seek to explicate the relation of Schmitt's concept of the political to his critique of the liberal concept of constitutional law, especially as developed in his most significant work on public law, Verfassungslehre (1928). Ernst-Wolfgang Böckenforde's essay, which is central to the German discussion, seeks to demonstrate that Schmitt's concept of the political as the struggle of friend and enemy was meant to apply to relations outside the state. Within the state the political referred to political unity beneath which constitutionally regulated competition could occur. The essays of Renato Christi, Ingeborg Maus, and John McCormick cast doubt on this thesis. Christi demonstrates that in Verfassungslehre Schmitt adds the notion of "the people" (das Volk) as the constituent power that produces constitutions to his resolutely monarchical (and antidemocratic) concept of sovereignty. By contrast, Maus argues that despite Schmitt's decisionistic notion of popular sovereignty in his account of the Weimar constitution, "the social function" of Schmitt's theory is always to privilege the supercession of general (constitutional) law by decisionistic measures of a dictator in the state of emergency, a possibility which is always-already present in every constitutional form because all legal norms originate in the state of exception (p. 206).

McCormick provides a somewhat more nuanced account located between Maus's socially functional argument and Christi's democratic reading of Schmitt. Carefully following the concept of dictatorship from Schmitt's early work to his later Weimar writings, McCormick rather convincingly demonstrates that Schmitt abandoned his earlier endorsement of limited dictatorship in emergency situations in favor of an absolute notion of sovereignty in order to counter revolutionary socialist and communist demands for the dictatorship of the proletariat. Thus, for McCormick, Schmitt embraces popular authority as a constituent power largely as a tactical move to counter revolutionary socialism.

Space does not permit a further critical discussion of the careful scholarship behind each of these interpretations. But it is worth pointing out that a central issue at the heart of this debate is the second question, that is, whether Schmitt's inclusion under the exceptional situation of a radical democratic popular will should be regarded as a mere tactical maneuver in support of a plebiscitarian executive power or an occasion for inserting popular sovereignty into the political struggle over liberalism.

This question is indirectly taken up in a number of essays that discuss Schmitt's assault on the political institutions of modern liberalism. Ellen Kennedy points out that Schmitt's concept of "public" as the unified "people" steers between the rational liberal public and the irrational public of crowd psychology. Robert Howse argues that Leo Strauss accepted a limited version of Schmitt's decisionism in the emergency situation, but by echoing Schmitt's criticism of Weimar he also mobilized classical natural right to defend a liberalism that had abandoned the grounds for its own defense. Rheinhard Mehring claims Schmitt vastly overstates his argument that the principle of liberal parliamentarism must inevitably collapse under the weight of its tendency to compromise contradictory political positions.

Other contributors seek to defend constitutionally limited government against Schmitt's notion of the supremacy of the political decision over any constitutional system of rights. In a deftly argued essay, Heiner Bielefeldt argues that liberal neutrality toward differing world views "can quite well go along with the willingness and readiness to engage in political conflict. Liberalism, especially Kantian liberalism, is after all a fighting political ideology" (p. 31). It struggles, he claims, against oppression and discrimination in all its forms and seeks to discover biases behind universal claims. This argument meets Schmitt's challenge only half way. It does not discuss the pressures to which a radical republican form of constitutional liberalism would be subjected once forced to engage in a partisan political struggle against those who oppose the elimination of barriers to human dignity.

Mehring and Dominique Leydet simply bypass this issue. Mehring claims that Schmitt overlooks the possibility of a "constitutional patriotism" that encourages peaceful compromises in a parliamentary setting. Leydet acknowledges the force of Schmitt's criticism that liberal parliaments tend to be taken over by private interests and disciplined parties, and therefore fail to live up to the liberal principle of rational public debate in which representatives overcome their party loyalties and converge on the proposal that is supported by

the best argument. We can answer Schmitt's political-sociological critique, Leydet insists, if we simply abandon the ideal of disinterested parliamentary discussion and instead rely on the public to screen policy proposals and hold representatives accountable.

The problem with both Mehring and Leydet's responses to Schmitt is that they cede him his most trenchant political-sociological criticisms while proposing rather pale institutional alternatives to offset them. A more vigorous challenge to Schmitt comes from McCormick, who points out that Schmitt quite rightly shows that liberal constitutions have neglected the problem of who should have sovereignty in a state of emergency and that his earlier Roman model is a potential solution to the problem; but he also argues that Schmitt is wrong that the exceptional moment is at the origin of all sovereignty. The institution of emergency powers is merely a constitutional mechanism.

William Scheuerman and Jeffrey Seitzer choose a different tactic. Scheuerman meticulously demonstrates how Schmitt, and Arendt for opposing reasons, quite deliberately distorts the political doctrines of the French and American revolutions to support the view that politics is prior to and supersedes all liberal constitutional rights of liberty. Scheuerman scores an important point against Schmitt for attributing to Sieyés a purely decisionist view of the people as constitutive power while overlooking this thinker's appeal to natural law and rights of individual liberty as a restraint on any popularly representative assembly. Similarly, Seitzer shows how Schmitt justifies a plebiscitary sovereign for the Weimar constitution by tendentiously reading the history of constitutionalism as a series of decisionist founding moments.

The question these last two readings raise, however, is whether merely showing Schmitt stylized and distorted constitutional history undermines the theoretical force of his existential theory of politics and sovereignty. Surely, neither Schmitt nor Arendt is the first theorist to distort history for the sake of projecting dynamic accounts of politics into static situations or, for that matter, to view politics from the viewpoint of polarized extremes. Thinkers as diverse as Machiavelli, Weber, Mannheim, and Gramsci have done the same for goals quite at odds with those of Schmitt.

Recognizing that Schmitt's theoretical style does not entail his conclusions, Chantal Mouffe places his concept of the political in the service of a concept of radical democracy that Schmitt would have resolutely rejected. Mouffe accepts his criticism of liberal cosmopolitanism and agrees that political equality in a democracy demands at once homogeneity of citizens as citizens and the distinction between citizens and noncitizens. Yet, she cunningly argues that Schmitt gets it wrong in claiming that the tension between democracy and liberal procedure must inevitably destroy any liberal constitution. Rather, she insists the tension between citizens demanding equal rights of various kinds and liberal universalism produces beneficial consequences for a democratic polity. The rights claims of citizens offset procedural uniformity, and procedural uniformity checks the tendencies of citizens to exclude groups within society (pp. 164-5). Thus, Schmitt can be deployed to discover a democratic dynamic between his two existential poles of dictatorship and pure friend-enemy conflicts.

By redirecting Schmitt's political existentialism, Mouffe avoids a recurrent difficulty that entraps some of the other contributors who seek to defend liberal constitutionalism against his critique. The difficulty lies in Schmitt's strategy of placing liberal parliamentary politics and constitutionally limited government between the existential extremes of absolute sovereignty, on the one hand, and heightened

partisan political conflict, on the other. Once Schmitt makes this move, critics either must deny the fairly obvious fact that even constitutionally limited government shares somewhere along the way in a will-centered decisionist notion of sovereignty and that this model is part of a partisan struggle against enemies of this political form; or they must alter their political position in light of the pressures this existential condition imposes on liberal constitutionalism. Only the essays of Mouffe, McCormick, and Bielefeldt directly confront this latter problem, in part by playing off one part of Schmitt against the other.

This weakness does not override the obvious strengths of this collection. All the contributers engage directly with the substance of Schmitt's criticism of liberal politics both in its Weimar setting and in its contemporary variants. Furthermore, they do a fine job of mediating between the German and the Anglo-American idioms of political and legal theory. All in all, this collection demonstrates why theorists who do not share Schmitt's authoritarian commitments have found in his dynamic critique of liberal politics and constitutional law a recurrent challenge to their own commitments—and perhaps more important, a provocation not to reduce politics to constitutional form.

Marxism and Social Science. Edited by Andrew Gamble, David Marsh, and Tony Tant. Champaign: University of Illinois Press, 1999. 381p. \$42.50 cloth, \$19.95 paper.

Engels after Marx. Edited by Manfred B. Steger and Terrell Carver. University Park: Pennsylvania State University Press, 1999. 294p. \$55.00 cloth, \$18.95 paper.

Robert A. Gorman, University of Tennessee

Those of us who thought Marxian social inquiry unceremoniously died alongside the Soviet Union and communist Eastern Europe between 1989 and 1991 may now want to reconsider. Neoliberal optimism no longer can ignore an intensifying global and domestic maldistribution of resources. Several European nations have elected socialists to begin what promises to be a long process of reclaiming politics from corporate boardrooms. Some intellectuals—with distinct linguistic, cultural, and political identities—have survived the commercialization of universities and are standing up for decency at home and abroad. Often they organize against sweatshops, in favor of living wages, or to support unionized workers. They are also thoughtfully reflecting on the future of socialism, reevaluating socialist thinkers, and injecting the Left back into civil society. A surprising number of wellreceived new books, penned by notable socialists as well as young professors, have recently resurrected critical scholarship. Engels after Marx and Marxism and Social Science are recent additions to this expanding list of noteworthy publications. These books indicate that Marxian scholarship has ripened into a complex, subtle, engaging project that mainstream democrats may find attractive. The mindless certainty that often soiled its reputation thankfully has fallen, with communism, into history's dustbin.

Engels after Marx and Marxism and Social Science are edited anthologies. The respective editors agree that Marxism is alive and meaningfully contributing to social inquiry. They also feel Marxists no longer must agree on a set of inevitable historical laws that preclude the give and take of democratic politics. Finally, they believe that Marx himself carelessly provided the fodder for totalitarians by writing phrases that later were easily misinterpreted or taken out of context. Beyond these shared convictions, however, lie salient differences. Andrew Gamble, David Marsh, and Tony Tant,

the editors of Marxism and Social Science, equate traditional Marxism with the materialist brand of Soviet-style totalitarianism that emphasized economism and determinism. Marxism died when the Soviet empire collapsed, they argue, and is now reborn in the critical research of nonmaterialists. They would thus exclude legions of materialist scholars, including several contributors to their book, from the Left. There are no objective Marxian standards, they imply, and anyone who disagrees is categorically wrong. Perhaps this is an odd, postmodern version of democratic centralism. Manfred B. Steger and Terrell Carver, on the contrary, feel Marxism today embodies materialists and nonmaterialists. The essays in Engels after Marx thus reexamine the greatest of all materialist Marxists, Friedrich Engels, from diverse leftist perspectives. Traditional Marxism erred, Steger and Carver infer, by not tolerating intellectual diversity. There is no mevitable linkage between materialism and totalitarianism. Marxists, moreover, do not have to be materialists.

Marxism and Social Science will likely have a wide audience. The essays in Part 1 consider Marxism's relationship to methods and other perspectives in social science (e.g., feminism, regulation theory, postmodernity, New Right Theory), and those in Part 2 assess the utility of Marxism for a broad span of substantive issues, such as the state, culture, nationalism, democracy, ecology, globalization, and postcommunism. Some, of course, are better than others. Glyn Daly artfully describes postmodernism and defends its nexus with Marxism. His postmodern critique of determinism, however, is more convincing than his Marxian synthesis. He tells us how postmodernists can be Marxists, but he does not, perhaps cannot, tell us why postmodernists should be Marxists. If, as Daly asserts, cultural discourse is the only source of meaning, and discourses change, then why should the popular belief in what Daly calls "universal freedom and equality" be morally compelling? Was fascism morally correct in Europe during World War II because it was popular? Tony Tant, in "Marxism as Social Science," fares somewhat better than Daly by denying the universality of scientific models, but nonetheless he chooses Marxism over others because it generates new and better ways to satisfy what he considers irreducible human needs. Jim Johnson and David P. Dolowitz concisely summarize Marx's incomplete theory of class. Weber's critique, and recent efforts to bridge the chasm. Colin Hay situates the famous Miliband-Poulantzas debate on the role of the state in a context that includes Marx's own ideas as well as the recent work of Fred Block, Theda Skocpol, and others. His work is enjoyable and informative. Stevi Jackson's treatment of feminism, Michael Kenny's work on regulation theory, and Chris Pierson's essay on the welfare state all admirably summarize pertinent literature for interested non-Marxists.

In contrast, Andrew Gamble's project of connecting Marxism and New Right Theory seems to trivialize what each side is saying; Neil Robinson's assertion that contemporary Marxists have generally neglected the evils of Soviet and Chinese communism seems silly, given the anticommunist literature; and both John Barry's foray into "Marxism and Ecology" and Simon Bromley's essay on globalization prove that not enough has yet been written to justify the efforts. Daryl Glaser's meager discourse on "Marxism and Democracy" does not even mention Michael Harrington, certainly America's preeminent democratic Marxist.

The strength of Marxism and Social Science is also a weakness. Its targeted audience, mainstream students and scholars eager to know how Marxism relates to contemporary academic debates, does not know the resonant history of Marxism's dialectical method. This involved a change in

meaning of dialectics from economic determinism to a method stressing reciprocity of ideas and matter, culture and economics. The cultural studies in *Marxism and Social Science* were portended by Marxian thinkers for years and demonstrate the interpretive, nondogmatic, totalizing qualities dialecticians prefer. Without this theoretical and historical context, which the editors have omitted, Marxism becomes quicksilver: always changing, stretching everywhere.

Engels after Marx moves us closer to this context by reexamining the putative founder of dialectical materialism, Friedrich Engels. Organized into two sections—one dealing with Engels's philosophy and theory, the other with practical politics and social science—the book will beckon those with an informed opinion about Engels's connections to both Marx and modern communism.

The best essays in *Engels after Marx* are provocative and educational. The worst are very bad. Among the latter are several that examine whose authorial voice wins in the Marx-Engels partnership. Turgid, self-indulgent, and possibly vacuous, these narratives aroused repressed memories of my own youthful trials with a journal called *Telos*.

In contrast, Peter T. Manicas's "Engels's Philosophy of Science" refreshingly whitewashes the standard critique of Engels's Hegelian metaphysics and its inherent totalitarianism. In "Engels and the Enlightenment Reading of Marx," Scott Meikle argues that Marx's Aristotelianism, his belief that critical science must serve an ethical purpose, was fundamentally different from Engels's Enlightenment scientism. Engels eventually succeeded in tying Marx to the very bourgeois concerns and commitments he wanted to end. Manicas and Meikle are very convincing. Their message is reiterated by Tom Rockmore, who employs a vaguely postmodernist tack, and Paul Thomas, who focuses on methods. But Marxism today is unpredictable and heterogeneous. S. H. Rigby's "Engels after Marx History" documents how both Marx and Engels at different stages of their lives articulated humanistic, dialectical, and metaphysical epistemologies and may have believed all three simultaneously. So if Engels is dangerous and mistaken, then Marx is as well. Western Marxists usually attribute Marx's philosophical meanders to naivete and Engels's scientism to a lifelong passion. Rigby's research goads us to think again. Douglas Kellner may have taken this strategy over the edge. His "Engels, Modernity, and Classical Social Theory" contends that the early Engels theorized the distinctive features of the modern world and inspired Marx's critical theory of capitalism. The pupil, for Kellner, actually taught and inspired the master.

These Engelsian conundrums are left behind by Manfred B. Steger and Lawrence Wilde, who are more interested in how Engels was influenced by German politics after 1883. Steger's "Friedrich Engels and the Origins of German Revisionism: Another Look" argues that Engels realistically revised his views on violence, electoral politics, and nationalism (the sole theme of Michael Forman's essay) as a temporary revolutionary tactic. Paradoxically, he inspired Eduard Bernstein to reformulate Marxian theory to better suit the new reformism. Engels, says Steger, was actually the father of Marxian revisionism. Wilde, in "Engels and the Contradictions of Revolutionary Strategy," feels that Engels's tactics created tensions that mangled the international socialist movement. The book concludes with two non sequiturs: Carol Gould's feminist take on Engels's The Origin of the Family, Private Property, and the State, and James Farr's "Engels, Dewey, and the Reception of Marxism in America," which explores the absorbing relationship between John Dewey and Sidney Hook as they both seek to understand Marx, Engels, and the meaning of scientific socialism. Dewey,

Hook notwithstanding, accepted only Engels's Marxism and, through Dewey's influence, so did the United States.

Engles after Marx reflectively revisits the past. Marxism and Social Science modestly interprets everyday life. They help dispatch Marxism into the twenty-first century purged of hubris, filled with hope, and committed to democracy, diversity, and dialectics.

Freedom of Association. Edited by Amy Gutmann. Princeton, NJ: Princeton University Press, 1998. 382p. \$77.50 cloth, \$20.95 paper.

William A. Galston, University of Maryland

The morning I began writing this review, the Supreme Court issued its long-awaited decision in Boy Scouts of America v. Dale. James Dale, an Eagle Scout and assistant scoutmaster, had his membership revoked when the Boy Scouts learned that he was gay. (A New Jersey newspaper had reported that he was copresident of the Rutgers gay and lesbian student organization.) Dale brought suit under a New Jersey law that bans discrimination based on sexual orientation in public accommodations. A narrow majority of the Court found that the New Jersey statute violated the Boy Scouts' First Amendment right of "expressive association," that is, its right to organize around the articulation of its preferred core values and to select members and leaders consistent with those values.

Boy Scouts v. Dale was decided against a complex threefold background of contested issues: a constitutional debate about the often competing claims of associational liberty and individual equality; a normative/theoretical debate about the nature of civil society and its relation to liberal democracy; and an empirical debate sparked by Robert Putnam about changing patterns of associational life in the United States and the probable effects of these shifts. The articles in Freedom of Association help frame the broader constitutional and normative/theoretical issues and suggest considerations that empirical researchers would be well advised to consider.

In parallel critical examinations of the leading Court decision in this area (Roberts v. United States Jaycees), George Kateb and Nancy Rosenblum offer spirited defenses of conceptions of freedom of association broad enough to support the majority opinion in Boy Scouts v. Dale. In her penetrating introduction, Amy Gutmann points out that freedom of association is not a constitutional absolute and that hard questions arise when it clashes with other core constitutional principles. She observes that it is not always clear when an association is "expressive" or should be regarded as a "public accommodation." In a detailed examination of issues and cases, Kent Greenawalt argues that constitutional claims based on religious association are often stronger than are other sorts of expressive associational claims, and this differential treatment is "reasonably defensible" from a wider normative point of view (pp. 137-8). Peter de Marneffe agrees, but with a qualification: It would be morally wrong for a government to compel the Catholic Church to ordain women as priests in violation of canon law and conscience, but it would not be wrong to override "uncodified local tradition" (p. 156). More broadly, he argues, there is no correct general account of associational rights; from a moral as well as constitutional standpoint, there is no alternative to a case-by-case approach that weighs the competing interests and arguments at play in specific controversies.

The remainder of the essays in this volume place associational life within a civic frame. Although much of the discussion of associational liberty revolves around "voluntary" associations, Michael Walzer argues that involuntary associations are inevitable and (within certain limits) a good thing. Freedom, he claims, requires nothing more than the possibility of breaking involuntary bonds, and the individual's ability to do so can only be cultivated within groups and institutions in which membership is unchosen. Will Kymlicka discusses the tension between a key form of involuntary association-groups based on shared ethnicity-and the characteristic efforts of nation-states (including liberal democracies) to forge what he calls a "common societal culture" (p. 182). Broadening this analysis, Yael Tamir points out that associational life in free societies does not always support liberal democratic norms and practices. Nonetheless, she argues, the state must be very cautious about intervening in civil society; it must act to assure that all individuals have meaningful rights to associate and to leave associations, but it should not overestimate its ability to intervene productively in the internal life of civil associations. Approaching this tension from a communitarian standpoint, Daniel Bell suggests that if liberal democracy requires a distinctive form of civil virtue, it is likely that some civil associations will impede rather than foster its development, and public programs and institutions will be needed to counteract the centrifugal forces of civil society. Samuel Fleischacker argues that "insignificant communities"—groups that lack a compelling higher order purpose (such as individuals gathered at public squares and bars)—are more compatible with liberal democracy than are more comprehensive and demanding associations. Alan Ryan suggests that modern cities are sites for distinctive and valuable patterns of free association, and Stuart White argues that modern liberal democracies have good reasons to promote trade unions as distinctively valuable forms of free association.

Not every reader will be interested in every essay, but each is written at a high level of clarity, rigor, and relevance to problems that matter. Comparable collections were published more than thirty years ago, and much has changed in scholarship and in the world since then, so this volume helps fill a substantial void in the literature. Indeed, it is clearly the best introduction to issues raised by freedom of association in modern liberal democracies. It helps us understand why Alexis de Tocqueville is so much the man of the hour, and why fashionable neo-Tocquevillean enthusiasms must be tempered by uncomfortable economic, political, and even psychological facts. Voluntary groups are the source of much that is valuable, but they are not a silver-bullet cure for the characteristic ills of modern liberal democracy, and some of their practices are bound to trouble even dedicated defenders of free association who also care about the equal dignity of every citizen.

The Feminist Standpoint Revisited and Other Essays. By Nancy C. M. Hartsock. Boulder, CO: Westview, 1998. 262p. \$18.00 paper.

Mary Caputi, California State University, Long Beach

This collection by Nancy Hartsock traces the increasing theoretical sophistication of a leading American feminist scholar. The breadth, seriousness, and occasionally anecdotal style of these pieces make for engaging and often humorous reading on a wide range of topics. These include definitions of political power, Marxist and revisionist Marxist theories, the relevance of structural anthropology to gender, and postmodernism's rethinking of language, subjectivity, and power. The book does more than simply demonstrate how

the thoughts and feelings of Hartsock herself have evolved over time, however. To a good degree, it also operates as a touchstone for the debates within feminist theory as a whole, charting the direction taken by the field in the United States over roughly twenty years. It is for this reason that the book serves the needs of both research and teaching, for in many ways it demonstrates how feminist thought has evolved.

The earlier essays, composed largely in the 1970s, recall the effort to ground and explain the theoretical importance of sex and gender. They defend the need to study systematically a crucial axis of power that for so long had been invisible to many (as indeed it still is in certain circles), obscured by gender's "naturalness," which ostensibly flows into and structures the social sphere. Hartsock thus defends the position that the personal is political—a staple of second wave feminism—and exposes gender inequalities so often dismissed as unavoidable, unchangeable, and at bottom unproblematic.

Also, these early essays do not address themselves to an exclusively conservative readership but enter into conversations on the Left. Both "Fundamental Feminism: Process and Perspective" and "Staying Alive" seek to expose the abiding blind spots that many leftist scholars and activists still have when it comes to traditional gender hierarchies. Such masculmist premises as the abstract individual, the separation of mind and body, and the discrete spheres of public and private life preclude what Hartsock terms "the male left" from recognizing its own ingrained sexist practices and reactionary mind set. This has allowed the Left to dismiss feminism as little more than a distraction from the more pressing topic of economic relations. "The implication of this . . . is that (feminist) concerns are special interests and are fundamentally irrelevant to the real questions raised by monopoly capital' (pp. 34-5). Thanks to her criticism of such ingrained blindnesses, Hartsock is to be counted among those pioneering scholars who initially articulated a feminist critique of the Left, exposing a reactionary drift in circles otherwise thought of as progressive.

The later contributions from the 1980s and 1990s reflect the movement's inroads into the halls of academe and into American culture at large. Entering into conversations that are ongoing in feminism, they engage the work of other feminist scholars who have significantly influenced the discipline: Gayle Rubin, Nancy Chodorow, Adrienne Rich, several postmodern scholars, and others. They offer Hartsock's response to such crucial topics as the structuralism of Claude Lévi-Strauss; the incorporation of psychoanalysis into feminist thought; Foucault's reading of the location and workings of power, the intersections of gender, class, and race; the pervasiveness of compulsory heterosexuality; and the abiding usefulness of Marxian categories to feminism. Indeed, Hartsock covers nearly all the central concerns of contemporary feminism (with the exception, perhaps, of French feminist thought). It is, of course, the question of Marxian categories that is at the heart of her most famous contribution to the field. This argument in favor of feminism's reoccupying of Marxist terrain is developed in The Feminist Standpoint: Developing the Ground for a Specifically Feminist Historical Materialism." It explains Hartsock's epistemological methodology, which, in its synthesis of feminism and materialism, most encapsulates all that she holds dear.

Hartsock insists that certain components of Marxian theory are indeed valuable to feminist scholarship, and her obvious disdain for market ideology keeps her from mincing words on this point. "It is often claimed," she writes, "that the market can act as a school for democracy and a tool for encouraging democracy. I, however, see the market as a

mechanism for increasing inequality both globally and locally" (p. 7). Following the lead of both Marx and George Lukács (the concept of a standpoint is itself Lukácsian), her analysis privileges labor and production over the market's system of exchange and identifies work as the key contributor to epistemological questions. Hartsock thus embraces the materialist precept that "work produces both our material existence and our consciousness" (p. 47).

Such a theoretical commitment to the connection between labor and epistemology subsequently focuses on the sexual division of labor, on how women toil, and on how "work" is defined. Examining the gendered nature of production raises a battery of thorny and often emotional issues so crucial to bourgeois ideology. To scrutinize the sexual division of labor not only asks questions about the organization of the work week, the sexual politics of the workplace, and equal pay but also examines the structure and dynamics of the family, the politics of child rearing and housework, and the exhausting nature of the "double shift." Feminist standpoint theory takes seriously the necessary care taking that goes into shopping, cooking, cleaning, chauffeuring, and after-school activitiescare taking that need not be performed solely by women. It highlights the fact that, traditionally, "only some of the goods women produce are commodities" (p. 114), yet Western women live in societies whose organizing economic principle is the production and exchange of commodities. All this, of course, results in a general undervaluing of so much of women's labor, in an elision of their toil, when in fact "women as a group work more than men" (p. 114).

The concluding essay allows Hartsock to revise what she later came to view as a shortcoming in standpoint theory. This was her insufficient attention to differences among women, her tendency to oversimplify things with little regard for class, race, and other crucial axes of domination. Her revision is a welcome one, for such axes must come into play if a reading of power and epistemology is to be viable. Once modified by this corrective measure, Hartsock's argument surely finds a place at the forefront of the discipline, for her careful synthesis of materialist philosophy, feminism, and difference constitutes a seminal contribution to the field. Indeed, standpoint theory has become a staple in feminist scholarship, an integral part of the field's literature, conferences, reading lists, syllabi, and general discourse. But the author's ambition is not merely to influence the discipline's discourse, to add to its verbiage in deference to the thralldom of language. Mindful of the Marxian heritage that she so respects, Hartsock instead offers a way of changing the world, of altering its institutions and practices in fundamental ways, because "what we mean by political change is structural change" (p. 29).

Early Modern Concepts for a Late Modern World: Althusius on Community and Federalism. By Thomas O. Hueglin. Waterloo, Ontario: Wilfrid Laurier University Press, 1999. 280p. \$39.95.

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Johannes Althusius (1557–1638) spent his life defending the prerogatives of small political entities against the encroachments of large political entities. As the syndic of the seaport town of Emden, Althusius waged a daring campaign of disobedience toward the local would-be absolutist territorial ruler, the count of Eastern Frisia, and even put the count under house arrest at one point. Althusius's writings on politics, including his classic *Politica* (1603), oppose the centralized sovereignty that came to characterize the national

state, championing instead the autonomy of plural communities, such as guilds and towns, and a federal, consensus-based politics governing their relations. Althusius died ten years before the Treaty of Westphalia (1648), which formalized the system of sovereign national states that he had devoted a lifetime of writing and political activity to prevent. His political thought, based as it is on a society of estates that has not existed for hundreds of years, might well have gone to oblivion with the Standestaat. As Thomas Hueglin ably explains, however, Althusius's work is actually quite relevant to our world of declining national sovereignty, globalization, and increasing interest in politics on the subnational and transnational levels.

Hueglin aims to establish Althusius as an important theorist of politics by reconstructing his system and then arguing for its relevance. The author rightly insists that the reader pay attention to the political and rhetorical contexts of Althusius's writings, demonstrating nicely how "a decontextualized Althusius can be put to the service of all kinds of ideological agendas" (p. 23). Hueglin leads the reader through the history of Althusius interpretation, summarizing and evaluating scholarship from Otto von Gierke, Althusius's modern "rediscoverer," through Carl J. Friedrich and on to Hueglin's very few contemporaries (including especially Anthony Black, Guilds and Civil Society in European Political Thought from the Twelfth Century to the Present, 1984). Next Hueglin presents a reconstruction of the Althusian political system, topic by topic, with issues of contemporary relevance included as they come up.

The advantage of this mode of presentation is that Hueglin largely succeeds in demonstrating the relevance of such Althusian concepts as consociation, federalism, and consensus-based, nonmajoritarian governance. With regard to consociation, for example, Hueglin not only points out the connection, via David Apter, to Arend Lijphart's work but also draws a connection between Althusius's very general concept of consociation as mutual communication and the contemporary debate on the modern distinction between the public and private spheres (chaps. 7 and 12). In five expository chapters (chaps. 7-11) and a concluding chapter (chap. 12), Hueglin discusses an impressively broad array of political theories as they relate to his subject. Althusius's early modern community-based federalism is compared to current theories of federalism, social contract, civil society, democracy, liberalism, socialism, globalization, citizenship, international relations, and even personal identity. Although discussion is frustratingly thin at times (e.g., Hueglin attributes to Kant the view that theory does not necessarily apply to practice [p. 37]), the author's vast knowledge of contemporary political theory serves his purpose well, as Hueglin can demonstrate at least "affinities" if not "lineage" (chap. 12) between Althusius and the present day.

There is a disadvantage to Hueglin's choice of argumentative style, however. By focusing on the reconstruction and application of Althusius's theory, Hueglin reserves his critique for Althusius's interpreters and fellow political theorists. He never presses Althusius for answers to the difficult questions, such as those posed from a liberal standpoint. Althusius opposes absolutism and supports plural federalism in the name of community rights, not individual rights. If political authority comes from the nonmajoritarian consent of a collection of independently legitimate communities, as Althusius argues, one might well ask about determination of membership, or about protection of minorities within these minorities, or about the effectiveness of any governing body that requires unanimity for its decisions. Hueglin has no time for sustained consideration of these and other "big ques-

tions" in this book, which sometimes seems to be more about vindicating interest in Althusius than about vindicating his theory.

The book gives us access to an interesting point of view by asking us to consider politics beyond Bodin's and Hobbes's presumptions about sovereignty and the state. Politics in Althusius's system is plural, in that institutions are organized without recourse to any universalist arbiter of the right. Authority is divided both horizontally and vertically, and power is kept close to fairly small units of society. Large-scale decisions are made on the basis of consensus, and some group rights are respected. Against any theory that would locate power in a central authority, Althusius posits a decentered, community-based system of governance. Hueglin cites the European Union, federally organized states such as Canada, globalization, and multiple overlapping citizenships, among other phenomena, and he concludes that today's political reality requires a new theory appropriate to its conditions. He offers a well-documented, exhaustively researched reconstruction of Althusius's political thought to fill that void.

Postmodernism Rightly Understood: The Return to Realism in American Thought. By Peter Augustine Lawler. Lanham, MD: Rowman & Littlefield, 1999. 193p. \$60.00 cloth, \$19.95 paper.

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This deeply serious and richly thought-provoking book opposes a "postmodernism rightly understood" to "postmodernism as it is usually understood." The latter, as "antifoundationalism, the assertion of the groundlessness of human existence, is really hypermodernism, or the exaggeration to the point of caricature of the modern impulse to self-creation." The former is the return to "Thomistic" or even "Socratic" rationalism, according to which "human reason exists primarily not to transform reality but to understand and to come to terms with it" (pp. 1–2).

More precisely, "realistic" postmodern thought recapitulates a Pascalian analysis of the human condition as defined by "the greatness and the misery of self-conscious mortality" (p. 5). Humanity's confrontation with the inescapable abyss of death necessarily engenders a pervasive sense of alienation that spurs the soul either to desperate escapism in manifold forms or to a quest for nonutilitarian freedom, love (eros), virtue, and wisdom that can find complete fulfillment only in the biblical God. At the core of the modern project, in contrast, is the pragmatic and therapeutic hope that rational manipulation of the environment and of the psyche can end all profound human suffering and thereby reveal humans as nothing more than very clever, historically malleable, and ultimately satisfiable animals whom death in itself does not deeply move. Therefore, they can be liberated from delusory transcendent longings that were artificially constructed in and by history.

The first chapter ascends from what Lawler regards as Fukuyama's equivocating presentation of Kojève's thesis on the "end of history" to the genuine article, in its ruthless consistency. Fukuyama proclaims the emerging global and permanent victory of prosperous, healthy, humane liberalism as the dawn of Kojève's "universal and homogenous" society—while incoherently attempting to assert that within this order there will persist scope for human dignity and achievement. Kojève sees that the realization of this ideal, since it

entails the eradication of suffering and principled conflict, leaves no challenge for transformative work or moral action, or even, ultimately, for passionate inquiry.

Even though Lawler remarks that, among other things, this "shows that the Christian conception of the eternal, or infinite, Creator is incoherent" (p. 34), he does not, to my disappointment, respond with any attempt at a refutation. Instead, he descends in the second chapter to a somewhat diffuse refutation of Rorty, the most telling point in which I discern to be the following. Rorty proclaims the impending victory of his view that "a merely material and secular goal suffices: mortal life as it might be lived in the sunlit uplands of global democracy and abundance" (cited on p. 48). But in stark contrast to the Hegelian Kojève, Rorty denies that this view can claim to be true, or even that his favored emerging society is "more natural or more rational than the cruel societies" of the past (p. 57), for the truth is the contingency and lack of objectivity of all such ultimately groundless "choices."

But what is the ground of this Truth? Most acutely, what experiences or facts speak against the greater rationality or truth of at least the "sufficiency" of universal solidarity and prosperity? The clearest answer is Rorty's recognition of another Truth, an insuperable dilemma of human nature that Rorty criticizes Marxists for ignoring: "the price we pay" (Rorty writes) is "the culture that put Socrates to death," dominated by the "bland, calculating, petty, and unheroic"; a culture where "we, the people who value self consciousness, may be irrelevant to the fate of humanity" (pp. 54-5). But then Rorty himself testifies to the unsatisfied or unsatisfiable spiritual longings that block full acceptance of pragmatism's goal as truly sufficient for self-conscious human nature:. Rorty's "posthistorical wisdom contradicts his own existence as a philosopher" (p. 59). Rorty tries to obscure but in fact spotlights the fundamental difficulty by retreating into "irony," and Kojève may similarly betray or call into question his thought by his comparable "irony" (pp. 60-1).

The curious twist is that the chapter on Rorty has as its most serious goal an attempted refutation of the more profound Allan Bloom, as the exemplar of the supposed inherent weakness of intransigently classical (unPascalian) Socratic rationalism. Bloom is portrayed as an unaware witness for Rorty, inasmuch as Bloom's Socratism compels him to describe contemporary students as satisfied in soullessness, which implies that all humanity except the "idiosyncratic" philosopher is naturally incapable, absent historical artifice, of responding to mortality with erotic passion: Bloom's Socratic message is that the "flat soul is the true soul" (p. 69). Not only does this amputate Bloom's moving accounts of the numerous students whose eros he has seen electrified by the redemptive great books, but also Lawler proceeds to contradict himself. In an awkward "Afterword" he is compelled to concede that actually Bloom spotlighted the "boundless" anguish of children of divorce as the "theme of reflection and study." Bloom called them "the symbols of our time" because he saw "their disarray in the cosmos" as only the "extreme form" of what he called our all-encompassing "spiritual vortex set in motion by loss of contact with other human beings and with the natural world" (p. 72).

The unsuccessful attempt to dispose of Bloom is meant to remove the competition from what Lawler so compellingly elaborates and explores in the subsequent, constructive chapters: the Christian writings of Walker Percy, bolstered by the Augustinian sociological class analysis of Christopher Lasch. Here, Lawler contends, we find the authentic, because Thomistic, rationalism, which shows that reason finds its fulfill-

ment, without truncation, in Christian faith and which proves that only reason so conceived can adequately comprehend humanity as constituted by self-conscious erotic mortality. But in fact Lawler has to show that Percy abandons Thomas's core Aristotelian teleology in the name of a farrago comprising the "tradition of Anglo-Saxon empiricism" (p. 84), a tendentious shot-gun marriage of creationism and Darwinism (pp. 88–9), and a Heideggerian "experience" of the human as "alien" in "a cosmos that is otherwise dyadic" or "stimulus-response" (p. 80).

What Percy calls his "Thomistic science" is, Lawler admits, in truth "a combination of Pierce's semiotic empiricism and Heidegger's [atheistic] existentialism" (p. 97). It is then not surprising that although lip service is paid to "natural law," no attempt is made (in the manner of authentic Thomists such as Vitoria, Suarez, or Pieper) to give the much needed argumentative defense of that core Thomistic doctrine. Above all, Thomas's moral grounding in Aristotle's Politics and Ethics, culminating in the peak of aristocratic greatness of soul as the foreshadowing of the contemplative life understood as the only intelligibly divine life, as the life whose acknowledged supremacy ought to be the beacon of inspiration for every rational human soul, shrinks almost out of sight. With all this subtracted, what is left of Thomas's argument for Christianity as the completion of Aristotelian (the only integral) rationalism?

Lawler's illuminating introduction to Percy as a truly weighty thinker for our time would grow in strength through a franker acknowledgment of the problematic gulf between Percy and genuine Thomism, then through a fairer and less evasive dialogue with the challenging alternative conception of divinity and of humanity found in classical, Socratic-Aristotelian rationalism.

Acting in Concert: Music, Community, and Political Action. By Mark Mattern. New Brunswick, NJ: Rutgers University Press, 1998. 240p. \$50.00 cloth, \$20.00 paper.

William Chaloupka, University of Montana

Political scientists have long been ambivalent about including some particular cultural and social modes of action into their standard inventory of political acts. Street demonstrations, crvil disobedience, and popular culture, to name a few, have been slow to capture their attention, despite wide popularity among advocates for such expressive vehicles. There are good reasons for this reluctance. Such forms are difficult to assess; their importance often remains a matter for speculation; and the range of interpretations can be broad. But whatever the reasons, it is clearly the case that other academic fields have gone farther than political science in studying some of the less mainstream modes of political expression and action.

Acting in Concent attempts to deal with one form of cultural activity—popular music—in a way that is distinctively within the realm of political science. Rather than adopt available models in cultural studies or ethnography, to cite two predecessors, Mattern studies music as if it were a field of public action that a political scientist would observe and analyze. Within the overall project of showing that music can have political implications and can be studied in this manner, the primary innovation is Mattern's suggestion of a taxonomy for popular music's political functions. In a community context, music can adopt different strategies for community-based

political action. Mattern (p. 25) introduces three categories: the confrontational, deliberative, and pragmatic.

The book examines at length three cases that illustrate these categories and also develops the argument that music indeed plays a significant political role. The three cases are oppositional music in Chile (from the 1960s through the 1980s, focusing on events surrounding the Allende government's rise and fall), among the Cajun community in Louisiana (in this century, focusing on the revival in the 1970s through the 1990s), and in Native American communities in the upper Midwest (in the 1980s and 1990s). In each case, Mattern reviews the political context from which the music emerges and then describes the music in terms of its origin, instrumentation, aesthetic qualities, and lyrical content or performance format. In the introduction and first two chapters, Mattern reviews the relevant political science literature and connects his argument to theories about the formation of community, primarily those of the American philosopher John Dewey.

Of the three, the Chilean case provides the clearest example of protest music sung in conjunction with dramatic political and social conflict. Deprived of access to other forms of mass communication, groups aligned with workers, the poor, and indigenous communities promoted their cause through songs that were clearly confrontational in lyncal content and performance format. Of all the examples in the book, Mattern (p. 38) is most critical of the "new song" movement's approach: "Although it is not surprising that musicians relied heavily on confrontational strategies, it was nevertheless unfortunate." He also criticizes the hypocrisy of educated and elite musicians who adopt the music of oppressed peoples to promote political ends (pp. 45–6).

The Cajun case provides elements of both the pragmatic and deliberative modes. The revival that began in the 1970s helped focus cultural identity, which served a pragmatic function. In the deliberative mode, the discussion around music provided a forum in which Cajuns could debate such crucial topics as assimilation and progress. But the Cajun case also includes a negative example. Although black and white musicians in southwestern Louisiana obviously interact and learn from one another, music has not played a significant role in bringing racially mixed audiences or communities together. There are limits to the community-building powers of music.

The Native American case likewise illustrates both the pragmatic and deliberative modes. Although Mattern finds little that is confrontational in the powwows he studies, he acknowledges that music plays a confrontational role elsewhere in Native American communities. Perhaps the best example of a deliberative function for music is found in his description of the ongoing debate over the secular-sacred split, which can persist even during powwow drumming and dancing, in the actions and announcements of participants.

Acting in Concert is a short book (146 pages of text). Given the task of providing contextual information for three diverse cases, as well as articulating the three categories, Mattern must necessarily take the role of provocateur. There is much to debate in these cases, and some exclusions can be questioned. Is it fair to criticize the Chilean rebels, who faced such difficult circumstances? Does it make a difference that both the Cajun and Native American cases discuss cultural revivals that parallel the development of identity politics in the United States? Several widely debated instances of cultural politics receive the barest mention, including rap music and the role of music in the 1960s counterculture. The use of culture as a target for resentment politics likewise receives little discussion, which might suggest that Mattern's persis-

tent focus on the intentions and functions of music could miss other important and relevant political events. In sum, Mattern provokes debates more than he resolves them.

The political role of music is a topic that could benefit from an interdisciplinary approach. Sociologists, art historians, anthropologists, literary theorists, students of social movements, and cultural studies scholars all have something to add to this discussion, beyond the scope of this brief study. After all, fostering community identity, a function that Mattern raises frequently, is as much a social and aesthetic event as a political one. There is little reason to draw disciplinary lines too severely, especially once a political science starting point has been established. By emphasizing function and intention, Mattern brings a thoughtful approach to a topic that contains potential for controversy and dispute. This is a reasonable place to start, not the least because it promises so many openings for further work.

Ruling Passion: The Erotics of Statecraft in Platonic Political Philosophy. By Waller R. Newell. Lanham/Boulder/New York/Oxford: Rowman & Littlefield, 2000. 205p. \$70.00 cloth, \$24.95 paper.

Stanley Rosen, Boston University

This extremely interesting book contains a subtle and finely detailed reflection on one of the main problems in Plato: the relation between eros and thumos (spiritedness). The text is dense but not obscure. On the contrary, it makes a multiplicity of observations that are at once original and yet so plausible that one is tempted to ask at countless points in the argument: "How could it have been otherwise"? At the same time, Newell's treatment of the problem is faithful to the dialectical presentation in the dialogues, which is to say, in praise of Newell, not criticism, that it could indeed have been otherwise. Even more fundamentally, one is driven to ask. What was Plato's view if not that he himself explains to us why it will very likely be otherwise?

Newell asks us to consider how the Platonic Socrates tries to rescue us from the tragic consequences of "primordial ontology" (Newell's phrase for pre-Socratic cosmology), an ontology that provides no basis for attributing significance to human life. Newell argues by way of close analyses of relevant parts of the *Gorgias*, *Symposium*, and *Republic* that Socrates proceeds in three main stages. In the *Gorgias*, he attempts to convince Callicles and others who are politically ambitious that their "longings for both honor and pleasure will ultimately be better fulfilled by civic virtue than by tyranny" (p. 2). This in turn depends upon the submission of the spirited part of the soul to an education in civic virtue. "Socrates hoped to engage Callicles' love of victory, but this is just what makes Callicles unreachable by dialectic" (p. 20).

In the Symposium, Socrates identifies himself with the thesis of Diotima that there is a continuous erotic ascent that takes us up the ladder via politics to philosophy; in slightly different terms, the philosophical erotics as practiced by Socrates provides us with both egoistic or self-perfecting pleasure as well as the spirit of philanthropy (pp. 63, 71). At the same time, there is a tension within eros between the desire for mastery (represented by Callicles and Alcibiades) and the desire to transcend the human condition in the view of the ideas (p. 82). The apparently seamless web of the erotic ascent already reflects a deeper version of the conflict between thumos and eros, namely, a dualism within eros itself. Socrates not only must reconcile thumos to philosoph-

ical eros but also must protect the philosophical component of eros from its root in the lower desires.

This brings us to the *Republic*, about which Newell argues that thumos can be educated to be both civic-spirited and in a way philosophical (p. 103). Newell's analysis of the nature of thumos is one of the highlights of his book, and it would be impossible to do justice to it in a short review. I can mention only the heart of the matter. Socrates separates thumos from the dangerous force of eros and makes it the key part of the soul for the political education of the auxiliary class in the beautiful city (pp. 63, 92). He does this by assuming that thumos can "see," that is, its perception of the difference between friends and enemies is capable of development into civic virtue by the proper education, and this is in a way philosophical (pp. 79f, 147).

In this section of the *Republic*, eros is suppressed; liberated from eros, thumos is made gentler and capable of political rule or civic virtue (pp. 116f, 139). But it returns, as it must, in the discussion of the philosophical nature, and here the problematic status of the relation between eros and thumos springs up with full clarity. To mention only the most important point, thumos, which is marked by "correct opinion," becomes dissatisfied with its subordination to philosophical eros; indeed, the skepticism of the latter toward the former serves to exacerbate the rebelliousness of thumos and thus leads to the overthrow of the city (pp. 144, 157, 169). Throughout the second half of his study, Newell is especially sensitive to the question whether philosophy can be made truly compatible with politics or civic virtue. He is eloquent on the harmful effect of the former on the latter, but at the same time he is hopeful that Socrates' philosophical erotics may be able to reestablish our vision of the unity of the soul (see, in particular, the remark about Glaucon on p. 180, together with p. 94).

This summary at least indicates the richness of Newell's book. I must add that there are sections of unusual brilliance devoted to the character and pertinence of Callicles to each of the three main dialogues studied by Newell, and there is an unusually brilliant commentary on the Leontius passage in the *Republic*. I had some reservations about a number of points in the main argument, of which perhaps the most serious is with respect to what Newell calls philosophical erotics. In a typical passage, Newell claims that "the investigation of civic virtue is the subject matter of philosophical erotics" (p. 179). This seems too restrictive, in view of Newell's own repeated perception that the highest form of eros is for the so-called Platonic ideas or, as he puts it, transcendence.

Newell has accomplished so much that it would be churlish to ask for more, but I suspect that, if he had dealt with *Phaedrus*, his portrait of eros would have been enriched. Otherwise put, Newell deals with the political context of eros—that is his privilege and his central theme—but as a result, we see only one side of the complex portrait of philosophy in the Platonic dialogues. Socrates is only a part of that portrait; other parts lead to the question of the status of eros in philosophy; for that matter, there is evidence in Alcibiades' speech in the *Symposium* that Socrates was not the lover of human beings he claimed to be. But this is almost beside the point; we have here one of the best books on Plato to have appeared in a number of years, and part of its excellence is that it draws us into the dialectic.

The Dominion of Voice: Riot, Reason, and Romance in Antebellum Politics. By Kimberly K. Smith. Lawrence: University Press of Kansas, 1999. 320p. \$40.00.

Mark Reinhardt, Williams College

Through this learned and imaginative theoretical excursion into American political history, Kimberly Smith issues a challenge to deliberative democrats and their fellow travelers. The book is a genealogy of the connection between democracy and rational debate: The goal is to make that connection more contestable in the present by revealing its historical contingency. In Smith's telling, early advocates of the politics of deliberation used conceptions of rationality, persuasion, and force in a way that impoverished the democratic repertoire. Yet, contemporary thinkers have unwittingly inherited and naturalized these-aporetic or untenable—terms. Democratic theory thus often employs ideals of political reason that cannot come to terms with "the positive role that interest, passion, compassion, and even violence might play in the political life of the nation" (p. 8). Upon recognizing these limitations, and the ways some antebellum insurgents overcame them, we will view rational deliberation not as the proper medium of our political life but merely as one among many democratic possibilities (p. 242).

Smith begins with an analysis of the contentious eighteenth-century discourse about riots. Before the Revolution, critics saw mob action as a deplorable breakdown of deference and order, whereas defenders legitimated it as a kind of purposeful and orderly disobedience, but both sides agreed that riots were acts of political resistance by the democratic element or "the people." Smith shows how the founders. especially Federalists, transformed this interpretive framework by presenting rioting as the triumph of uncontrolled "passion" over rational argument and, as such, an attack on the foundation of the republic, the people's capacity for self-rule. Smith's key point is that this ideal of argument was not cause but artifact of the Federalists'-independently motivated rejection of popular turbulence and insurgency. Reconceptualizing riots was crucial to their decisive redefinition of politics (p. 49).

Subsequent chapters follow the checkered career of the norm of rational politics, beginning with antebellum critiques that placed mob action still farther outside the boundaries of the political and proceeding to the contending conceptions of public debate and deliberation that grew from those critiques. Neoclassical advocates of the art of rhetoric could account for the power of political speech to move popular audiences, but only in a way that fueled fears of demagoguery: Critics maintained that a politics of eloquence would erode rationality, equality, and citizen independence. Enlightenment advocates of a more rationalist model of public discourse honored those threatened values but at the cost of rendering politics impossibly sterile; their strictures on deliberation left no room for existing means of or motives for political mobilization.

Of course, as Smith adds, elite advocates of an enlightened politics usually wanted not to motivate but to curtail popular activity (p. 149). In the concluding chapters, on abolitionism, she argues that mobilization against dominant institutions required opposition to prevailing epistemological premises and rhetorical models. This subtle argument correctly emphasizes the centrality of pathos and sympathy to abolitionist rhetoric but recognizes that slavery's opponents often invoked moral and cognitive universals. Smith captures this complexity in an astute analysis of slave narratives as "romances about the enlightening power of political reason" (p. 166). Both the reason and the romance matter. Abolitionists

saw slavery as infecting the whole of people's moral nature, corrupting both thought and feeling. Smith shows that this corruption could be confronted only through an ambitious "politics of storytelling," a politics that, like earlier forms of street protest, fell outside the boundaries of deliberative models. She thinks these examples show us why and how the boundaries need to be redrawn now.

Smith's argument is nuanced and impressively well researched, illuminating both broad intellectual traditions and specific exemplary texts (e.g., Uncle Tom's Cabin). Her account of the uses and meanings of "sympathy" in abolitionist discourse is particularly rich. She offers fresh insights even when primarily engaging work by other scholars: Her commentaries persuasively revise accepted accounts of, for instance, the rise of professional police and the tasks of the slave narrator. Most important, the book succeeds in its larger genealogical ambition of showing how variable, confused, and contested were the antebellum boundaries between argument and violence, reason and passion, and how politically problematical was the drive toward a deliberative conception of democracy.

Still, the argument has some notable shortcomings, for all its impressive scholarship. Some of these concern its approach to history. Smith's accounts of the neoclassical and enlightenment approaches to public speech sometimes reify these ideal-typical constructs, obscuring the messiness of antebellum discursive formations and conflating her categories and concerns with those of her subjects. Similarly, when asking whether slave narratives were able to persuade readers about the facts of slavery, she answers less through an empirical assessment of reader responses than through her own philosophical reasoning about what constitutes good evidence (e.g., p. 197). An inverse weakness mars her otherwise first-rate analysis of abolitionism. The texts she analyzes were thoroughly Christian: On her own account, their politics of storytelling was dependent on readerly consensus about the nature and existence of God. Yet, Smith clearly wants us to take that narrative politics as a model. Can we do so in a more spiritually heterogeneous world? Smith seems troubled by the question but rushes past it, ending the final substantive chapter on a note of confusion and obscurity.

This weakness is carried over to the book's brief and disappointing conclusion. Smith reiterates her claim that contemporary democratic theorists underemphasize the role of compassion and overlook the ways in which reasoned argument can serve domination (pp. 240-1), but although this claim frames her whole project, she neither develops her theoretical argument nor engages the advocates and critics of rational debate. Smith has provided a remarkably rich historical context for the current disputes over deliberative democracy, but she seems strangely unaware of their scope (pp. 4, 241). Yet, the very theoretical issues that concern her are central to work by such contemporary theorists as, say, Iris Young (e.g., Justice and the Politics of Difference, 1997; Interesting Voices: Dilemmas of Gender, Political Philosophy, and Policy, 1990), and they have been crucial to the debates between Habermasians and those influenced by Foucault or Arendt. Indeed, much of the conflict over what has gone under the sign of "postmodernism" touches on Smith's themes. Unfortunately, little evidence of this is found in her book.

Although Smith fails to make the best of the opportunity, that she offers so much that could be put to use in the current debates is one important measure of her achievement. *The Dominion of Voice* exposes, sometimes brilliantly, the scandalously contingent and fraught historical relationship be-

tween democracy and rationality. In doing so, it makes an original and significant contribution to the study of American political thought.

The Common Law Mind: Medieval and Early Modern Conceptions. By J. W. Tubbs. Baltimore and London: Johns Hopkins University Press, 2000. 253p. \$42.50.

Cynthia J. Neville, Dalhousie University

Legal scholars of the modern era have attempted vigorously and with varying degrees of success to find in late medieval and early modern jurisprudence a single "common law mind." The characteristics that set English common law apart from continental models ensured that writers on the law from the thirteenth through the seventeenth century did not speak in a cacophony of voices, and there has been some consensus that a strand of legal thinking can indeed be traced from the Year Books through to the writings of Coke and beyond. The chief representative of this school of thought remains J. G. A. Pocock, whose work first stirred up the scholarly community more than a decade ago (The Ancient Constitution and the Feudal Law, 1987). Tubbs sets himself a difficult task, then, in his attempt to challenge currently received historiography about early jurisprudential writing in England. That he fails to identify a single common law mind is perhaps inevitable, given the breadth of his survey of surviving primary sources. Indeed, he admits that it is a near impossible task: "The most that we can say, on the evidence we have, is that if there was a common law mind in the period, it was divided" (p. 195). Tubbs arrives at these concluding remarks with some assurance, and on the way to doing so his examination of legal writing in England offers thoughtful, if sometimes uneven, analysis.

The attempt to prove the existence of a multifaceted common law mind allows Tubbs to develop two separate themes, both of which are explored at length in each chapter of the book. The first concerns the ways in which legal thinkers from the anonymous compiler of the twelfth-century Leges Henrici Primi through to the great authors of the period preceding the English Civil War conceived of the idea of reason and of the relationship between the legal past and constantly evolving notions about reason. The second theme, less extensively emphasized but equally well developed, has as its focus the degree to which Roman law influenced English law and jurisprudence. Tubbs ranges far and wide in his exploration of both themes, touching on such topics as attitudes toward statute law, the definition of the infinitely variable idea of custom, the place of precedent in legal decision making, and the role of equity in the history of English law. Although the arguments he makes in respect of these subjects are variably sustained, there is much here to provoke both criticism of and praise for the author's use of an extensive range of source materials.

The contention that there never existed a single common law mind is an ambitious claim, and it leads Tubbs of necessity to review and reassess—and often to cast doubt on—the work of scholars who have preceded him. There is hardly a single historian or philosopher, alive or dead, whose arguments are not subjected to careful scrutiny, then rejected. Brian Tierney's treatment of *Bracton*'s use of the terms consuctudo and leges is tenuous because it is based on a misunderstanding of these words (p. 14); T. F. T. Plucknett relies on only "a handful of cases" in what are widely regarded as definitive statements regarding statute law (p.

35). Similarly, Samuel Thorne is mistaken in linking changes in parliamentary procedures with evolving doctrines of statutory interpretation (p. 117) and Charles McIlwain's views of the nature of parliament are based on a "mistaken theory" (p. 157). Pocock himself is wrong in his assertion that lawyers of the early modern period equated common law with custom (p. 129).

Such bold statements are, of course, the stuff of historiographical debate, but they sound rather too strident coming from a scholar as new to the profession as is Tubbs. Moreover, the author's tendency to criticize his predecessors' use of source materials by means of frequent reminders that his own research encompassed an exhaustive reading of "all extant works on English law from the twelfth through the seventeenth centuries" (p. xii) is offset by some notable lacunae in the secondary sources he consulted. For some topics Tubbs relies almost exclusively on long outdated works. Notable is the discussion of the uses for which medieval Year Books were intended, where the only secondary works cited are those of William Holdsworth (pp. 42, 63). More recent studies have superceded the limited conclusions offered by Holdsworth some time ago and in some respects have shown them to be fundamentally flawed.

Tubbs is on much surer and less contentious ground in his treatment of the enduring influence of civilian and canon law traditions on English legal thinking and writing. The argument is not novel: Scholars such as Richard Helmholz (see e.g., Canon Law and the Law of England, 1988; Roman Canon Law in Reformation England, 1990) have maintained consistently that Roman law exerted a more profound effect on the common lawyers of the medieval and early modern periods than legal historians generally allow. Tubbs's strength lies in an impressive ability to offer specific examples of the link between Roman and common law thinking in the writings of various authors. Thus, the (few) passages in the treatises known as Glanvill and Bracton relating to the nature of customary law are almost all "borrowed or derived from the writings of civilian jurists" (p. 189); later still, Sir John Davies (d. 1626), writing on the subject of royal prerogative, employs arguments that are based, "almost without exception," on Roman law authorities" (p. 134). In his more specific discussion of late-sixteenth- and early-seventeenth-century views on custom, however, Tubbs uses the term "classical" rather than "Roman" or "civilian." Here, the strength of the argument depends heavily on the inclusion of Aristotelian traditions of logic within the framework of the term "civilian." The change of emphasis is subtle but essential to what the author believes is an accurate understanding of the role of custom in common law.

Tubbs's conclusions about the influence of the civilian tradition are essentially sound; less satisfying are those offered with respect to the variety of "common law minds" in the medieval and early modern periods. Despite a commendable effort to unravel distinct strands of thought in jurisprudential interpretations of the notion of custom, Tubbs is forced to acknowledge in the end that virtually all discussions about the law were in fact linked inextricably to that same notion. His argument regarding the plurality of common law minds is not as far removed from those of other scholars as he might wish. Still, Tubbs's book provides a salutary, if not altogether convincing, reminder that the incontrovertible schools of thought that so often inform scholarly studies of medieval and early modern English law are in constant need of discussion and, sometimes, revision.

The Third Millennium: Reflections on Faith and Reason. By David Walsh. Washington, DC: Georgetown University Press, 1999. 256p. \$65.00 cloth. \$22.95 paper.

Joshua Mitchell, Georgetown University

"Without war," Rousseau writes in the First Discourse (1750), "there would be no history." What, then, is the place of political philosophy in a cosmopolitan world, in which passions have been transposed into interests, and war has been rendered obsolete by free trade? Perhaps that is a dream for the future, but nevertheless we are justified in asking whether political philosophy is useless when wars end. The efflorescence of political philosophy in the twentieth century-Strauss, Arendt, Voegelin, and a host of others come to mind-cannot be understood without reference to the conflagrations that were witnessed. What now? The century of ideology has ended. Whence political philosophy? After Ideology (the title of one of David Walsh's books [1990]), now what? That is not directly Walsh's concern in Third Millennium, but his recent work wrestles with just this question. The present book extends his ruminations on the subject.

A perusal of the political philosophy literature at the dawn of the new century suggests that Rousseau's intimation is being confirmed: Democratic theory and other largely ahistorical modes of inquiry are rising to preeminence, and the study of the history of political thought seems to many to be of antiquarian interest only. Walsh's book attempts to redress this imbalance by considering the meaning of the advent of the third millennium, both for the West and for the world as a whole. Tocqueville remarks in Democracy in America (1835) that, in the democratic age, man deliberately forgets his past and attends to the immediate concerns of management. Contemporary political philosophy increasingly conforms to this prejudice. For Walsh, our entrance into the third millennium offers an occasion to take stock of the larger meaning of history for human affairs. In doing so, perhaps there is also an opportunity to retool political philosophy for the postideological age, in such a way as to take history seriously again.

What shall we make of the Christian categorization of history, according to which we have just entered the third millennium? This is, for Walsh, more than a mere convenience. It speaks to the ongoing presence of a Christian world, which is confirmed less by doctrinal and confessional adherence than by the existence of those two pillars of the modern, increasingly cosmopolitan, world—the natural sciences and human rights. Both are historically indebted to Christianity and, more to the point, must continue to rely on Christianity in order to retain their coherence. The question of their ongoing debt is the interesting one to consider, of course, since today a great many secular liberals—Walsh is a theological liberal—think that the debt need no longer be paid. Modern man is debt free.

In order to assess this question, the relevant categories must be established. Those whose allegiances are owed to the Enlightenment think that the proper categories are enlightenment, on the one hand, and religion, on the other, the latter of which is taken to be inimical to scientific investigation and the idea of human freedom. Walsh does not cite Augustine, but he well might have. Augustine's City of God (430) supposes that the real alternatives are not enlightenment or religion but paganism or revealed religion. (In Levathan [1651], Hobbes understood this, too.) Man is a religious animal; he cannot live long without enchantment. The question is not whether mortal life will be enchanted but where it will be enchanted.

Myth is one manner in which the world can be enchanted,

but the age of myth is surely over, anthropologically at least. The Greeks, notwithstanding their discovery of philosophy, retained the idea of a cosmos that was enchanted, and so they could not develop natural science worthy of the name (pp. 131-2). With Christianity, however, we have for the first time the definitive possibility of recognizing "this world as a world ... [for] it is only the light of transcendent perfection glimpsed momentarily that reveals the finitude and imperfection of everything else" (p. 130). The possibility of conceiving of a disenchanted world is made possible by a divine condescension "into the world," a phrase that already confirms Walsh's point. Revelation occasions the differentiation that the natural sciences require in order to proceed. Furthermore, by virtue of God becoming man, "no higher value can be attached to finite existence than to regard each individual as a unique irreplaceable vehicle for the radiance of transcendent presence" (pp. 140-1). God in mortal form reveals to man his own final destiny and standing.

The escape from religion is not the precondition for the success of the natural sciences and human rights, far from it. They require a differentiation of reality into distinct aspects that Christianity best provides. The natural sciences are possible because on this side of the divide enchantment is impossible; human rights are possible because on the other side of the divide we find an authorization for human personhood. The ongoing separation, which Christianity decisively achieves, is necessary for the natural sciences and for the idea of rights to remain coherent. Thus, the modern mind is apt to find the twin pillars of the modern age—the natural sciences and human rights—natural to the imagination because, "irrespective of personal inclinations or faith, it is impossible to think outside of the categories defined by the revelation of Christ" (p. 129). Christianity has won! Nietzsche states in The Genealogy of Morals (1887) that "it is the Church and not its poison that offends us." The natural sciences and human rights are the poison that we continue to imbibe, although not to our detriment, as Nietzsche would have it, but rather to our benefit.

Like all historical achievements, however, there is no absolute assurance that the Christian disenchantment of the world will retain its hold on the modern imagination. Walsh's point is that Christianity maximally differentiates the world; the open question is whether such differentiation can be secured into the future without the ongoing presence of Christianity, however conceived.

One of the more interesting claims of *Third Millennium* is that without the existential balance, as it were, made possible by the advent of Christ, which situates all that we do in the context of both finitude and transcendence (p. 144), reason loses its stability, and human life "oscillates between enthusiasm and dejection" (p. 226). Not only science is at risk, however; without a substantive grounding in Christianity, human rights talk proliferates without end. "In place of a well-defined understanding of human dignity, we are left with a blank slate on which anything may be written" (p. 100). To those who argue that the modern achievement of the natural sciences and human rights no longer need acknowledge a debt to Christianity in order to be vouchsafed for the future, Walsh suggests that any deformation in the Christian accomplishment will deform the natural sciences and human rights talk as well

A host of questions can be raised here, some perhaps answerable, some not. If Christianity is necessary, how may we may render it what is due? The influence of Voegelin on Walsh's thinking rules out any easy answer. Moreover, this influence occasionally colors Walsh's assessment of modernity too strongly: The close connection between faith and

reason in this superb analysis quietly indicts the project of the Reformation, which sought to rethink that connection in light of the experience of the radicality of sin and the experience of anxiety that attends it. Because any analysis of the emergence of modernity must give considerable weight to the Reformation, a supplemental analysis is required. Finally, although there is very little in the way of Christian triumphalism in Third Millennium, it would be nice to hear more about the global conversation among world religions that surely will occur in the present century. If Christianity best authorizes the natural sciences and human rights, and if these are the true global currency, then what does this say about the manner in which other religions—or Judaism and Islam within the monotheistic tradition—can authorize such things? These are important questions, for on them hinges two distinct possibilities: either Christian imperialism or the arrival of a genuine global pluralism, in which the natural sciences and human rights are each authorized by endemic religious traditions.

This is a very fine work that I recommend highly to anyone interested in the question of where political philosophy ought to direct its attention now that the totalitarian crisis has receded.

Taking Language Seriously: The Narrative Foundations of Public Administration Research. By Jay D. White. Washington, DC: Georgetown University Press, 1999. 217p. \$65.00.

John A. Rohr, Virginia Polytechnic Institute

This serious, demanding book provides a valuable service to advanced students of public administration and their professors. They will not always like what they read, especially when Jay White tells them they are philosophically naive and scolds them roundly for the poor quality of the research he finds in their doctoral dissertations and in their leading professional journals. Although White overstates the problem, there is just enough truth in his unsettling charges to hurt. Fortunately, there is also enough sound advice on how those inclined to do so may repent their previous transgressions and mend their academic ways.

At the outset, White notes the gap dividing philosophers from public administration scholars and tells us his book "tries to bridge that gap by showing in a clear and understandable way how recent developments in the philosophy of science can help to clarify the logic of research in public administration and related fields" (p. xi). Given the ambitious purpose of his undertaking, White delivers admirably on his promise. His success is due primarily to his impressive command of the literature in both fields and his skillful integration of this literature into his overall argument.

White sets about his task by distinguishing three modes of research—explanatory, interpretive, and critical—and clarifying the differences among them. Distinct as these modes are from one another, he argues that they share a linguistic foundation which researchers ignore at their peril. Hence, the book's title: Taking Language Seriously. This leads to an argument intended to establish the narrative character of this linguistic foundation, which is nicely captured in the subtitle and grounds the main theme of the book, a defense of "the notion of research as storytelling in its explanatory interpretive, and critical modes" (p. 39).

With the salience of storytelling in place, White provides some fascinating examples across a broad range of academic endeavors. The discussions of legal reasoning and literary criticism are particularly well done. Jurists will be impressed by the innovative interpretation of the well-known works of Edward Levi and Benjamin Cardozo. No less impressive are lucid analyses of the writings of Paul Ricoeur and Richard Rorty. All this is undertaken in support of the research-asstorytelling theme.

The author's most illuminating effort is the chapter devoted to the somewhat illusory notion of "action theory." Practicing what he preaches, White tells the story of his own experience as a consultant in an unnamed state's department of social services. His clients, hoping he would recommend the hiring of additional caseworkers, were disappointed to learn that his findings did not support this conclusion. Combining both interpretive and critical modes of research with action theory, White enabled agency personnel to see that the real need was for additional clerical staff to free caseworkers to spend more time and effort on service delivery instead of paperwork.

The book is well organized and well written despite an occasional lapse in grammar or syntax. Particularly helpful are the brief introduction and conclusion in each chapter. They guide the reader through the intricacies of White's closely reasoned text.

The major flaw is White's excessive claim that "all research is fundamentally a matter of storytelling or narration" (p. 6). He makes a good case for the linguistic foundation of research, but the lyrical leap from there to storytelling as the basis for all research relies far too much on assertion and the uncritical acceptance of writings by authors of White's persuasion. Indeed, he seems to assert that storytelling is the

only alternative to the discredited universal laws "in the positivist conception of scientific knowledge" (p. 22). Such a position can be defended only by insisting upon an inordinately broad definition of "storytelling" that would fit the foreordained conclusion but do violence to the meaning of the word and thereby undermine White's case for "taking language seriously."

Specialists in public administration will be disappointed that White does not get around to stressing its character as a "profession" (p. 167) and a "field instead of a discipline" (p. 179) until the final pages of the book. This is too little and too late to parry the thrust that his criticisms of public administration treat it precisely as though it were an academic discipline. Particularly telling are his negative comments on the quality of research in Public Administration Review (PAR). White and the critics whose works he cites seem to ignore PAR's role as the professional journal of the American Society for Public Administration, which is not an academic association. PAR publishes manuscripts submitted by both practitioners and academics. It should come as no surprise that much of the research published in this journal is "applied rather than basic" (p. 1) and, I would add, rightly so. This point is reinforced by the fact that a substantial portion of work in the field is intended to refute extravagant attacks on the administrative state. During the past quarter of a century, public administration has been a field under siege, and its defenders offer no apology for presenting applied research to address the practical problems of the day.

American Politics

Gay Politics, Urban Politics: Identity and Economics in the Urban Setting. By Robert W. Bailey. New York: Columbia University Press, 1999. 396p. \$49.50 cloth, \$22.50 paper.

Kenneth J. Meier, Texas A&M University

The theoretical goal of Gay Politics, Urban Politics is to demonstrate how traditional economic or political economy approaches to urban politics are incomplete, especially when examining gay political issues. Robert Bailey champions identity politics as an alternative framework, not to displace the predominant economic orientation but to qualify and sharpen the economic perspective. Identity politics, the process of defining oneself politically, including an identity as gay, lesbian, or bisexual, still requires an individual or collective interest, but that interest might not be economic. "Changes in status, access, and the distribution of affirming symbols would also be considered as an important policy effect" (p. 14).

To illustrate the argument, Bailey examines the political attitudes, voting behavior, and residential patterns of gays and lesbians. He then follows with four in-depth studies of Birmingham, New York, Philadelphia, and San Francisco, cases selected to illustrate both the range of gay politics in different cities and to cover a collection of different issues, such as redistricting, schools, police practices, and domestic partnerships. The result is a fairly representative cross-section of gay politics at the local level in the United States. Bailey concludes that "sexual identity has challenged several cities" traditional governance patterns" (p. ix).

Gay Politics, Urban Politics has much to commend it. First, Bailey's analysis of the political behavior of gay people uses

an astonishing set of sources (exit polls, census data, mailing lists, membership lists, and so on) in an aggregate analysis. The result is a clever gleaning of information about gays; the method could serve as a blueprint for studying other groups that do not appear in large enough numbers in national surveys. Second, the four case studies are carefully done and well designed. Not all issues can be covered in a single book, but the reader is left with the impression of a serious effort to cover the waterfront and at the same time provide the rich detail of the actual politics in the four cities. Third, the book directly and indirectly sets a research agenda for this area. The clever construction of mass behavior data could be used to distinguish between gays and lesbians. Despite much anecdotal evidence that the political identification process of lesbians differs from that of gay men, systematic assessments are nonexistent. Similarly, taking Bailey's argument about identity seriously, as it should be, suggests returning to methods such as Robert Lane's, with long, in-depth interviews rather than more traditional short survey work.

Bailey does less well with his postpositivist indictment of mainstream political science for ignoring gay politics. Identity is clearly an important concept for anyone studying gay politics. The process by which individuals sort out potential identities and the process by which being gay is both a personal and a political identity is central to understanding gay politics. Despite an extended argument, however, the difference between a positivist use of categories and the identity approach of Bailey is unclear in the chapters on gay political behavior. Jargon often mars the theoretical arguments: "What separates 'identity politics' in gay and gender studies from identify politics as an aspect of the decolonization process is its emphasis on the psychological and constructionist aspects of political conflict and a deep resistance

in gay and lesbian studies to the reification of any category" (p. 23). The argument that mainstream political science has ignored gay politics, in addition, is not as strong as Bailey paints it. Bailey does not cite work on gay politics in mainstream journals by Kenneth Wald, James Button, and Barbara Rienzo (there is a single peripheral citation to their book), Donald Haider-Markel, James Gibson, Barbara Gamble, and M. Kent Jennings and Ellen Andersen. Gay politics is a dynamic, growing field of political science that cuts across numerous subfields. Scholars who ignore this developing literature do so at their peril.

Every scholar of gay politics, and I think every scholar of urban politics, will want a copy of this book. Students of political behavior, although they are likely to be critical of some of the analysis, could benefit from reading about Bailey's efforts to mine secondary data. The book could serve as a text for graduate classes in urban politics, social movements, and identity politics. The philosophy of science discussions might be somewhat heavy for most undergraduates, but the substantive chapters could easily be used in classes at that level as well.

California in the New Millennium: The Changing Social and Political Landscape. By Mark Baldassare. Berkeley: University of California Press and Public Policy Institute of California, 2000. 283p. \$27.50.

Greg Andranovich, California State University, Los Angeles

In a recent essay in the Los Angeles Times (July 9, 2000), Richard Rodriguez notes that times are changing when the newly elected president of Mexico, Vincente Fox of the National Action Party (PAN), addresses a news conference in his country in English, and U.S. presidential candidates Gore and Bush address rallies here in Spanish. What is going on? The changes underlying these remarkable events are documented in Mark Baldassare's California in the New Millennium. The book is an analysis of the California vote in the 1998 midterm election, reported in a policy-oriented style that is very accessible to the interested reader. The analysis is based on a series of focus groups and five voter surveys conducted during the 1998 election cycle (reported in six appendices covering 46 pages), backed up by voting records and other published data.

Baldassare focuses on three trends that have played a confounding role in California politics for some time but may be on the verge of changing how we think about governance issues. These are political distrust, racial and ethnic change, and regional diversity. Following a short introductory chapter to establish the California policymaking context and to describe the data and methods, four substantive chapters describe and analyze the election year data. These provide a wealth of information but are organized in a fashion easy to follow (i.e., description of data and focused political history, election results, policy implications). The climate of political distrust is the first topic and is covered in two chapters, one of which focuses on political disengagement and distrust. These are examined through a discussion of survey and focus group findings on voter turnout, interest in politics, gaining political information, attitudes toward the federal and state government, and attitudes toward government intervention in people's lives.

If voters do not believe in the efficacy of the state's politicians and political institutions, how is policy made? This is the topic of the second substantive chapter on distrust, the

voters' revolt. The rise of the independent voter in California and the use of the ballot initiative to bypass the state governmental process are analyzed historically and in the context of the 1998 election. California's short-lived blanket primary (Proposition 198 was approved by state voters in 1996 but struck down by the Supreme Court after the 2000 primary election in June, after this book was released) is used as an example of the continuing erosion of political parties' importance to voters in California. Baldassare's analysis of the vote on four ballot initiatives (two passed but two others failed; there were 21 ballot initiatives in the June and November elections) highlights the larger lesson of policy unpredictability; the number of ballot initiatives and their complexity has grown, and the interest level and attention by voters has waned.

The third substantive chapter addresses racial and ethnic change, and a prominent place is given to the role of the Latino voter in California. Only one in seven voters in 1998 was Latino, but one in five eligible adults in the state is Latino. Although the Latino voter has been less interested in politics ("nonengaged") than the notoriously uninterested white voter in California, the July 2000 election of the opposition candidate in Mexico, coupled with the changes Baldassare documents, will increase the unpredictability of the Latino vote in the November 2000 U.S. election.

The fourth substantive chapter raises perhaps the most troubling topic for California politics: great regional diversity. Distinct differences based on population composition, growth patterns, economies, geography, politics, and public concerns make it very difficult to develop a public policy consensus. Baldassare notes that the one commonality shared across the state is the high degree of political inattentiveness in the 1998 election cycle, and state elections focused on regional distinctions rather than statewide similarities.

The final chapter begins with eight conclusions that can be derived from California's political history and the 1998 election cycle. Among these are: Reaching public consensus on policy issues will be more difficult if current trends continue; California elections will continue to be dominated by political distrust; initiatives will continue to play a major role in elections and in shaping policy directions; Latino voters will play an increasingly important role in state elections; central valley voters will increase, which will shift the balance of power and policy priorities. Baldassare concludes his analysis with an excellent discussion of ten policy recommendations to combat these trends, and a number of them could lead to beneficial discussions in many other states (e.g., state government should remove barriers and create more programs to encourage registration and higher participation; the initiative process should include legal testing; state and local governments and the private sector should encourage "smart" regional growth).

Overall, the book does a very good job of interweaving the structural changes in California's political landscape with political attitudes and election behavior to show how the public policy process is affected. Although the surveys are for one election cycle (and the raw material is provided in the appendices for critique), the author does an excellent job of contextualizing the 1998 data. A drawback, however, is that a lot of survey information is presented and a lot of comparisons (e.g., 60% to 35%) are made. And the book's relevance suffers from a generic problem of policy analysis, a changing policy environment. The recent Supreme Court decision that struck down the blanket primary might require a more tempered view of the role of the independent voter in California's primary process, just as the recent election in Mexico might require a less tempered review of the nonen-

gaged role of the Latino voter in California. We will know in November.

Finally, one might quibble with certain categorizations (e.g., within-region differences in an area as large as the central valley are masked by aggregating the data), but this book raises provocative questions regarding the relationship among social change, elections, and policymaking, and the uniqueness of the California survey data contribute to a better understanding of the politics of change.

Creative Politics: Taxes and Public Goods in a Federal System. By Glenn Beamer. Ann Arbor: University of Michigan Press, 1999. 174p. \$39.50.

Paul Brace, Rice University

The premise of *Creative Politics* is that federalism matters. It matters for all the familiar reasons. It creates both horizontal (interstate) and vertical (federal-state-local) political relationships that offer opportunities and constraints for policy-makers. It alters the incentives and constraints individuals face when making public policies, circumscribes a domain of feasible alternatives for politicians, and shapes how voters favor or oppose various policies and programs.

Beamer proposes that past models of federalism have been preoccupied with the structures and purposes of this form of governmental organization, but they have devoted insufficient attention to how actors in political institutions view the politics of federalism or capitalize on the political opportunities federalism creates. To rectify this shortcoming, Beamer conducted case studies in eleven state legislatures in 1994 and 1995. He selected these geographically representative states because each was considering changing one of its basic taxes. He conducted open-ended interviews with 123 state legislators and dozens of legislative staffers, lobbyists, local officials, and political activists.

Two core questions anchor the book. First, how do the relationships among institutions at various levels of government, and the levels of competition between each, influence the collective decisions that are reached? Second, how does the public sector's responsibility for the provision of collective goods change the nature of federal politics? His interviews were guided by these questions and a general interest in the perceptions these individuals held concerning taxation, education financing, economic development, health care, and government mandates.

Perhaps the strongest chapter develops a model of representation embedded in federalism that incorporates the nonexclusionary nature of public goods in the representation calculus. The model clarifies representatives' individual governing principles, how relationships among levels of government influence representatives' thinking, and how the nonexclusionary nature of public goods leads to variations in state politics and federal policies. Presumed to be motivated by a desire to improve the benefit/tax ratio of their constituents, representatives structure policy changes within the incentives and disincentives offered by the federal matrix. The dependability, horizontal and vertical transferability, obscurability, accountability, and equity offered by various policy options shape representatives' choices.

The model might strike some as too complex to serve as a useful heuristic. Given so many principles, levels of government, and policy options, it becomes difficult to lay out concrete hypotheses about the conditions under which certain policy choices will and will not be preferred, or why. In

fairness to the author, it would be better to direct this criticism at the nature of the phenomena being studied than to any inherent shortcoming in the model. Beamer has added realism at the cost of parsimony but not without reason. It is sensible to argue that understanding the particular effects of federalism on policy can necessitate consideration of a broad array of factors.

This breadth does not come without a cost. In ensuing chapters the basic thrust of the model is evaluated with interview results, and readers may reasonably wonder whether any or all propositions stemming from the model could be both true and false simultaneously. The anecdotal accounts derived from legislators and others are often colorful and entertaining, but they do not always connect well or serve as convincing evidence in support of some of Beamer's arguments. This is not to say he is wrong, but because of the general looseness of the interview approach, it is not clear what could serve as falsifying information.

Beamer evaluates several major issues in state finance and policy. One seeming enigma concerns why, given the incentives in the federal Tax Reform Act of 1986, states have not adopted their own income taxes. With the exception of Connecticut, no states have instituted new income taxes, and the general trend has been toward an increased reliance on sales taxes. Beamer reasons, not unrealistically, that income taxes are a symbol of big (federal) government; they are paid in large increments and thus are very noticeable to constituents; and those in place became high by virtue of inflation in the 1970s and the resulting bracket creep. Legislators seek taxes with obscure costs to voters and, consequently and despite federal incentives, continue to favor sales taxes over income taxes. This is far less than an ideal outcome, however. for two reasons: Sales taxes may fail to generate sufficient revenues to meet increasing demands for services, and the rise of e-commerce and mail order sales has changed the nature of many sales transactions.

Economic development is another issue Beamer explores. It has come to encompass almost every aspect of state policymaking. Education, infrastructure, and even prison construction are now cast with an eye toward their effects on state economic prospects. According to Beamer, legislators find economic development attractive because it offers the hope of improving the benefits/tax ratios of constituents. Among the legislators Beamer interviewed, virtually all had an opinion if not expertise in the area of economic development. In the literature there is a division concerning whether to promote growth with incentives and tax abatements to industry or with investment in education, training, and improved physical and technical infrastructure. Some legislators choose the particular-benefits (incentives) approach to claim credit for job creation. Others pursue public-goods (investment) strategies to claim credit for the education and infrastructure benefits. Just as the move to sales taxes is arguably far from optimal, it is also true that the obsession with economic development continues unabated despite limited evidence these policies work and some evidence they may harm state fiscal health.

A key component of much of the economic development debate concerns the role of education. Beamer considers public education financing in the states and how choices concerning education provision are linked to the taxes imposed to finance it. Over the past three decades, states have come to play a larger or even dominant role in the financing of education. Much of this change has resulted from drives for property tax reform. Beamer argues that current taxes and tax rates influenced the extent and content of property tax reform and that mechanisms for change (legislative,

judicial, referenda) influenced reform proposals and choices. Ultimately, the shift from local to state financial responsibility raised significant issues concerning accountability, as responsibility for administration remained at the local level. Here again, the drive for less objectionable but still revenue-sufficient taxes seemingly produced an outcome that arguably was less than ideal.

The last policy area Beamer considers is health care. He notes that although the federal government tried and failed to pass health care legislation in 1994, many states had already adopted some kind of reform. In these cases, however, they were largely responding to policies first adopted by the federal government that influenced the parameters of state policies and politics in the first place. When enacted, Medicare reduced demands for state funds for health needs of the elderly and created slack resources that the states would then use to provide matching funds for Medicaid to provide health care for the poor. Medicaid, however, consumed an increasing portion of state budgets, growing from 10-18% of total state budgets between 1987 and 1993 as costs skyrocketed, and Beamer notes that these escalating costs forced legislators to reshape health care policies in the states. By the 1990s, they were promoting reforms to transform the particular-benefits program (Medicaid) into a public good. Efforts to bring subsidized managed care into Medicaid to produce savings that would expand coverage or increase state general-revenue funds were common. Once again, however, we might wonder whether these reformsdriven by the timing and parameters of Medicaid changes, the ability of state politicians and bureaucrats to convince federal officials to grant Medicaid waivers, and the availability of acceptable tax options to fund them were ideal in terms of the breadth and scope of health care coverage.

Long ago Louis Brandeis described the states as laboratories of democracy. The virtue of this arrangement was that, rather than have single policies developed and imposed by a unitary government, the states were free to experiment with different policies, abandoning those that failed and mimicking those that worked. This view of federalism must be contrasted with contemporary concerns about a "race to the bottom"; that is, locked in the grip of horizontal competition and receiving fewer resources from the federal government, states are driven to cut or reduce all programs not directly related to economic development: From this perspective, the states may be agents for change and experimentation, but they choose from a greatly circumscribed array of policy options in the competitive milieu of federalism.

In the end, there is probably an element of truth in both perspectives, and Beamer makes this clear. From reading the book it is evident that states are repositories of considerable creative political energy. These energies, however, are often directed at the issues or constraints imposed by federalism. In each of Beamer's examples, the dominant policy choices made by the states are arguably far from ideal and were developed as much for expediency as efficacy. This is not to say the choices could not have been worse, and it does not suggest that policy change was unresponsive to public sentiment. It is to say that the politics of federalism within states takes place within a domain of feasible alternatives, and often this domain does not intersect with the set of optimal policies. In his descriptions of the tax and policy choices made by state legislators, Glenn Beamer has helped clarify this crucial issue. The book is a good choice for students of state politics, public policy, and federalism.

Race, Money, and the American Welfare State. By Michael K. Brown. Ithaca, NY: Cornell University Press, 1999. 381p. \$55.00 cloth, \$22.00 paper.

Joseph P. McCormick II, Howard University

A sound understanding of the formulation and legitimation of social welfare policy in the United States involves making some sense of the interplay of the following set of factors: presidential initiatives and the guidance that the chief executive receives from his advisors; congressional responses to those initiatives (and the partisan and regional features that influence those responses); fiscal policies tied to capital accumulation, national economic growth, and deficit control; interest group politics electorally linked to the aforementioned congressional responses; and racialized decision making, which in America tends to taint directly or indirectly all the aforementioned factors. A number of scholars have given analytical attention to some of these aspects in their efforts to understand the evolution of American social welfare policies (e.g., W. J. Wilson, The Truly Disadvantaged, 1987; Hugh Graham, The Civil Rights Era, 1990; Jill Quadagno, The Color of Welfare, 1994; and D. C. Hamilton and C. V. Hamilton, The Dual Agenda, 1997), but few have attempted to weave all of them together and describe national social welfare policies across time, from the inception of the welfare state in Franklin Roosevelt's New Deal through the first Reagan administration. This is the carefully documented task that Michael Brown, a political scientist, takes on in this wellwritten book.

Brown mines a multidimensional database: the extant literature on various dimensions of the American welfare state; archival material from the files of presidents Roosevelt, Truman, Kennedy, Johnson, and Nixon; records of the Bureau of the Budget, the Social Security Board, and the Works Progress Administration; the papers of the National Association for the Advancement of Colored People and the National Urban League; the George Meany Memorial Archives (which provide a range of perspectives from organized labor); doctoral dissertations on various facets of this complex topic; congressional hearings; and interviews with offlcials who served in both the Johnson and Nixon administrations. Such a database provides ample material for telling a story with which one must be familiar in order to appreciate fully the sort of welfare reform that occurred in this country toward the end of the twentieth century.

Brown's able historical analysis takes us from the first administration of Franklin Roosevelt (chaps. 1 and 2), when the American welfare state was created; through the administrations of Harry Truman (chap. 3) and Lyndon B. Johnson (chaps. 6, 7, and 8), under whom the welfare state grew and became more redistributive; to the first administrations of Richard Nixon (chap. 9) and Ronald Reagan (chap. 10). The GOP "laid the foundations for an assault on the [American] welfare state and ushered in the collapse of New Deal liberalism" (p. 352). That assault resulted from the outcome of the 1994 congressional elections, which mark the temporal stop sign of this book.

There is a useful discussion of universalistic social policies (which are more common in Western Europe and which tend to diminish class and racial differences) versus those that tend to be means tested (which are more common in the United States and which tend to accentuate both class and racial differences), theoretically grounded in the work of Joseph Schumpeter (none of which is cited in the book) and Gunnar Myrdal (whose seminal work, *The American Dilemma*, 1944, demands acknowledgment among those who write about racial issues in American politics). Brown then singles out for

analytical attention "the strategic choices of policymakers (chiefly the presidents from Franklin Roosevelt to Richard Nixon) and the self-interested actions of the main advocates and opponents of a federal controlled welfare state" (p. 17). These are major factors that aid our understanding of the "variegated pattern of social [welfare] provision in the United States" (p. 17). These policy choices, the author tells us, were shaped largely but not entirely by money and race. Both these factors, perhaps more than others, shaped the size and scope of the American welfare state as well as its beneficiaries. In this regard Brown reminds us: "It is the capacity to tax and spend that determines what kind of welfare state emerges" (p. 6). In fact, "questions of fiscal capacity—the ability of policymakers to raise [the] revenues necessary to finance new policies to spend—lie at the center of the political development of the American welfare state" (p. 7).

If fiscal capacity lies at the center of the political development of the American welfare state, then racial stratification and its attendant behavior component, racialized decision making, are core and contextual factors that contribute to its programmatic content and operation. Far too many scholars who deal with various aspects of America's multidimensional racial dilemma use "race" in a vague and uncritical manner. Historically, the use of this concept has tended to direct analytical attention to "race relations" between blacks and whites. Brown's discussion goes beyond such a superficial distinction and leads him to a systematic examination of two key components of racialized decision making: racially motivated exclusion, which was more common in labor markets during the administrations of Roosevelt and Johnson, and the exploitation of racial differences, which was a key element in policy decisions of the Nixon administration. Racially exclusionary public policies in various arenas of American society have been declared unconstitutional and offset by more ameliorative policies that emphasize racial inclusion, but the legacy of racial stratification in the now dissolving American welfare state and reproduced over time by the policy choices of various presidential and congressional policymakers nonetheless lingers. In at least three places in this book-chapters 5, 8, and 10-Brown provides some detailed discussion of how racialized decision making, along with a number of other factors, shaped the content of social welfare policies.

The conceptual linchpin in Brown's analysis is his notion of "truncated universalism" (chap. 3). This term is used to describe the sort of welfare state that had emerged by the time of the Truman administration. Political circumstances and efforts by organized labor as well as tactical concessions by owners of capital (chap. 4) led to a particular pattern of social provision: "a welfare state that mingled the equalitarian promise of universal social insurance [envisioned by the New Deal but never fully realized] with a fragmented assortment of public policies (veterans and housing programs) and private social policies (health and pension benefits subject to collective bargaining)" (p. 100). Brown points out that the de jure distinction established between social insurance (e.g., Old Age and Survivors' Insurance, aka: social security) and public assistance (e.g., initially Aid to Dependent Children, which by 1950 had become Aid to Families with Dependent Children; Old Age Assistance; and Aid to the Blind), established by the Social Security Act of 1935 become racialized, such that less well-off African Americans came to be proportionally overrepresented within the public assistance component and proportionally underrepresented in the social insurance component.

The racialized character of the American welfare state, Brown's analysis indicates, was a reproduction of the class and racial stratification that existed in the larger society and out of which the American welfare state emerged. During the New Deal period, racial discrimination, particularly in the South, was legally sanctioned in the labor and housing markets as well as electoral politics. Some of the principal legal architects and defenders of this apartheid system, southern congressional Democrats, occupied key chairmanships in both the House and Senate and could veto social welfare initiatives that even remotely suggested a transformation in the American racial hierarchy. Racial patterns set in place at this time tended to become further entrenched by fiscal policies initiated by various presidential administrations, congressional responses to those initiatives, and interest group behavior either in support of or in opposition to the programmatic content and direction of social welfare policy.

Although the Personal Responsibility and Work Opportunity Act of 1996 is not singled out for treatment (it receives but one sentence, late in the book, and is not specifically mentioned by its legislative title), an observation that Brown offers in the final chapter underscores why I think this book is of value to students, policy practitioners, and scholars who wish to have a better understanding of the nature and evolution of social welfare policies in the United States: "The history of race and social class [and the fiscally circumscribed decisions of various presidential administrations] in the development of America's welfare state . . . provides a critical perspective on these arguments and a basis for assessing our contemporary predicament" (p. 360). A sound understanding of the present must begin with the kind of analysis of the past that Brown provides in this provocative piece of scholarship.

Driving Forces: The Antomobile, Its Enemies, and the Politics of Mobility. By James A. Dunn, Jr. Washington, DC: Brookings Institution Press, 1998. 230p. \$44.95 cloth, \$18.95 paper.

Michael N. Danielson, Princeton University

America's enduring romance with the automobile is one of the great love stories of the twentieth century. It is a tale of fulfillment for many, disappointment for some, and enormous change for all. More than is the case with most components of the American political economy, automotive transport has been a public-private partnership in which government's role has been central. From the dawn of the automobile age, government has provided the right of way for cars, trucks, and buses, investing billions of dollars in highways, streets, traffic controls, and other essential infrastructure. James A. Dunn examines the interplay of industry, government, and the public in *Driving Forces*.

Dunn's work has much to commend itself for political scientists. The author underscores the tremendous political appeal of the automobile, which derived primarily from the enhancement of personal mobility and freedom of movement. The financing of public roads through "user taxes" ensured highway agencies a source of funding that is widely perceived as legitimate. Dunn properly emphasizes the critical interplay between automotive transport and urban development that created the sprawling low-density contemporary metropolis, an urban form utterly dependent on automobile and highway, serving a population that resists efforts to limit freedom of movement or increase the costs of automotive use. Dunn grounds his analysis in the interest group literature as well as Anthony Downs's issue-attention cycle, the work of Paul Sabatier and Hank C. Jenkins-Smith on advocacy coalitions, and James Q. Wilson's client-interest group-majoritarian-entrepreneurial politics typology.

Dunn examines a number of policy arenas: highway finance, air pollution, safety, sprawl, and rail alternatives to the automobile. At times, this analysis bogs down in minutia, particularly legislative maneuvering, that obscure the broader political and policy trends. Dunn also largely ignores a critical element of automotive politics: road building and the shift of power from road builders to affected publics. He properly emphasizes the importance of the responsiveness of politicians to their auto-loving constituents, but many of these same constituents resist highway construction projects that would adversely affect their communities. A study that relies so heavily on public acceptance in explaining the dominance of automotive transport would be more persuasive if the analysis encompassed adverse public reaction to highways and the responses to it, which have restricted road building and increased the costs of those roads that are built.

The failure to treat public resistance to roads is symptomatic of a larger problem with this study. Dunn seeks to do more than explain automotive politics and policy; his goal is to discredit those who advocate alternatives to automotive transport, especially environmentalists who worry about air pollution, planners troubled by sprawl, and promoters of urban transit and intercity passenger rail service. These policy vanguards, as he terms them, undervalue the automobile and its attendant freedom and mobility, they overestimate the dangers of sprawling urban development and global warming; they promote expensive and inefficient rail transit schemes; and they propose land use and travel regulations that are anathema to the American ideals of freedom and mobility.

The problem is not that Dunn is off the mark in much of his critical appraisal of the automobile's "enemies" or in his negative assessment of the political prospects of most of their policy agenda. He provides ample documentation of the policy shortcomings and political weaknesses of environmentalists, planners, transit advocates, and auto haters. But Dunn's desire to discredit the critics leads him to understate the importance of the costs of automotive transport and thus the necessity for more far-reaching changes than the mild incrementalism advocated here. Sweeping changes already have occurred in the policy system—highway trust funds are used for other modes, regulation has reduced automobile emissions, foad building must accommodate community values and NIMBY concerns—changes that are not adequately explained by Dunn's paradigms. Public views about the automobile need to be disaggregated: The American love affair does not extend to a public embrace of air pollution, unsafe vehicles, or highways in one's backyard. As valuable as this study is in providing a comprehensive political analysis of the contemporary struggle over automotive policy, the model is too static to explain some of the most important policy and political changes that have occurred over the past quarter century, or those that seem certain in the not-too-distant future as congestion increases, pollution worsens, and the earth gets warmer.

The Choices Justices Make. By Lee Epstein and Jack Knight. Washington, DC: Congressional Quarterly, 1998. \$21.95 paper.

Pablo T. Spiller, University of California, Berkeley

Choices questions the presumption, prevalent among many law and political science scholars, that decision making by justices is based either exclusively on legal issues (as assumed by the traditional legal profession) or exclusively on ideological tendencies (as suggested by the attitudinalists). Instead, the book's premise is that justices are—as humans—strategic,

and their strategies are conditioned by their own preferences, the preferences of their competitors (other justices and other institutional players, such as Congress), by institutions (internal and external to the Court), and by public opinion.

The strength of the book is its systematic analysis of this issue using both the Court's public records and data from the private papers of justices Brennan, Douglas, Marshall, and Powell. (The data used in *Choices* are available in http://artsci.wustl.edu/~polisci/epstein/choices/.) The authors combine basic statistical analyses with direct citation from the private papers. Along the way, one tends to be convinced that justices are, indeed, strategic players. *Choices* shows that strategic behavior and thinking permeates the Court throughout its opinion production process, all the way from the *Cent* conference, to the negotiations on the opinion writing, to the writing of concurrences or dissents.

The book lacks, however, an analytical framework to explain when strategic considerations within the Court are important. Let me start with the median voter theorem, which specifies that with Euclidean preferences, a single-dimension policy space, and no agenda-setting powers, the median voter's ideal point is the only equilibrium. No more assumptions, strategic or otherwise, are needed. Once the median policy is proposed, no other proposal will beat it, and it becomes the outcome.

Consider, now, voting within the Court. The only agenda power of the chief justice (or the senior justice in the majority) is the assignment of who will write the opinion, but, as Choices shows, other justices make all sort of suggestions (proposals) through memoranda, conversations, and concurrences. In principle, then, the agenda power in the Court is minimal at best. When issues are single dimensional, a majority should not be that hard to form, in principle, and should include the "median" justice on that dimension. One can understand the communications among justices as simply preference revelations, so as to figure out who is the median justice and, as a consequence, what is the median outcome. Indeed, unless it is expensive to write an opinion, an issue never formally addressed in this book, the final opinion could not differ from the median justice's preferred opinion. If there is any difference with her preferred outcome, she would write such an opinion and garner a majority. Asymmetric information about preferences, however, does not trigger strategic behavior in the absence of writing costs. Justices simply reveal their preferences, and the median justice's preference prevails. (In multiple-dimension issues, the median may not be properly defined. But observe that for most cases facing the Court, the policy space resembles more a lattice than a compact set. As a consequence, stable majorities could form, although not necessarily.)

Consider the vote in Craig v. Boren over the appropriate test to assess sex-based classifications (chap. 1), a case to which the authors refer throughout the book. Essentially three options were presented to the Court. The strict scrutiny standard was advanced by Craig, the plaintiff; if adopted, the Court would almost never uphold a sex-based classification. Oklahoma advanced the rational basis standard; if adopted, the Court would almost never reject a sex-based classification. The ACLU, as amicus curiae, proposed something in between, a test now called heightened scrutiny; if adopted, the Court could strike down some statutes and uphold others. At conference the Court was divided. Justices Burger, Stewart, Powell, and Rehnquist favored the rational standard, justices White and Stevens preferred something in between, justices Brennan and Marshall favored the strict standard (although Brennan suggested that an in-between standard would be a viable solution), and Justice Blakmun was undecided but leaned toward Craig. On the merits only, Rehnquist leaned toward Oklahoma, and all others leaned toward Craig.

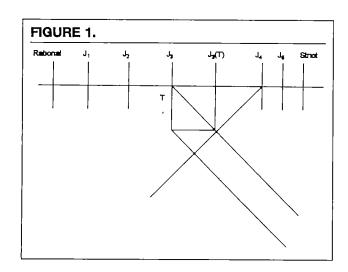
Once the case is granted Cert, then, the outcome will have to favor Craig, preferred by eight out of nine over Oklahoma. But the Craig position cannot be reconciled with the rational standard. A decision setting a standard would have to rule for either the in-between or the strict scrutiny standard. But the latter will not win, as six out of nine prefer something else. At conference a majority was formed by justices Brennan, White, Marshall, and Stevens. The first three advocated a strict standard, but Stevens preferred something more than rational but less than strict, in other words, something in between. Brennan assigned to himself the writing of the case. He cannot expect to get a majority with a strict standard, and those who advocate the rational standard cannot obtain a majority. Simple majority voting suggests that a middle-ofthe-road outcome is the equilibrium. Indeed, the median voter theorem says that a policy representing those who want something in between (such as the ACLU's standard) will be the equilibrium decision. Brennan cannot write anything too different from such a decision, as someone else would make a proposal (probably someone who is not in the majority) that upheld Craig but set a standard in the middle.

The interesting question is not why Brennan (and several other justices) voted for something contrary to his stated preference, but why he assigned to himself the writing of the case (p. 9) rather than let someone else write it. To understand this, one has to bring opinion writing costs to the analysis.

Assume that writing an opinion imposes a cost of T on the writer. Assume that the utility function of justice, j, is given by $V_j(x,I_j) = u(|x-x_j|) - I_j^*T$, where x is the policy being voted on; x_j is the ideal point of justice, j; and I_j is a variable equal to 1 if j writes the opinion and to 0 if she does not. See Figure 1.

With no agenda setting, justice 3 is indifferent between writing his own opinion in favor of in-between standard, J_3 , or letting any other justice write an alternative opinion, as long as the in-between standard is no farther apart than is $J_3(T)$ from his ideal point. $J_3(T)$ is the policy to the right of J_3 , which leaves 3 indifferent between writing the opion or letting someone else write an opinion to the right of J_3 .

Assume that J_5 is the justice with the ability to say who will write the opinion. Then the justice will assign justice 4 (or himself) to write the opinion. Justice 4 knows that she cannot set the in-between standard farther to the right than $J_3(T)$,



because justice 3 will offer a decision at J_3 , or even justices 1 and 2 will offer a decision to the left of J_3 . Thus, justice 4 will give an in-between opinion at $J_3(T)$. (Justices to the left of 3 could think about offering a similar opinion to the left of 3 [i.e., a little closer to J_3 than to $J_3 - 2J_3(T)$], but if they are forward seeking, they know that justice 4 will match that by offering a decision just to the left of $J_3(T)$, until the final outcome becomes J_3 , but the writing cost is incurred not by 3 but by either 4 or perhaps 2. In order for this not to be an equilibrium, it has to be the case that $V_2(J_3,1) \leq V_2(J_3(T),0)$. If V(.) is linear, the equality holds, and there is no gain for 2 to undertake the action.)

We see that the power of agenda setting is in that 3 will vote with justices 4 and 5 for an in-between decision that is not his most preferred position. Similarly, justice 4 will write an in-between decision different, also, from her most preferred decision. But $J_3(T)$ is the unique equilibrium given that 5 (or 4) has the power to assign the writing of the decision.

Observe that the distance between $J_3(T)$ and J_3 depends on T, the writing costs, as well as on the saliency of the issue (the slope of u(.)). The larger is T, the farther apart is $J_3(T)$ from J_3 ; the higher the saliency, the closer is $J_3(T)$ to J_3 . Writing costs are a function of the complexity of the case, the urgency of the case, and the shadow value of time of the justices and their staff. The more crowded the docket with important cases, the higher is the shadow value of time and, hence, the greater the agenda power of the justice who assigns case writing. Strategic behavior becomes important in complex and urgent cases as well as in periods when the docket is full. But an important result of this framework is that the more salient the case, the less important is agendasetting power and the greater the power of the median justice.

Coming back to Craig v. Boven, it now becomes clear that Brennan's writing was more toward the strict rationale than justices Stevens or Blackmun would have preferred, but for them to garner their own majority would imply writing an opinion from scratch, a cost they seemed unwilling to pay. Why did Brennan assign the case to himself rather than give it to White or Marshall? In principle, if either of these two justices had written the opinion, the outcome would have been similar, and Brennan would have saved himself some time. It could well be that White and Marshall were extremely busy (or the case was not too salient for them), so they would produce a decision not very palatable for the median justices (Stevens and Blackmun). That could have triggered a round of alternative opinions which would end with a decision closer to the position of Stevens and Blackmun than Brennan desired.

In summary, *Choices* is an extraordinary and stimulating book. It is a most welcome addition to our literature.

Why Americans Hate Welfare: Race, Media, and the Politics of Antipoverty Policy. By Martin Gilens. Chicago: University of Chicago Press, 1999. 296p. \$25.00.

Joe Soss, American University

Why Americans Hate Welfare is a lucid and provocative investigation of how Americans think about poverty and what they believe government should do in its efforts to help the poor. Readers who hope to find a compelling explanation for hostility toward public aid will not be disappointed. In fact, this carefully argued study delivers far more than its title

promises. Sifting through a welter of poll results, Gilens shows that "much of what is commonly believed about Americans' attitudes toward welfare is wrong" (pp. 1–2). His account of public opinion is nuanced, informative, generous, and in places unsettling. It is also the point of entry for an incisive analysis of how race has come to pervade and distort poverty politics in the United States. Few books, if any, have done so much to demonstrate how racial stereotypes shape the treatment of poverty in mass media, public policy, and the minds of citizens.

Most readers will not be surprised to find that Americans tend to dislike "welfare" (understood as cash assistance to the able-bodied poor) and distrust welfare recipients. The intriguing premise of this book, however, is that such negative attitudes must be understood and ultimately explained as anomalies. Contrary to conventional wisdom, Americans generally do not reject social provision as a violation of liberal individualist values or as a threat to their economic self-interest. By and large, they endorse efforts to assist the poor and support the modern welfare state. Most Americans say they want government to do more for the poor and are willing to pay higher taxes to help those in need. They tend to support nonuniversal programs targeted at poor people, and their commitment to public aid does not flag when economic hard times might be expected to curtail generosity.

Why, then, does welfare evoke such deep hostility? In part, Gilens suggests, individualist values shape moral distinctions between the deserving and undeserving poor: American support for public aid tends to wither when recipients are perceived as lacking a commitment to hard work and personal responsibility. Viewed in this light, the question of why Americans hate welfare must be recast as the question of why welfare recipients, virtually alone among the beneficiaries of social programs, are seen as undeserving freeloaders. The brief answer, elaborated throughout the book, is that white Americans mistakenly believe that most welfare recipients are black and that black people lack sufficient commitment to the work ethic.

Gilens's careful dissection of how and why racial beliefs have come to exert so much influence over welfare attitudes is a first-rate piece of political analysis. Despite dramatic changes in racial attitudes over the past forty years, Gilens demonstrates that the old stereotype of black laziness persists. Drawing on survey data and a clever survey experiment, he also shows that this stereotype leads many whites to view welfare recipients as undeserving, withdraw support from welfare spending, and prefer get-tough approaches to welfare provision. By focusing tightly on the stereotype of black laziness, Gilens cuts a distinctive (and very productive) path between those who dismiss the importance of race and those who emphasize more global orientations, such as antiblack prejudice or the defense of group interests. In addition, he raises the critical question of how racial stereotypes and images of the poor have become so intertwined in the public

Gilens provides a rarity even among very good studies of public opinion: an empirically grounded historical account of the political and cultural forces that have shaped contemporary attitudes. Based on a content analysis of major newsmagazines (1950–92), Gilens concludes that a potent tie between race and poverty was forged in the mid-1960s against a backdrop of black northern migration, increased welfare participation, a civil rights movement demanding economic equality, and the turbulence of urban riots. In contrast to earlier periods in which black poverty received little attention, "from 1967 through 1992, blacks averaged 57 percent of the poor people pictured in [poverty stories]—

about twice the true proportion of blacks among the nation's poor" (p. 114). Since the 1960s, pictures of whites are significantly more likely to accompany sympathetic poverty stories; pictures of African Americans predominate in periods of hostility toward welfare and in stories that are less sympathetic toward the poor or focus on more stigmatized subgroups of the poor.

Although Gilens introduces all the proper caveats regarding causation, the final chapters of the book offer a variety of good reasons to suspect that decades of systematically biased media images have had important political consequences. Gilens first turns to psychology for insight into how media images and racial stereotypes influence public responses to poverty, and he then reviews policy analyses to show that welfare provision follows a racial pattern in precisely those programs that the public links to African Americans. The book concludes with some insightful reflections on contemporary welfare reform and the future of welfare provision in the United States.

As in any book, some issues receive less attention than they perhaps deserve. Gender, for example, rarely enters into the analysis. This omission strikes me as important, given the prominence of sexual, reproductive, and familial issues in welfare discourse. Stereotypes of poor black women extend beyond laziness to include a variety of equally stigmatized behaviors. By devoting more attention to these issues, as well as the ways that work expectations for women have varied across racial groups and over time, I suspect that Gilens might have strengthened his analysis of how racial stereotypes fuel perceptions that welfare recipients are morally undeserving.

To focus too much on this omission, however, would be a mistake. Why Americans Hate Welfare is an extraordinarily good piece of scholarship. Researchers will find much to admire in Gilens's skillful analysis, careful attention to problems of inference, and efforts to address squarely alternative explanations. Teachers should take note that this book's accessible style, clear primers on theory and previous research, and wide range of thought-provoking claims make it ideal for assignment in undergraduate courses. Carefully researched and cogently argued, this book should be essential reading for scholars in a variety of fields.

Organizational Report Cards. By William T. Gormley, Jr., and David L. Weimer. Cambridge, MA: Harvard University Press, 1999. 272p. \$39.95.

B. Guy Peters, University of Pittsburgh

Despite all the changes implemented within the public sector during the past several decades, accountability remains the central political question for public administration. Gormley and Weimer identify an important instrument for accountability that has gained prominence over the past few years and demonstrate its potential effects on management and policy. The organizational report card compares the performance of one organization with that of others doing the same or similar tasks. The information can then be used to improve the organization, either by changes in internal management or through external pressures from clients, the public, and legislatures. In general these report cards are made public and can be the impetus for political debates on the performance of the public sector.

The authors do an excellent job of isolating the ways in which report cards have been developed and used in a number of policy areas in the United States. They focus primarily on health care and education, especially the ways in

which schools and hospitals are assessed, but also examine such areas as job training and environmental policy. They point out the difficulties of measuring many tasks in the public sector and of attributing differences in performance—especially for final policy outcomes—to the actions of individual organizations. The authors are also sensitive to the increasing role of private and third-sector organizations in providing public services and to the even more tenuous linkages that may exist for performance in those settings.

Despite the many virtues of the book, a number of questions are not addressed adequately. To some extent these flow from the failure to anchor the analysis and findings in the burgeoning literature on performance and quality in government. As the administrative reforms initiated during the 1980s become more institutionalized, they appear to be consolidating around the issues of measurement and how to attribute success and failure to individual organizations (see Christopher Pollitt and Geert Bouckaert, Public Management Reform: A Comparative Analysis, 2000). These—and especially the measurement issue—are touched on too lightly here. Furthermore, the relationship of policy and program design to individual organizational performance is largely dismissed, which means there is more focus on management than on the larger array of factors that may produce different outcomes.

Without an anchor in the literature, the discussion appears rather parochial. Perhaps more than any other country, the United Kingdom (Christopher Hood, Regulation Inside Government: Waste Watchers, Quality Police and Sleaze-Busters, 1999) uses report cards and other means of rating the performance of public organizations, but that experience is hardly referenced at all. Beginning at least with the Major government and the Citizen's Charter (in reality a consumer's charter for public programs), there has been substantial emphasis on measuring performance and using that information to improve the quality of programs. This has increased under the Labour government, as organizations are "named and blamed" for poor performance.

Not enough emphasis is given to the role of politics and political forces in the construction and use of rating instruments. Most of the issues raised by Gormley and Weimer, for example, stress the technical aspects of implementing reporting systems rather than their political ramifications. There is relatively little discussion of how these indicators of performance feed back into the policy process, although the social construction of performance seems to be a crucial aspect of the politics surrounding contemporary governing. Likewise, little attention is given to other changes occurring in the same policy areas. For example, at the same time that report cards are becoming important as measures of accountability for education, such initiatives as vouchers and charter schools are reducing direct public sector control over education. This apparent contradiction, and others like it, would be extremely useful to explore, but this volume does not do so.

Performance and quality in the public sector are not only crucial elements in restoring public confidence but also are important components of institutional politics. The Government Performance and Results Act, for example, tends to shift power toward Congress and away from the president. Likewise, the Labour government's Best Value program in the United Kingdom tends to centralize power in London (Paul Gosling, "An Inspector Calls." Public Funance 24 [March 1999]: 5–9) and may substitute political judgments for professional judgments. Furthermore, report cards to an important extent reshape the nature of program accountability. Accountability becomes more populist, but it also becomes more focused on average performance within the

organization rather than exceptional events that can be used to embarrass political leaders. The shift has important political implications that are not explored sufficiently in this book.

Gormley and Weimer offer an extremely useful discussion of organizational report cards and how they can be employed in the public sector. Their research is impressive, and the writing is exceptionally lucid. The crucial missing element is the political dimension of this rating instrument and the way the information is fed back into the policymaking and political process.

Something Within: Religion in African American Political Activism. By Fredrick C. Harris. Oxford: Oxford University Press, 1999. 227p. \$35.00.

Allison Calhoun-Brown, Georgia State University

Within the study of African American politics there has been a debate over whether religion is the opiate or the inspiration for political activism. Those who suggest that it is the opiate focus on the otherworldly nature of much of black religiosity. Those who suggest that religion is the inspiration focus on the institutional strength of black churches and the centrality of black ministers in the African American community. Something Within moves beyond this debate by considering both religion and political activism to be multidimensional constructs. Instead of focusing only on churches or the political influence of ministers, the book highlights the complexity of the relationship between religion and politics among blacks by exploring the ways that both individual and organizational religiosity can serve as important resources for various aspects of political mobilization.

Although the multidimensionality of religion has been appreciated by religion and politics scholars for many years, this understanding has rarely been applied to studies of African American politics, which has resulted in the simplistic kind of either/or thinking about religion and politics in the black community that is reflected in the opiate-inspiration debate. The literature has not significantly informed this debate because much of its emphasis has been on the important and notable differences among various faith traditions instead of the myriad ways that different types of religiosity can affect politics within a faith tradition. Using both qualitative and quantitative methods, Harris does an excellent job of addressing these omissions by exploring how religion can act separately as an organizational, psychological, and cultural resource in the political mobilization of the black community and how these religious resources can have differing effects depending on both the structure of political opportunities and the types of political activities involved.

Something Within makes a number of important contributions. It offers a theoretically rich framework for studying religion and political behavior among blacks, which has been sorely lacking in many studies of this type. Harris does not provide the standard "civil rights movement" treatment of the intersection of race, religion, and politics, which highlights the organizational influence of the church as an institution or focuses at the elite level on ministers as charismatic leaders. Instead, he concentrates on how individuals are motivated to participate politically through religion. Although the research confirms that religious institutions within the African American community play an important role in facilitating black political mobilization by providing such things as meeting facilities, access to communication networks, political forums, endorsements, and fund raising, it

also establishes that the effect of religiously based resources extends far beyond these.

Harris points out that religion works as an important political resource both through and independent of black churches. Internal religiosity or private devotionalism enhances significantly the psychological resources associated with political participation, including religiously inspired political efficacy and interest in politics. One of the most significant political effects occurring through black churches as organizations is the opportunity for involvement that they provide. Those who attend church are likely to vote, but those who are active in church work are also likely to engage in many other forms of community-oriented collective action. Harris also stresses that the "mass response to mobilization is more complex than the psychologically and organizationally centered models of activism convey" (p. 136). The analysis adds to these models the importance of religious culture in explaining how political actors are able to construct meaning and frame individual and collective political actions.

Another major contribution of this book is that it incorporates the literature in such a way that it improves our understanding not only of African American politics but also of the importance of religion in American politics. Harris is particularly insightful with regard to how churches as voluntary associations influence the development of civic culture. In Almond and Verba's classic work (The Civic Culture, 1963) religion was not specifically identified as being significant to the creation of civic culture, and thus the civic dimensions of religion went largely unexamined. Yet, as Harris explains, "one would expect the religious dynamics of civic associations to stimulate citizen activism by giving church members the opportunity to learn and practice civic skills through their religious institutions. Thus, as thriving components of civil society, religious institutions—and the legitimizing values they promulgate—should complement civic culture in the United States" (p. 38). This research suggests system-supporting linkages among religion, civil religion, and civic

System support describes only part of Harris's theory of the role of religion as a political resource. History and much of the social movement literature clearly highlight the integral role of black churches and religious culture in encouraging people to challenge racial injustice. Black religion affirmed black people in times when the social, political, and economic system in the United States tried to dehumanize and demotivate them. Religion provided a means for blacks to challenge their marginality in society. Harris's theory of religion as a political resource is novel in that it incorporates both the system supporting and system opposing components of the black religious experience. He calls the dualistic orientation of the culture and institutions of dominated groups an "oppositional civic culture." Simultaneously, religious institutions have helped "develop positive orientations to the civic order" and provided "African Americans with material resources and oppositional dispositions to challenge their marginality through modes of action and thought that call for inclusion in the political system instead of exclusion from the polity" (p. 40). This finding highlights Harris's point about the need for a multidimensional approach to the study of both religion and political action because of the need to identify which social "norms and institutions lead to loyalty to the polity and reformist opposition and which norms and institutions undermine civic loyalty" (p. 68).

One of the most interesting discussions examines how the religiously based oppositional civic culture has had both positive and negative consequences in African American politics and society. Although it has provided the means to

resist racial oppression and has provided support for the civic order, its patriarchal nature, autocratic structure, and theocratic norms can undermine democratic processes. Harris's chapter on religion and gender in African American political life is particularly perceptive on these points.

Finally, this book is a fine example of how ethnographic and quantitative research methods can be combined to tell a full, rich story. Using multiple methods and multiple data sources, Harris writes a highly academic, theoretically insightful book that is also an interesting read. In the process he successfully reveals much more about the nature and complexity of the relationship between religion and politics in the African American community than has been appreciated by scholars before.

Campaign Talk: Why Elections Are Good for Us. By Roderick P. Hart. Princeton, NJ: Princeton University Press, 2000. 328p. \$29.95.

Kelly D. Patterson, Brigham Young University

The United States has just concluded another presidential nominating season. Judging from television ratings for the conventions, comments of pundits, and public opinion polls, America's utter disdain for presidential campaigns seems to be in full swing. Amid all the criticism and apathy, I had the distinct pleasure of reading this book. It mounts a vigorous defense of campaigns and the language used to conduct them. It undertakes this defense with an impressive array of data and theory and makes an expansive argument about the important functions performed by modern presidential campaigns.

The book relies on a unique source of data. As part of the Campaign Mapping Project, Hart and his project codirector, Kathleen Hall Jamieson, have performed the herculean task of collecting the campaign speeches of presidential candidates, news coverage, and letters to the editor written by average citizens. Most of the material is for the campaigns between 1948 and 1996. With a powerful computer program (DICTION) that analyzes the types and amount of words, Hart explores the language used by presidential candidates, reporters, campaign ads, and the citizens who express their opinions about politics. The result is a large data set that reflects the ways in which different components of a presidential campaign use language to communicate with one another.

The analysis of language is perhaps the most controversial aspect of Hart's project. The DICTION program breaks the transcripts into 500-word segments and then counts the frequency with which particular words occur. The program also segments the language into five main variables: certainty, optimism, activity, realism, and commonality. The research then establishes comparisons between the various participants in campaigns and how each uses language along those variables over time. Critics may scoff that a computer cannot capture the subtleties of language and context and that mere quantification misses the meaning conveyed by campaign' communications. Hart anticipates such criticisms and moves the reader easily through the logic of his research design and the assumptions it makes. Certainly, a computer count does miss some aspects, but it does not miss all. Indeed, very few measures of campaign communication are perfect (I am not aware of any that are). Those who study elections know that tradeoffs must be made when constructing measures that purport to explain and understand the effects of campaign communications. It is fairly easy to assess the author's

assumptions about the measures and the analysis because Hart clearly confronts the limitations of the design.

The findings detailed in the book (too many to list in a short review) can be either heartening or sobering, depending on one's perspective; all are interesting. For example, Hart reports that various themes (e.g., God, country) have been stressed consistently in politics since 1948. He also finds that political speech is less certain than it was in 1948. Voters, politicians, and the press are all more likely to use a language that conveys less assurance about what to do and how to do it. He also finds that the language used by all three actors is increasingly personal. It is an age in which a presidential candidate will answer the question: "Boxers or briefs?" Hart provides stark confirmation that the United States has clearly embarked on a new era in politics. In comparing the voices of politicians to those of the press and the people, Hart finds that "candidates address political initiatives, the press concentrates on political actors, and the electorate emphasizes political communities" (p. 202). These findings are not necessarily unexpected, but they are noteworthy because of the role that each entity plays in a campaign. The differences in voice endure over time, which ensures that campaigns provide the country with a conversation about the type of leaders who can provide solutions to problems that individuals find the most relevant to their communi-

There are two minor points with which a student of campaigns may want to quibble. First, the book does not try to establish a direct link between campaign rhetoric and its specific effects on public opinion or some other part of the political system. Hart seeks to identify the voice of the politicians, the press, and the people and how those voices have evolved over time. It is not an attempt to assess systematically the extent to which these changes may have precipitated effects in some other arena (e.g., legislation, shifts in public opinion, party fortunes). Second, the defense of campaigns at the end of the book applies mainly to the presidential race. Congressional elections, specifically those for the House, might not benefit from Hart's defense. Many congressional elections suffer from an acute lack of competitiveness, whereas presidential elections always have two strong candidates who establish voices and emphasize themes that they hope will resonate with voters. Congressional elections often have only one significant candidate who seeks to have a conversation with the electorate, and they rarely receive the same kind of media coverage as the presidential race.

The best that can be said about any book is that it is thought provoking. This book certainly merits such a description. At a time when so many segments of American society question the value of political campaigns, often on dubious grounds, Hart provides a thorough and cogent defense of presidential campaigns and the rhetoric they spawn. His theoretical insights range over a variety of topics and raise broad questions about democracy and culture. Perhaps the book appealed to me because I read it in the heat of a presidential campaign. I found it easy and helpful to apply the categories developed in the book to the political spectacle I witnessed on the news. Yet, Hart raises questions about political campaigns that endure well beyond the campaign season. Students of campaigns, political pundits, and political consultants would all benefit from reading this book.

Presidential Greatness. By Marc Landy and Sidney M. Milkis. Lawrence: University of Kansas Press, 2000. 278p. \$34.95.

Lane Crothers, Illinois State University

This work is part of a broad tradition that addresses a central dilemma of democratic leadership: how leaders, especially bold, visionary, "great" leaders, can resolve contemporary crises while protecting the democratic character of the system. After all, leaders can engage in demagoguery, manipulation, and distortion. Alternatively, failed leaders place the political system at risk by not addressing and overcoming the crises of the day. Great democratic leadership means both answering society's needs and empowering the citizens in whose name leaders act.

Landy and Milkis take on this dilemma by first outlining a theory of democratic leadership and then exploring how five great U.S. presidents—Washington, Jefferson, Jackson, Lincoln, and Franklin Roosevelt—embody this model. Presidents are great when they achieve "conservative revolutions" (p. 4): systemic changes that expand and enhance democratic participation while at the same time protecting the stability of the political order. Thus, rather than oversee the collapse of the polity into demagoguery or ungovernability, great presidents resolve crises of the moment in ways that establish new political regimes. Furthermore, these new systems are seen to be inevitably associated with the establishment, expansion, or reorganization of political parties. Parties, according to the authors, are the vehicles through which public interests are structured, aggregated, and expressed in ways that empower both ordinary citizens and leaders while at the same time constraining the abusive potential of great leaders.

The text has several strengths that deserve special note. For example, the authors again draw our attention to the context in which great leaders act. Leadership is not just a trait of personality or a group of behaviors. It is grounded in a dynamic political context that shapes both the possibilities and meanings of leadership acts. Similarly, their argument about the role political parties have played in both empowering citizens and constraining leader abuses is important and innovative. Finally, their assertion, which will be discussed later, that great leadership is impossible after Roosevelt's establishment of the administrative state is stimulating if not entirely persuasive.

The book has weaknesses, however. The chapter on Andrew Jackson is by far the least convincing. For example, given the importance Landy and Milkis place on political parties as agents of presidential greatness, they necessarily focus on the development of the Democratic Party during this period. But most of their discussion focuses on Martin Van Buren, who is the real subject of 10 of the chapter's 33 pages. How Van Buren's unquestionable skill at party building makes Jackson a great president is unclear. Furthermore, on pages 86-8 the authors discuss Jackson's invasion of Florida in 1818-19, but the relation between this event and Jackson's greatness is never established. There is also an extended examination of the Eaton affair (pp. 94-7), a sexual scandal involving a Jackson supporter that occurred in his first term, but this seems to have no other purpose than to explain why Jackson came to trust Van Buren. Again, how this made Jackson a great president is not explained. Those acts that are seen to establish Jackson's leadership-Worcester v. Georgia, nullification, and the National Bank-are covered only in the final one-third of the chapter. Jackson may have been great, but this is not clearly established.

Another weakness lies in the authors' argument that greatness is necessarily related to party. For example, parties

were at best incipient during George Washington's time in office, so his greatness was necessarily established beyond the institutions of party. Party may be a factor in greatness, but it cannot be the whole explanation. On a related note, the authors' assertion that greatness is impossible after Roosevelt's establishment of the administrative state is unpersuasive. In their view, the possibility of presidential greatness has passed because parties no longer serve the mobilization and empowerment functions they once did (p. 198). This argument is unconvincing because (1) no clear standards for who was a great president are ever presented, so it is not clear why no president between Lincoln and Roosevelt was great, and (2) as the authors themselves note (p. 198), it was a longer gap in time between Lincoln and FDR than between FDR and the present. Concluding that presidential greatness has disappeared on the basis of this evidence is weak.

The volume also suffers from a problem common to many studies of great leaders: There is no comparison with failed presidents, many of whom faced the very problems that successor, great presidents were able to resolve. It is one thing to explain that great presidents handled significant problems with skill, but if this success is what qualifies them as great in the first place, it is not a particularly powerful analysis of presidential greatness. A comparative study of why some presidents addressed major crises while others, usually facing the same problems, did not would do much to delineate what factors made the great one great. Similarly, the presidents who follow great leaders almost always struggle to deal with the consequences of the politics their predecessors create; a comparative study of great and not great presidents facing similar contexts would go far to shed light on the shape and meaning of presidential greatness.

On balance, this book is a useful addition to the literature on leadership, democracy, and greatness. Its grounding of leadership in specific political contexts is important. The theory of party and greatness it outlines is deserving of substantial attention and should stimulate much research. Its insistence that democratic leadership must both resolve contemporary crises and empower citizens nicely frames the question of how the practice of leadership can augment, rather than compromise, democracy. Perhaps more important, its limitations can serve as the foundation for more theorizing and applied research. Understanding both why some leaders are great and why most are not in a democratic context is sure to remain a central question in political life.

International Politics and Civil Rights Policies in the United States, 1941–1960. By Azza Salama Layton. Cambridge: Cambridge University Press, 2000. 217p. \$49.95 cloth, \$17.95 paper.

Brenda Gayle Plummer, University of Wisconsin, Madison

This engaging book approaches the dynamic relationship between policymaking and interested publics by analyzing the issues from which policies arise in a specific temporal setting: Cold War America of the Truman and Eisenhower eras. The mid-twentieth-century drive for civil rights for African Americans provides a unique opportunity to examine policy formation in the context of both rapid social change and fundamental debate about the national interest. It is gratifying to know that more behavioral scientists are catching up with historians in realizing the salience of race as a factor in international affairs. Although the title suggests a complete examination of the war years, that part of the study is cursory. The work instead highlights the Truman years as the beginning of federal decision making that explicitly acknowledged

the difficulties that racial conflict and segregation posed for the U.S. mission abroad. In this account, the end of the Nazi racialist project and the exigencies of Soviet-American rivalry forced policymakers to oppose discrimination in American life. Later, Eisenhower, no advocate of rocking the boat on race relations, was also forced to recognize the implications of Cold War realities for domestic social practices.

Layton pushes this analysis farther. She avers that the emergence of a federal impetus for desegregation seems to have occurred at an unlikely time. Given the continued hostility to integration evidenced by opinion polls and violent incidents in the South during the late 1940s and early 1950s, what accounts for the weight that Truman assigned to racial reform? As a skilled politician, why would he risk the ire of the South and its entrenched senior representatives in Congress? Eisenhower found himself in a similar position. He came into office vowing not to interfere with the southern way of life, but early in his second term his historic decision to send federal troops to Little Rock reaffirmed the legacy of Reconstruction and reversed 80 years of hands-off policy. The thesis here is that international concerns outweighed domestic factors, including black protest, as reasons for growing governmental support for integration during this era. Those concerns included the possibilities that the Soviet Union would gain an ideological advantage over the United States, and that developing areas awaiting independence would see in U.S. racism reminders of their own anticolonial struggles. Efforts to align future independent states with the West would thus be undermined by Jim Crow.

Primary research in Department of State and other archival documents as well as readings of the foreign press and historical literature provide the empirical foundation for Layton's findings. Her argument is generally convincing but gives too much credit to the Department of State as an active reformer and protagonist. In fact, change was almost as often foisted upon an unwilling Department of State as on recalcitrant southern politicians. Michael L. Krenn (Black Diplomacy: African Americans and the State Department, 1945–1969, 1999) has documented the uphill struggle to integrate the Foreign Service and break down discriminatory practices of U.S. embassies and consulates abroad during this period.

Layton premises this study on a resource mobilization/ political process model. It thus joins the literature that traces fundamental social change to shifting opportunity structures. In this case, the conjunction of profound economic change, the discrediting of sociobiological explanations of race during the war, and the political imperatives brought about by Soviet-American competition made civil rights reform possible. Layton could have exploited opportunity structure dynamics more fully than she did. Political process theory generally perceives the organizational abilities of interest groups and the less well-defined but nonetheless important realm of consciousness as critical elements in effecting change. The author's desire to make the international agenda primary in the shaping of federal responses to civil rights causes her to understate somewhat the complex roles that interest groups played in making the issue relevant to government policymakers. The reader needs a more privileged view behind the scenes of official pronouncements and a clearer picture of how the rapid cultural transitions taking place in American life affected policymakers' perspectives.

The study raises intriguing questions. If racial-ethnic minorities have historically advanced most rapidly during times of war and when the nation faces ideological competition, it will be interesting to see in the future the implications for race relations of the current unchallenged U.S. hegemony. Does America need an external check on its potential for

human rights abuse? Are the swelling minority prison populations, the drive to limit minority access to higher education, and the popular affinity for the death penalty omens of a future in which no accountability is asked and none given?

International Politics and Civil Rights Policies in the United States is a fine introduction to the growing literature on the salience of race in the intersection of foreign and domestic affairs. Its concise format and clear language recommend it for instruction in university political science, sociology, and history courses.

Eco-Wars: Political Campaigns and Social Movements. By Ronald T. Libby. New York: Columbia University Press, 1999. 254p. \$45.00 cloth, \$21.50 paper.

Voices and Echoes for the Environment. By Ronald G. Shaiko. New York: Columbia University Press, 1999. 299p. \$49.50 cloth, \$21.00 paper.

George A. Gonzalez, University of Miami

During the 1960s and early 1970s, mass social movements developed that sought to circumvent the major political parties and directly affect the formulation and implementation of public policies. Among the most important was the environmental movement. Libby and Shaiko both seek to elucidate our understanding of the ability of this movement, and the interest groups that it spawned, to affect public policy development in the contemporary period. Shaiko, however, takes an appropriately skeptical approach, whereas Libby's view is more sanguine and as a result less probing.

The focus of Voices and Echoes for the Environment is the relationship between the leadership of environmental interest groups and their members. Shaiko specifically seeks to understand the ability of leaders and membership to communicate on policy questions. This requires a dual assessment. First, Shaiko analyzes the extent to which interest group leaders solicit members' opinion on various policy questions and the extent to which institutional mechanisms within these organizations allow members to communicate their policy preferences to the leadership. Second, he analyzes the ability and success of environmental interest groups to mobilize the membership on policy questions. Shaiko uses data from five environmental interest groups to gain an understanding of the relationship between leaders and members: Sierra Club (SC), The Wilderness Society (TWS), National Wildlife Federation (NWF), Environmental Defense Fund (EDF), and Environmental Action (EA). The most significant conclusion drawn from the analysis is that leaders tend to prioritize organizational maintenance over political advocacy. A similar argument is made by Mark Dowie in Losing Ground (1995). Shaiko, however, brings theoretical and empirical rigor in contrast to Dowie's often polemical tone.

According to Shaiko, a key reason for the increasing emphasis on organizational maintenance is the public interest group milieu. During the last thirty years there has been substantial growth in the number of public interest organizations competing for a limited pool of individuals with the inclination and disposable income to pay membership fees. Environmental interest group leaders, to varying degrees, have come to view their members more as an economic constituency and less as a political constituency. This is best exemplified by two trends: the hiring of individuals outside the environmental movement to be leaders for the specific purpose of organizational maintenance, and the offering of incentives to join or renew memberships, such as credit cards, posters, calendars, and magazine subscriptions. Moreover, Shaiko's analysis demonstrates that environmental interest

group members tend to join these organizations largely for these incentives and less for reasons related to political advocacy and public policy.

Among the groups Shaiko examines in detail, the EDF most overtly takes the position that its membership is primarily, if not exclusively, an economic resource for its leadership. The NWF does not openly embrace that view, but it nonetheless limits its political communications to members for fear of alienating existing and potential dues-paying members. The NWF by far has the largest membership among environmental groups (p. 41). EA is the most active in seeking to mobilize its membership to effect political change. Significantly, EA historically maintained a relatively small membership base, and in 1996 it went defunct due to insufficient financial resources. The SC and TWS make more concerted efforts to communicate to the membership on political issues than do either the EDF or the NWF. In addition, the SC maintains an institutional mechanism to allow members to communicate to the group's leaders on 188ues of public policy. Shaiko nonetheless concludes that in the contemporary period the leadership of these groups give higher priority to organizational maintenance than political

Shaiko has produced an important book that should be read and carefully examined by all scholars interested in environmental politics and policy. This work has one significant shortcoming that may limit its appeal, however. This relates to the way the book is laid out. Shaiko began this project as his dissertation, which was completed more than ten years ago. The book reads like he began his research with the sole objective of describing the relationship between the organizational elites of environmental interest groups and the mass membership, and he only came to his central and most significant argument years after much of the data were collected and analyzed. Shaiko only directly treats his argument about organizational maintenance versus political advocacy at the beginning and at the end of the book. Therefore, I often found myself asking how the data and analysis related to and supported the author's central thesis. Although less systematic and rigorous, Dowie's book will probably remain the seminal work on how the professionalization and bureaucratization of environmental interest groups have enervated U.S. environmental politics and the environmental social movement.

Whereas Shaiko makes a concerted and fruitful effort to clarify the complex relationship between leadership and members, Ronald Libby simply assumes that the membership is a resource that is readily mobilized for political purposes. The core of *Eco-Wars* is the description of five public policy issues that putatively involved social movements: (1) the controversy surrounding the 1993 FDA approval of bovine somatotropin (BST) to increase milk production from cows, (2) a failed 1988 Massachusetts initiative requiring the strict regulation of the management of farm animals, (3) a failed 1990 California initiative requiring the strict regulation of the state's agriculture and timber industries, (4) a 1994 failed initiative sponsored by Philip Morris to weaken many of California's local antismoking laws; and (5) the failed effort in the mid-1990s to reauthorize the Endangered Species Act.

My use of "putatively" is purposeful, because Libby does not put forward a method or means to assess the existence of a social movement. Without that it becomes difficult for the reader to determine whether a social movement is actually active in the policy issues he treats. In lieu of a method to verify directly the existence of a social movement, Libby uses the activity of what he terms "social movement organizations" (SMOs) as an indicator of social movement behavior.

His examples of an SMO are the Natural Resources Defense Council and the Sierra Club. As Shaiko's work aptly demonstrates, the assumption that such organizations represent and/or mobilize social movements is extremely dubious.

Moreover, it is unclear why Libby chose his case studies. According to my reading, broadly based movements existed in only two of his cases: the Philip Morris initiative and the reauthorization of the Endangered Species Act. The other three cases appear to involve only particular interest groups seeking to influence public policy through initiatives or the legislative process.

Also, Libby's use of the label "expressive" to identify certain interest groups is somewhat perplexing. It appears that an expressive interest group is simply one that takes a leading role on certain public policy issues, whereas groups that are only supportive earn the label of SMO. These labels are not analytically useful, and the label of SMO again serves to obfuscate.

In conclusion, the strength of Libby's book is his comprehensive and informative treatment of the case studies. In particular, his description of the events surrounding the failed attempt to reauthorize the Endangered Species Act is original and illuminating. He also draws some interesting generalizations about the tactics used by interest groups as they seek to influence policy formulation. Nevertheless, *Eco-Wars* only makes a limited contribution to our general understanding of interest groups, social movements, or the political influence of either of these entities.

Stuck in Neutral: Business and the Politics of Human Capital Investment Policy. By Cathie Jo Martin. Princeton, NJ: Princeton University Press, 2000. 262p. \$49.50 cloth, \$19.95 paper.

Sandra L. Suárez, Temple University

Cathie Jo Martin makes a novel contribution to our understanding of business preferences and political behavior. She argues that a considerable number of American firms strongly support "human capital investment" initiatives, such as national health care, training, child care, and family leave policies. Large employers' support for these policies, however, has failed to translate into legislative support. In order to understand why big business has not been able to promote the enactment of human resource policies, Martin proposes an institutional analysis of corporate preferences. In so doing, she opens the black box of business in order to understand its public policy interests.

Martin's explanation of business behavior challenges the interest-group and class-based approaches, which generally assume that economic motivations offer a complete explanation of business preferences and political activity. Martin points out that corporate preferences are problematic. Firms operate in an uncertain economic and political environment, and answers to important questions are not obvious to corporate managers. Still, to argue that there is a great deal of corporate support for national social policies goes against the laissez-faire thinking that is hegemonic in corporate circles and is contrary to the perception that big business has been responsible for the limited scope of welfare policies in the United States. Hence, it is important to offer a more nuanced understanding of corporate preferences.

Martin argues that business managers' support of human capital investment policies depends on their "corporate policy capacities," which she defines as the organizational facilities for "gathering, processing and disseminating technical arguments about social initiatives" (p. 11). Corporate policy

capacities have three components: private policy professionals and their position in the corporate structure, which may be important influences to corporate managers and may change their attitudes toward social policies; participation in business associations, which may also shape corporate preferences; and policy legacies in the form of failed private attempts, which may increase managers' support for government intervention, especially if the issue at hand can be linked to other issues of importance to the firm.

The book contains persuasive empirical evidence in the form of case studies and quantitative data. Martin conducted a large number of interviews with business managers, policy experts, and public sector employees. In one case studyhealth care reform—she examines the significance of institutional factors in shaping firm preferences regarding Clinton's national health reform initiative between 1993 and 1995. Martin finds that economic concerns led many firms to become interested in health care reform, but firms that participated in groups seeking collective solutions or firms with government affairs offices in Washington were more likely to support employer mandates than firms that obtained their information from health insurers, for example. Martin's qualitative analysis convincingly demonstrates that such institutional factors as corporate structures and group memberships are important determinants of the political position of a firm. The quantitative analysis of the data obtained from 59 Fortune manufacturing and service firms is somewhat less persuasive, although still informative. For example, she operationalized internal policy expertise depending on "whether or not the company had a government affairs office in Washington, D.C." (p. 104). If, as she says, such institutional factors as the position of a policy expert within the firm are important, having a Washington office in and of itself does not necessarily imply expertise in all policy issues.

In addition to an analysis of business preferences, Martin provides a sound explanation for why and how certain social issues become more salient for firms than others. In particular, she examines how corporate managers have dealt with training and work-family issues. She finds that training typically (1) fell under the jurisdiction of human resource departments, which enjoy a close relationship with the companies' own government affairs departments; (2) was associated with education reform; and (3) was deliberated at the National Alliance for Business, which also promoted legislative action on the issue. By contrast, a work-family issue such as child care (1) was dealt with by newer departments or by outsiders to the firm, (2) was associated with the feminist movement, and (3) could not count with a national organization bringing together business and government to promote a common position. Hence, child care issues have not been altogether neglected by business, but they have been tackled at the community and not the national level.

After examining preference formation and issue saliency, Martin explores why big business support for human capital investment policies (as measured by polling and interview data) did not translate into firms' collective participation in the legislative process. She finds that the corporate policy capacity of big business varied a great deal among training, health, and work-family issues, which had important consequences for the political outcomes of these initiatives during the Clinton administration. Martin explains that one reason most of these social initiatives failed was that small firms, which cannot afford to maintain an individual presence in Washington and are forced to rely on their representative associations, were able to communicate their opposition to social policies more effectively than big business was able to convey its support.

Martin's analysis fills a theoretical void in the interest group literature and invites further research to establish a dialogue with other social scientists concerned with preference formation, especially organizational sociologists. Her case studies convincingly demonstrate that the way business preferences are formed can have important implications for the success or failure of social policy initiatives in the United States. Plotke has shown that when big business is able to develop a common position it has an enormous capacity to influence the political discourse (David Plotke, "The Political Mobilization of Business," in Mark P. Petracca, ed., The Politics of Interests, 1992). Martin persuasively shows how difficult it is for big business in the United States to adopt common positions and formulate a political strategy in the realm of social policy. Stuck in Neutral is a must read for scholars of welfare state politics, policymaking in the United States and abroad, and business and government relations at the local, state, and federal level.

Controlling Technocracy: Citizen Rationality and the NIMBY Syndrome. By Gregory E. McAvoy. Washington, DC: Georgetown University Press, 1999. 184p. \$55.00 cloth, \$18.95 paper.

Brent S. Steel, Oregon State University

The emergence of the United States as a postindustrial society has led to an increasing array of social and political problems that confound government's ability to make and implement policy decisions. One is what may be termed the "democracy and technocracy quandary." The United States faces many policy problems that are highly technical and increasingly scientific in nature, including genetically engineered foods, habitat restoration for dwindling salmon runs, and the regulation of second-hand tobacco smoke in public settings. At the same time, the United States is a democratic system that has experienced over the past several decades a noticeable growth in distrust of government and increasing public demands for citizen involvement in governance, especially in the environmental policy arens

The concern is that the relationship between participation (democracy) and scientific expertise (technocracy) is mutually exclusive. On the one hand, placing too much emphasis on science and expertise as the ultimate determinants of policy outcomes risks the erosion of democracy. On the other hand, too much democracy (i.e., direct involvement of citizens in policymaking and implementation) may relegate technical and scientific information to a peripheral role and increase the probability that complex problems will either be ignored or addressed in a suboptimal manner.

Gregory McAvoy provides a well-written and timely investigation of the tension between technocracy and democracy in the context of hazardous waste policymaking. Hazardous waste siting involves complex issues in which substantial amounts of technical and scientific information are critical to the decision-making process. Identifying an acceptable location to store and/or process hazardous waste often leads to the not-in-my-backyard (NIMBY) syndrome from potentially affected communities. The book presents an interesting case study of two northern Minnesota counties (Koochiching and Red Lake), where affected communities and citizens challenged the state's siting decision.

The book is well grounded in the relevant literature and

offers a useful, albeit very brief, theoretical framework that can be used to analyze other policy processes in which the NIMBY syndrome is prevalent. In addition, a major strength of the book is its reliance on both qualitative and quantitative data to investigate elite and citizen motivations and preferences concerning waste siting policy. Elite and citizen interviews as well as public opinion surveys provide strong support for the arguments presented. McAvoy also is careful to identify any potentially unique cultural and political features of Minnesota that may bias the results. Nevertheless, he makes a convincing argument that his case study is applicable to other environmental policy processes involving complex technical expertise.

McAvoy challenges the notion that policy "experts" and "elites" should be accorded a privileged place in the policy process and that citizens are "overly emotional and self-serving, and therefore unable to participate in politics in a way that will allow society to achieve its best interest" (p. 90). He argues that citizen participation in the policy process is desirable for two reasons. First, it provides "legitimacy to democratic governance"; second, it leads to good policymaking because it "requires all participants to justify their technical assumptions and implicit and explicit value judgments" (p. 141). He further argues that democratic decision making should be seen as an important feature of managing complex and highly technical policy issues, such as hazardous waste siting.

This book is especially timely because of recent developments in the environmental and natural resource management arenas. The growing embrace of ecosystem management (EM) in the United States elevates the role of science as a tool for understanding environmental problems and policy issues. Yet, central to EM efforts are calls for more innovative, decentralized institutional arrangements that require government managers at the regional or local level to integrate not only the interests of other agencies that have adjacent jurisdictional and policy responsibilities but also the concerns of private citizens. EM acknowledges the limits of top-down regulatory approaches to environmental and natural resource management and recognizes that more effective alternatives require complex, collaborative partnerships among diverse government, civic, and business actors at the state/provincial and local levels. In this light, McAvoy's defense of citizen participation in the hazardous waste context has broader application to other developments in environmental and natural resource policy processes.

One aspect of the book that could have been further developed is the level of knowledge citizens actually have about the risks associated with hazardous waste and its proper disposal. Including some indicators of policy-relevant knowledge in the citizen surveys would have strengthened the multivariate analyses and potentially McAvoy's argument that citizens do have the capacity to understand and participate in complex policy issues and that the NIMBY syndrome is not necessarily the product of emotions and selfishness.

Overall this is an interesting and clearly written book that makes an important contribution to our understanding of how citizens can mobilize and influence environmental policy processes. Given the content, length, and price of the paper-back version, it would make an excellent supplemental text for senior or graduate courses in environmental politics and policy.

A Unified Theory of Voting: Directional and Proximity Spatial Models. By Samuel Merrill III and Bernard Grofman. New York: Cambridge University Press, 1999. 213p. \$54.95 cloth, \$19.95 paper.

Keith T. Poole, University of Houston

A Unified Theory of Voting is a "must read" for anyone wishing to stay current with the controversies in modern spatial theory. The book is somewhat uneven in exposition, but Merrill and Grofman do an excellent job of developing an elegant synthesis of several important spatial models and explaining how each model works. Part 1 is concerned with spatial models of voter behavior, and Part 2 covers spatial models of candidate/party behavior and strategy.

In the classic Downsian spatial model, citizens and politicians are assumed to be points in a multidimensional policy space. When a citizen votes, she chooses the politician closest to her in the policy space. This model has come under strong attack during the past 13 years from the directional theory of voting developed by George Rabinowitz. In contrast to the Downsian model, the directional model states that a voter chooses the politician who will change policy in her direction. She bases her voting decision upon direction, not distance. For example, in a simple one-dimensional world the citizen could be just to the right of zero (the "neutral point") with two candidates, one just a hair to the left of zero and another to the right of zero. She would vote for the candidate in her direction (right), even though she is closer to the left candidate. In its original form, Rabinowitz's theory required a neutral point and a "region of unacceptability"; that is, a region so distant from the neutral point that any candidate in the region is regarded as an extremist and thus cannot win an election. Needless to say, these two restrictions are a great source of controversy.

The publication of Rabinowitz's directional theory in 1989 touched off a lively debate with proponents of the proximity model, and both sides offered competing analyses of voting data from a variety of countries. Merrill and Grofman do an excellent job of synthesizing this literature and add their own original contributions to the debate. In particular, in Part 1 (chaps. 1–3) they show how the classical Downsian model and the directional model can be combined into a unified framework that can be empirically tested. The exposition is very clear, and these chapters only require an understanding of some very simple properties of vectors. This portion of the book could be used in an undergraduate course to introduce students to the basics of spatial theory.

Chapters 4-7 of Part 1 show various empirical tests of the two models. This requires a bit more sophistication on the part of the reader, but anyone familiar with the basics of OLS and probit/logit should find it interesting. The bottom-line findings are that voter choice appears to be a mix of directional and proximity components, and this mix varies with the type of candidate (challenger or incumbent). The key empirical result is that voters appear to respond to candidates who offer policies in their direction but reject candidates distant from themselves.

The mixed model solves the problem of the region of unacceptability, and Merrill and Grofman argue that it also explains the success of "moderately extreme" political parties. It has been apparent for some time that political parties do not converge as predicted by the Downsian spatial model. Instead, they diverge, but only to the "moderately extreme" region of the policy space. Merrill and Grofman show that their mixed model works well for a variety of countries and electoral systems, but they confine themselves to showing these empirical regularities and do not address why the

model works so well. They frankly state that "we are not really ready to distinguish between interpretations of our unified model" (p. 10). I am sympathetic, but this is the big question raised by this book. Provided that further empirical tests support their unified model, what model of individual behavior would generate this choice behavior?

Part 2 (chaps. 8-11) deals with the choice behavior of political parties. This portion of the book is much more technical and requires a good working knowledge of spatial theory. In addition, the exposition at some points is not very clear. For example, in chapter 8 the authors discuss stars and star angles without any explanation of what they are (graphs of median lines for electorates, as clearly defined in an appendix).

Merrill and Grofman show a number of theoretical results for the mixed model that produce political party behavior consistent with the empirical tests of voter behavior. For two-candidate elections in one dimension, the candidates will diverge from the median, and an equilibrium exists. In more than two dimensions, there may be no equilibrium, but there will be a region (a "pseudo-yolk") within which any candidate located there cannot be defeated by a candidate located outside the region. For more than two candidates in one dimension, the candidates will diverge, but neither the proximity nor the directional model necessarily predicts convergence of the candidates. Here, Merrill and Grofman argue that the mixed model is superior to the pure models empirically.

By summarizing and synthesizing the current state of the literature on directional versus proximity spatial models, Merrill and Grofman have moved the debate forward in an important way. Their empirical results for the mixed model raise important questions about the nature of individual choice. What model of individual behavior would generate these results? The debate will be a lively one. Stay tuned.

Black Social Capital: The Politics of School Reform in Baltimore, 1986–1998. By Marion Orr. Lawrence: University Press of Kansas, 1999. 242p. \$35.00 cloth, \$17.95 paper.

John Portz, Northeastern University

School reform has been near the top of the public agenda for more than fifteen years. Federal, state, and local policymakers have launched numerous reform initiatives, ranging from vouchers to high-stakes competency tests. Success remains elusive. Particularly in large cities, where poverty, racial divisions, and fiscal stress heighten the challenge, school reform has had limited effect in raising student achievement. Marion Orr helps us understand why. He offers an informative and well-written case study of recent reform initiatives in Baltimore. He presents a cogent political analysis of reform strategies, such as school-based management and privatization, and also develops and elaborates the concept of social capital as his theoretical framework.

Orr's application of social capital to school reform is an important contribution to urban studies as well as the general political science literature. As developed by Robert Putnam and others, social capital highlights the role of trust, norms, and networks in facilitating coordinated action. Similarly, Orr defines social capital as "an ability to work together to achieve social ends, based on past experiences and attachments, with minimal reliance on direct payments or coercion" (p. 13). As Orr argues, when applied to the education arena, social capital plays a critical role in developing and sustaining essential community support for school reform.

Orr finds social capital theory as currently developed to be

lacking in several respects. Using the insights of an urban regime perspective, he extends the concept in two major ways. First, social capital theory typically emphasizes interpersonal and intragroup bonds and connections that facilitate collective action. Orr argues, however, that such broad policy initiatives as urban school reform require collective effort of a different scale. Fostering greater individual participation in the affairs of a community is insufficient; policy initiatives require intergroup and cross-sector formation of trust and networks of cooperation. In the case of school reform, bonds of trust are needed that span a diverse set of community actors, including teachers, parents, business leaders, community leaders, and elected officials. "A major flaw in the social capital argument is that it does not consider the difficulty in transferring intragroup into intergroup social capital" (p. 10).

Building cross-sector networks is particularly difficult in America's cities, many of which face racial divisions and fiscal stress. In the case of Baltimore, Orr traces the historical development of social capital within the black community. He highlights the role of churches and the civil rights movement in creating a collective capacity among black leaders and the black community in general. With substantial economic and political resources controlled by white elites, however, the critical task becomes cross-sector and intergroup formations of trust and cooperation that include both black and white leaders and organizations. It is here that Baltimore, and many other cities, face a daunting task.

This points to the second way in which Orr extends the use of social capital theory. Typically, it highlights the participation of individuals in community affairs; governmental institutions and political leaders play a limited role. In contrast, Orr's analysis gives particular attention to government and its leaders. He argues that governmental actors and institutions, such as mayors, play an important part in fostering the development of social capital.

In Baltimore, the development of social capital was shaped significantly by government, its leaders, and their practices. Orr describes the emergence of a patronage culture in which blacks received particularistic benefits, such as jobs, from whites in control of city government. As the school department and even general city government came under the control of blacks, patronage remained a factor that limited broader appeals to collective action. Mayors also played a critical role. Kurt Schmoke, elected in 1986, was very active in the school arena and, with mixed success, often sought to build cross-sector support for reform efforts. Orr's discussion of mayoral roles is particularly interesting in light of the recent trend in large cities, such as Chicago, Detroit, and Boston, to give mayors more authority over school affairs.

With this theoretical framework, Orr analyzes the social and political dynamics that stymied or supported major school reform in Baltimore. School-based management, for example, faced entrenched interests, principally school administrators and the teachers' union, that were able to limit its adoption. Efforts by parents and teachers to establish a new school within the system, known as Stadium School, proved more successful in building critical cross-sector alliances. Highlighting the challenges of building bridges to the business community, Orr traces the development of Baltimore Commonwealth and the CollegeBound Foundation, two reform efforts that attempted to tap the resources of business elites. In separate chapters, Orr analyzes the unsuccessful experiment in privatizing the management of nine schools by Educational Alternatives, Inc., and he dissects the more recent consent decree with the State of Maryland that traded additional state monies for greater state oversight of the school district. In each reform experience, Orr highlights the struggle, often unsuccessful, to extend social capital to encompass a broad coalition of community actors.

Black Social Capital makes an important contribution to our understanding of education reform and urban politics. The faults in this book are few. In a couple of instances, the reader is left pondering a point that is treated briefly. For example, Orr details the difficult struggle to open the Stadium School as a within-district charter school, and he then concludes the chapter with a reference to the creation by 1997 of ten schools that appear to be similar. How these ten schools were created is a story that merits additional attention. Notwithstanding such details, Orr's study of Baltimore offers a clear picture of the challenges faced in America's cities as civic leaders and city residents attempt to build networks of cooperation in support of public education.

Citizen Participation in Resource Allocation. By William Simonsen and Mark D. Robbins. Boulder, CO: Westview, 2000. 179p. \$59.00 cloth, \$22.00 paper.

Jeffrey Kraus, Wagner College

Simonsen and Robbins examine Eugene Decisions, an effort to engage citizens in the local decision-making process in Eugene, Oregon. The community participation movement was an outgrowth of the civil rights movement and Lyndon Johnson's War on Poverty. In large urban areas, African Americans and other disenfranchised groups came to believe that local political institutions were, at best, indifferent to their concerns. There were also antipoverty warriors and theoreticians (notably Kenneth Clark) who believed that empowering the poor was essential to breaking the cycle of poverty.

During the 1960s, federally funded community action agencies, local development corporations, "Little City Halls," and decentralized government services constituted the elite response to demands for greater participation in urban areas. In many communities, the activism of these efforts disrupted the established political regimes. As Dennis Judd and Todd Swanstrom (City Politics, 1998, p. 405) observe, shifting city services (and control over those services) from middle-class white ethnic neighborhoods to low-income minority areas was a development that most city governments resisted. In 1967, in response to pressure from mayors, Congress passed the Green Amendment, which required federal funds for community action agencies to be allocated through state and local governments, to ensure that the recipients would not challenge the status quo (Bertram M. Gross and Jeffrey Kraus, "The Political Machine Is Alive and Well," Social Policy 12 [Winter 1982]: 35-47)

During the 1970s, white middle-class residents in suburbs and big city enclaves began seeking more influence over local government decisions. This movement relied on two strategies. The first was the ballot initiative. Beginning with California's Proposition 13, middle-class residents, fed up with paying taxes for services of poor quality or for services that they believed were being consumed by poor (primarily minority) residents in cities, began reining in local governments by limiting their ability to raise taxes. Oregon was another state where this occurred; it adopted Measure 5 in 1990, which limited local property tax levies to 1% of real market values. The second strategy was to increase involvement in grassroots politics. Harry Boyte (The Backyard Revolution, 1980) made the first effort to chronicle this movement

In recent years, there has been even greater disillusionment with government. Many people perceive that elected officials have become beholden to the interest groups that help fund their campaigns (one person, one vote has been supplanted by one dollar, one vote).

In the introduction and first two chapters, Simonsen and Robbins discuss the theoretical and historical context of public administration and contemporary techniques for citizen involvement, as well as the general decline in citizen participation and the concurrent distrust of public officials. For those who lack an understanding of citizen participation efforts, these chapters offer a brief yet comprehensive analysis.

The remaining three chapters focus on Eugene. The goal there was to engage citizens in a dialogue with their elected officials on the city's budget priorities. The "Build Your Own Budget" survey allowed people to articulate their priorities for the community. Participants in the November 1991 survey were informed that the city would face a budget deficit of \$8 million by 1994. They were asked about their support for service cuts, tax increases, or the imposition of user fees.

"When faced with realistic options for service reductions, the overwhelming majority of respondents chose at least some revenue increases" (p. 68). As the authors note, however, these findings are from an "informed public." The uninformed public is less likely to support revenue enhancements. Cuts suggested by survey respondents were in such areas as planning, park maintenance, and building maintenance—not dissimilar to the choices that public officials make when paring budgets: Cut services that do not have an immediate effect on the public.

In chapter 4, the authors test the notion of whether fiscal information and service use influence citizen preferences. They find that the more people know about the costs of government service, the less likely they are to support taxes for services. "Fiscal conservatives who wish to stymie government spending would be well served in this aim by publicizing the true costs of government services. On the other hand, it appears that fiscal liberals wishing to increase spending would do better with their program if the citizenry remained in fiscal ignorance" (p. 108). Of course, this also suggests that fiscal liberals would best be served if people remained estranged from the political process.

The authors report that some people (good citizens) are willing to pay for services that are not individually consumed but that provide a public service, whereas others (private citizens) are more concerned about obtaining the lowest price for individually consumed services and less concerned about collectively consumed services. Of course, this leads to the question of what happens when there are not enough good citizens to support essential collective services.

The authors also raise questions about the "customer service" focus of the Clinton administration's reinventing government initiative. If they are not customers, will taxpayers be willing to pay for services they do not use? The authors seem to have some concerns about this prospect and fear that social services and other programs targeted at the poor will be the "first casualties" of a "privatized government" (p. 111).

In the final chapter, Simonsen and Robbins reflect on what they have found. Ironically, they assert, making a group of citizens participate in the budget process by providing them with financial information means they are less representative of the larger public they are intended to represent. The authors also suggest that citizen perceptions of government are affected by their contacts with it. Do traffic tickets or road construction delays cause a decline in support for government service? Does one's perception that he is a customer of government or is a member of a cooperative affect his view of government?

The authors demur on the issue of whether local governments should use citizen participation processes. They contend they have studied one tool of governance and conclude: "As with any tool; its use to craft something useful relies for its satisfactory expression on those who use it" (p. 123).

There are questions that the authors do not raise or answer. First and foremost, do they believe that the kind of citizen participation exercise undertaken in Eugene (with a population 93% white) is feasible in more heterogeneous communities? Would the model work in New York City? Or does it work only in homogeneous, middle-class, white communities?

In an age when we bowl alone, does it make sense to believe that people will make decisions as if they were part of a "family," the term Mario Cuomo often used to describe what he believed the state polity reflected during his years as governor of New York. The authors accept the premise that more citizen involvement is desirable, but in light of some of their findings regarding the unwillingness of people to pay for services they do not use, this may not lead to positive outcomes. Even if there is an eagerness for enhanced citizen participation, unless we are headed in the direction of an Internet-powered plebiscitary democracy (as suggested by Dick Morris in Vote.com, 1999), we are still going to hold public officials accountable for their actions. Do we really need more participation if it is going to result in policies that fail to take into account the common good? Who knows? Perhaps Mr. Madison had it right back in 1787.

Reaching beyond Race. By Paul M. Sniderman and Edward G. Carmines. Cambridge, MA: Harvard University Press, 1997. 191p. \$22.95 cloth, \$14.95 paper.

Jennifer L. Hochschild, Princeton University

This book is part of a seemingly endless multisided argument among social scientists about the causes and trajectory of white American attitudes toward African Americans. The most prominent claims are, roughly, those of symbolic racism (whites are prejudiced because of a mix of racially innocent moral values and deeply seated racial aversion); group interests (whites are prejudiced because they fear that the advancement of blacks will injure the well-being of whites as a category); social dominance theory (whites are prejudiced because they like being on top); and principled conservatism (most whites are not prejudiced, and prejudice explains little of the opposition to government policies designed to help blacks). The latter view is the least favored among scholars, and it is the view expounded by Sniderman and Carmines.

Reaching beyond Race has two very great strengths that make it essential reading for anyone in the fields of public opinion or race relations. First, it takes people—even conservatives who oppose affirmative action or other policies to promote racial equality—at their word. If a survey respondent says the equivalent of "I'm not prejudiced, but...," Carmines and Sniderman assume that is true unless proven otherwise. "The more closely we listened to expressions like this, the more they seemed to testify not to a wish to cover up opposition to racial equality but to a desire to reaffirm commitment to it" (p. 46). One may reject this view—I coauthored an article that revolves around just such a rejection—but Sniderman and Carmines force us to accept that, in the name of moral symmetry, racial conservatives deserve the same starting point as racial liberals.

The other powerful virtue of Reaching beyond Race is its imaginative and innovative use of experiments within the rigid confines of a survey to tease out meanings and nuances

of responses. These authors (along with several others, such as James Kuklinski) are pioneers in taking advantage of the power of randomization of respondents and systematization of computer software in order to make surveys much more flexible than we imagined they could be a decade ago. Even readers with no interest in racial attitudes, or no tolerance for what sometimes reads as an apologia for white racial complacency, should study this book carefully in order to learn how to push surveys beyond their normally frustrating limits.

The central argument should appeal to all political scientists: Policy preferences about racial inequality, like policy preferences about most other matters, are at their core political. Citizens choose among specific alternatives shaped by political elites; the process of determining alternatives and choosing among them is at base a moral concern and not merely a material one; people can be persuaded to choose what they and others think is right by the politics of framing morally desirable alternatives. Less abstractly, because enough Americans value fairness, equality of opportunity, and racial equity, they can be led to choose policies that will promote these values so long as the policy choices are not framed in terms of special benefits for blacks. Policies such as racially based affirmative action violate Americans' core universalistic principles, so people oppose them for reasons that have very little to do with racial prejudice.

Sniderman and Carmines complicate this basic argument in interesting ways by comparing liberals and conservatives, those who admit to racial prejudice and those who claim to "like blacks," and other basic groups. But in the end they come back to the claim that if elites frame choices in ways that reach beyond race, a majority of Americans will support policies that promote racial equality.

This is a surprisingly controversial argument for several reasons. Sniderman and Carmines do not help their case by characterizing people who hold one of the other three views described in the first paragraph as "both wrong and wrongheaded" (p. 115) or by emphasizing the overall results when it suits their argument (e.g., p. 30) but the differences between groups when that suits their argument (e.g., p. 40).

More serious is their, and other surveyors', insistence on analyzing views about affirmative action through the language of "preferences" and "quotas," on the one hand, and "pure merit," on the other. Quotas are almost always illegal, according to the Supreme Court, and are very rarely used to my knowledge, and I know of no university or employer who would operate on a principle of pure merit but for affirmative action. So these survey questions generate apparent attitudes that are distorted in at least two ways through no fault of the respondents.

Another reason for controversy is the authors' (or their editor's?) insistence on "dumbing down" the book to keep it short and snappy. That has the predictable effect of making scholars constantly irritated by the scanty reports on how regression analyses were conducted, or how questions were worded, or how seriously we should take the results of subgroup analyses (in one figure "the number of respondents ranges from 13 to 55" [p. 95]). It also keeps their definition of prejudice equivalent to whether whites "like" blacks, which is arguably irrelevant to the real issues of racial structuring in the United States. Finally, keeping the book short and snappy makes the last chapter of moral exhortation and policy recommendations too superficial. These are serious scholars who have thought for decades about the conundrums of race in America, and at least this reader remains frustrated that they kept their conclusions so simplistic.

But the main reason that the argument is controversial is the point with which I began: Sniderman and Carmines insist that racial conservatives deserve the same standing as racial liberals, that liberals are just as likely (or even more so) to be racially hypocritical as conservatives, and that ignoring race is the best way to reduce racial inequality. These are genuinely contentious claims, and the authors probably come closer to the median American view than do most scholars focused on pervasive racism. That fact does not make Sniderman and Carmines correct, but it does mean that we should pay close attention to the arguments in Reaching beyond Race.

Comparative Politics

Comparative Peace Processes in Latin America. Edited by Cynthia J. Arnson. Washington, DC: Woodrow Wilson Center Press and Stanford: Stanford University Press, 1999. 216p. \$60.00 cloth, \$24.00 paper.

Fen Osler Hampson, Carleton University

This important book examines how negotiation processes have contributed to efforts to terminate guerrilla insurgencies in six Latin American countries: Colombia, El Salvador, Guatemala, Mexico, Nicaragua, and Peru. Although these nations have widely disparate political, economic, and social histories, they share a number of characteristics. The guerrilla msurgencies in most cases continued past the end of the Cold War, all have democratically elected governments, and all sought at one point or another—with the notable exception of Peru—some sort of negotiation with the leaders of guerrilla movements.

The volume is divided into two parts. The first is comprised of essays that examine the conflict and negotiation history of each country in considerable detail. The second section contains comparative analyses and assessments of efforts to

consolidate the peace and political reform processes in different country settings. Justice and reconciliation issues are the subject of two particularly insightful chapters by José Zalaquett and Priscilla Hayner, respectively. In an equally valuable contribution, George Vickers examines the problems of reforming internal security institutions and the lessons of Central America, dramatically underscoring how high crime rates and a tenacious pattern of civil violence pose major and continuing threats to democratic consolidation in this region. Finally, postconflict political economy problems and challenges—an altogether understudied topic—are the focus of Rachel McCleary's short but good chapter.

Many edited volumes contain essays of uneven quality that wander across too wide a canvass. These chapters, all of high caliber, do not stray from the two central questions posed: (1) What conditions help sustain the transition to democracy in countries that have been wracked by recurring civil violence and social unrest? (2) What are the ingredients for a negotiation process that leads to a peace settlement and some form of political accommodation between governing elites and guerrillas?

In response to the first question, the case studies and the conclusion argue that in those countries that made the jump to democracy, guerrilla movements not only had to be convinced that they could achieve their political aims through the democratic process but also had to become active participants in the process. Other factors include a country's ability to tackle such problems as the demobilization and reintegration of former combatants, the needs of displaced peoples, poverty and underdevelopment, and electoral and judicial reform; the subordination of the security forces of the state to civilian control presents the biggest challenge.

In response to the second question, the volume argues that peace processes involve not only interests but also perceptions. Cynthia Arnson, in the concluding chapter, takes issue with rational choice theorists who typically argue that the cost-benefit calculus of key actors will only change in favor of a political settlement when the conflict reaches a plateau, or what is sometimes called a "hurting stalemate." Instead, she suggests, "the perception of interest of key actors as to the costs of continuing war and as to their future with or without a negotiated settlement" derives not just from their military strength/weakness or the real pain they are experiencing but from political will or a "voluntad de paz" (p. 451) as well. In advancing this argument, she argues that because perceptions are malleable and can be manipulated or influenced by external third parties, one does not have to wait for so-called ripe moments. One can create them by engaging the parties in a dialogue that explores a wide variety of political alternatives to continuing violence and conflict. Negotiated confidence-building measures and interim agreements can thus pave the way to a more robust set of political understandings and agreements that eventually terminate hostilities and leave the door open to democracy. Unfortunately, Arnson sidesteps the analytical problem of just how we should measure or assess the "psychological" as opposed to "objective" factors associated with "ripeness." She appears to argue that we should assess ripe moments by the parties' perceived willingness to accept confidence-building measures, in which case we are asked to judge actors' motives by their actions and fall into the same tautological trap that plagues the rational-choice definition of "ripeness." Even so, there is much to commend this book to both an academic and practitioner audience, particularly its insights and lessons about democratic transitions in war-torn societies and the sorts of measures and conditions that are required to sustain an often fragile peace process.

Why Governments Waste Natural Resources: Policy Failures in Developing Countries. By William Ascher. Baltimore, MD: Johns Hopkins University Press, 1999. 333p. \$65.00.

Gregory White, Smith College

William Ascher examines a fundamental puzzle in the politics of natural resource exploitation: Why do governments waste natural resources? For him, this question is most trouble-some for "developing countries" in Africa, Asia, and Latin America. Many countries in these regions depend on oil, forestry, mining, land, and water resources to stimulate economic growth, create employment, and generate revenue for the national budget. Blessed with abundant natural resources, however, far too many countries are cursed with the poor management of their resources. The result is not only failure in terms of economic goals but also environmental degradation and pollution.

To his credit, Ascher is critical of analyses that merely invoke greed, corruption, or incompetence by policymakers as a complete explanation. "The easiest way for a political scientist to seem penetrating and tough-minded is to 'model'

the behavior of political leaders as self-interested people out for power, personal financial gain, or both" (p. ix). In Ascher's view, such modeling is fundamentally cynical. More important, it leaves no room for potential institutional and policy reforms.

Instead, Ascher pursues a sophisticated, widely ranging comparison of sixteen cases of resource policy failure in, for example, Indonesia (oil), Malaysia (timber), Mexico (irrigation), Costa Rica (land and timber), India (copper), Nigeria (oil), and Cameroon (land, forests, and parks). He argues that officials in a variety of contexts resort to six strategies of natural resource exploitation in order to: (1) finance development programs; (2) provide economic gain for particular groups, regions, or individuals; (3) build the coffers of the central treasury; (4) generate rents to build political support for other endeavors; (5) capture the profits generated before other actors do so; and (6) avoid accountability. Natural resources are especially vulnerable to such efforts because there is little political cost; natural resource exploitation usually occurs in remote locations. If there are people on the land, they are frequently isolated and marginal. Moreover, natural resources are seen as abundant national endowments, a country's birthright to be "developed" rapidly.

Why Governments Waste Natural Resources details the range of policy failures that have occurred in the cases at hand, including poor pricing of commodities, failure to ensure competition through monopoly arrangements, imprecise property rights, inadequate information, overexploitation, wasteful spending, unwise investment decisions, and poor strategic planning. The enormous strength of Ascher's book is the manner in which it details and illuminates the wide array of policy failures. Moreover, the author adeptly demonstrates the enormous environmental damage. His examination of Cameroon's forest sector, for example, illustrates the cost in terms of deforestation, the loss of the bulk of rents to foreign logging interests, and the effect on local forest dwellers of logging in "conservation" areas.

In many important ways, Ascher's work points to the ongoing importance of vibrant area studies research. By skillfully plumbing a disparate set of countries and resource sectors—Ascher uses secondary materials when the case falls outside his own expertise in Latin America—the author is able to examine the validity and reliability of sophisticated theoretical concerns. His book has an admirable richness and complexity.

Despite its advantages, the volume suffers from two short-comings. First, Ascher only briefly treats the enormous and unpredictable external demand for natural resources in the international economy. It may be somewhat questionable to challenge mismanagement of a natural resource when the country is buffeted by fluctuating commodity prices in the international market or when foreign firms control the vast bulk of a given commodity's extraction and production. Ascher seems well aware of a given sector's connection to the external economy, yet he does not allow the possibility that the options open to policymakers may be exceptionally narrow.

On a related note, he may not give adequate attention to a country's colonial legacy or the manner in which its resource sector was incorporated into the international economy. Chapters 1 and 2, which lay out Ascher's theoretical concerns, seem to avoid a treatment of such matters. Yet, chapters 3-6, in which Ascher examines the cases in detail, do engage crucial external connections. For example, regarding Indonesia's oil company, Pertamina, Ascher notes its genesis in the 1950s under military control and its full

emergence in the 1960s after agreements with such international firms as Shell, Texaco, and Mobil.

Second, Ascher demonstrates a faith in market-based solutions that may not be sensible. He concludes that governments are ill-equipped to serve in the roles of resource developer and resource conserver. It is hard to quibble with this conclusion. Problems typically emerge when governments are expected to exploit a natural resource as well as regulate its exploitation. The author also makes a reasonable argument on behalf of restoring nongovernment control. Increased participation by private and communal entities would be consistent with trends toward privatization, community empowerment, and decentralization; such participation might enhance the regulatory capacity of government. Nonetheless, the implicit assumption in Ascher's discussion of the best practice and optimal policies—in order to distinguish from policy failures—makes a case for "dynamic efficiency" and the wisdom of market forces. Indeed, in Ascher's framework, government interference in market forces is what tends to prompt policy failures. One is not always convinced, however, that the market would treat natural resources better than meddling government officials or bad development

Despite these shortcomings, the book is an outstanding and provocative contribution to an important field of inquiry. Ascher makes a compelling case that governments should restructure their resource exploitation by, for example, simplifying the mandate of agencies, clarifying jurisdictions, and prioritizing through the central budget. The book also makes a worthy call for governments and citizens to pay greater attention to the effect of development strategies on the natural resource endowment. Again, nongovernmental actors figure into Ascher's framework in that they could play important roles by publicizing abuses, supporting opponents of policy, providing crucial epistemic support for proper policies, and challenging questionable governmental policy and claims.

Readers from the fields of environmental studies, comparative politics, and policy studies will be rewarded.

Community Conflicts and the State in India. Edited by Amrita Basu and Atul Kohli. Delhi: Oxford University Press, 1998. 363p. \$34.95.

Democracy, Development, and the Countryside: Urban-Rural Struggles in India. By Ashutosh Varshney. Cambridge: Cambridge University Press, 1995. 168p. \$39.95.

Ronald Herring, Cornell University

The common lesson of these seemingly disparate books is the difficulty of reading interests, identifications, and political dynamics from structure. It is handy to read interests from social structure, either via political economy or macrosociology; doing so not only enables parsimony and deductive power but also is easier than learning from individuals how they conceptualize the political world, and their interests and identities in it, which is tedious work typically left to anthropologists. Both books try to come to terms with the same question: Which of many possible political identities will become operative? Both are fine books, well worth the reading. Varshney has the harder job because he wants to resolve a specific puzzle. The authors and editors of the Basu and Kohli volume seek a framework within which divergent cases make sense.

Of the many ways to slice categories of political power, interests, and identifications, one is urban-rural. Varshney investigates the paradox of rural power. It is typically low at

early stages of industrialization, when peasants constitute a numerical majority, and very high proportional to population after industrialization, when fewer people are occupied in agriculture. The standard answer derives in part from the theory of collective action. Farmers in rich countries are able to mobilize easily—being fewer in number—and their demands are more affordable as agriculture proportionately declines in an expanding GDP. As Barrington Moore argues in his classic Social Origins of Dictatorship and Democracy (1966), peasants as a class are victims of modernization, but the few who survive—the farmers—are empowered by the economics of wealthy societies and the politics of Mancur Olson. Varshney argues that rural empowerment in India has operated both from above, in parliamentary politics and state institutions, and from below, in the form of mass mobilization.

Why should one select this particular political morphology, that is, see the world as urban and rural rather than as class divided, or sector specific, or inhabited by "communities," as in the Kohli and Basu volume? Varshney takes the urban-rural categorization to be unproblematic, but "rural" power is notoriously difficult to measure. Agriculturalists have increased their share in India's parliament over the years, replacing businessmen and lawyers, but parliamentarians do not always represent their origins very well, identifying instead with their destination. Moreover, more rural MPs do not necessarily translate into a prorural policy; the state has other compulsions. Does democracy make a difference to power and well-being of the countryside?

For Varshney, both identities and interests are constructed. Were there to be an imagined community of rural India, he believes that unity would prevail in democratic processes: Rural people are, after all, the majority. How much can the multiplicity of individual interests be obscured by a popular version of the ecological fallacy of social scientists—to think first of a "rural" interest? Varshney argues that rural power will be limited as long as ascriptive identities remain important. At the level of mobilization, then, the answer to the democratic paradox is that crosscutting cleavages of the sort investigated in the Basu and Kohli volume put limits on rural power. But is this the explanation of urban power? Urban interests are divided by similar alternative identities; after all, it is in caues that ethnic/religious carnage is more common. More important, I think, is that the power of capital is structural, not electoral. The state is looking over a political economy in which the interests of urban capital loom large: The "business climate," investment strikes, capital flight, and attracting foreign direct investment are all factors that confront and constrain any government seeking to retain power. Peasants will continue to produce or go hungry otherwise. Capital is mobile and hard to appease. That only large capital needs to be propitiated for its structural position is a logic that pervades the state as well as political rulers. Varshney himself acknowledges that the state is the critical actor blocking full success of agrarian power (and his disaggregation of the state is very

Dealing in sectors also leads to some awkward analytics. Varshney is certain that "rural India" has been empowered relative to rural sectors in other societies at similar stages of industrialization, but he acknowledges that between one-fourth and one-third of rural India—the landless laborers—have been left out of this empowerment and enrichment. But, he argues, a similar percentage of the urban sector has been left out, again the poor. If so, then why is the sectoral analysis important? Could one simply conclude that the poor have not been empowered, whether urban or rural? After demonstrat-

ing that rural interests are differentiated across crops and classes, Varshney concludes that in material terms farmers did not get the best-case scenario—increasing returns to agriculture—but also did not suffer the worst case—falling prices because of surpluses (awkward in a society with mass malnutrition). And subsidies rose, but netting out the effect of subsidies, low taxation, and terms of trade movements influenced by price setting and exchange rates is so tangled an enterprise that net effects are difficult to ascertain.

Nevertheless, new forms of mobilization did arise around what can be called scissors politics: Farmers caught between rising input costs and stagnant output prices press the state to fix one or the other. Contrary to the logic of Karl Marx's Eighteenth Brumaire argument about "potatoes in a sack" that peasants cannot form a class for themselves—Varshney devotes a fine chapter to the rise of agrarian power at the grassroots in the 1970s. At the level of cultural politics and movement rhetoric, rural India became an episodic political reality. The movements for higher prices were a new political form: nonparty, sectoral in self-identification, neither revolutionary nor trapped in ineffectual deployment of "weapons of the weak." The puzzle for collective action is that more people in rural India would be hurt by higher food prices than would benefit: Varshney admits that "price agitations...do not unambiguously serve the interests of the entire sector" (p. 138). Perhaps this observation is in itself sufficient to explain why there was only sporadic, regionally isolated, and unsustained collective action; the existence of alternative identities may not be necessary to understand the self-limiting nature of rural mobilization around prices.

The authors in the Basu and Kohli book take up just this problematic but find that, like rurality, identities organized around religion, caste, language, and region may not be stable. If identities are "politically constructed," and the raw materials vary enormously (not just caste, religion, language, and region but the specificity of each by place and time, further complicated by permutations of these variables), then which identity is emergent in particular configurations of political appeals and state action becomes mind-bogglingly complex. Moreover, state cues, state openings, state repression, and political competition among various groups frame the context of mobilization. The book is a refutation of primordialism in the form of "ancient hatreds," but, not surprisingly with so many variables at play, generalizations elude both authors and editors.

Kohli's model is basically competitive: With scarce resources, politicians deploy ethnic appeals, but if demands are met, the process is self-limiting. Competition for state resources has become increasingly democratic in a context in which hierarchical understandings of order have decayed. Competition, for example, leads politicians to endorse affirmative action, which in turn mobilizes backlash in previously privileged groups. It is inconcervable that all demands can be met. Both opportunists and hardliners exacerbate ethnic violence into self-reinforcing spirals. It is hard to know when concessions will defuse a conflict or when a show of force will escalate it, and therefore it is hard to know how the interests of state operatives lead to behavior.

It is likewise unclear ex ante what political strategies will work, but it is certainly true that the strategic cognitive maps of political actors matter fundamentally in mobilizing identities. In the piece by Mary Katzenstein, Uday Mehta, and Usha Thakkar, for example, the Shiv Sena shifts from ethnic ("sons of the soil") to religious (Hindu nationalism) appeals to reposition itself strategically to graduate from a regional party to a national presence (with some demonizing carryover of political tropes). Christophe Jaffrelot's chapter

illustrates that religious riots serve the interests of the Bharatiya Janata Party; by polarizing "communities," riots reinforce self-identification of Hindus, which allows the party to expand geographically. Amrita Basu notes that governments may be complicit with communal violence as a means to party ends. The methodological conclusion is that even more phenomenology of leaders would be valuable: What is inside the heads of those who pull the levers of state and formulate mobilization strategies? Again, reading interests and behavior off structural position confronts insuperable obstacles.

The results of these studies of identity politics are unclear. Varshney sees the mobilization of a specifically rural identity as furthering Indian democracy. Basu concludes that ethnic (often "caste") conflict may further the democratic process of reversing traditional patterns of oppression; but religious primordialism is more likely to be a threat to democracy. Arun Swamy's analysis of alternative modes of discourse of populism in Tamil Nadu illustrates the first point: groups with lower class/status mobilized against a system of ascriptive privilege and exclusion. Competition within the movement was between cultural nationalism and propoor populism. The results have been good for the poor and excluded, at least relative to plausible alternatives. One could draw a very similar set of conclusions from the mobilization of blacks in the American South, in terms of both forms and consequences of identity politics.

Some ethnic conflict deflects class issues, and some overlaps with class mobilization, particularly when ascriptive status coincides with class position, as it often does, in India or elsewhere. By coding such political forces only ascriptive, the real interests at stake (which are often, but only partly, material) are obscured. Jyotirendra Dasgupta faults the "standard narrative of violence" for failing to recognize "the long-term constructive implications of many anti-authority struggles" (p. 183). Lower-class movements in identity mode have, in Basu's words, "broadened the parameters of democracy by demanding greater rights and opportunities" (p. 246). In this sense, demands by social movements and political parties using the language of ethnicity have included devolution of power and resources in ways similar to those of social movements centered around the environment, civil liberties, and women's rights.

Both books are about appropriate units of political analysis; in neither is that unit the individual. There is a tendency in the Basu and Kohli volume to see a world composed of "communities." Varshney vacillates between a standard methodological individualist approach, which finds collectives problematic, and an assumption that there really is a rural community facing cross-cutting cleavages from other collective identifications of the sort analyzed in the Basu and Kohli book. What both works establish clearly is the multiple, unstable, and situation-contingent nature of political identities, interests derived therefrom, and subsequent political behavior. This is a useful corrective to both structural organicism, which takes social morphologies as given, and deductive methodological individualism, which reads interests of individuals off position. There is a largish gap between social structural position and perception of interest because positions are overlapping and because consequences of behavior are difficult to predict. How will higher farm prices affect employment prospects in the long run? How much economic loss will ensue from local riots? Will one's house be torched? Finally, identity interests are emergent and unstable, dependent on state cues, political opportunity, state openings and repressions, and complex imaginings at the individual level

that escape macro analysis altogether. One leaves these books wishing there were more about those individuals.

Environmental Politics in Japan: Networks of Power and Protest. By Jeffrey Broadbent. Cambridge: Cambridge University Press, 1998. 440p. \$64.95 cloth, \$22.95 paper.

Deborah J. Milly, Virginia Polytechnic Institute and State University

Japan experienced several decades of astounding postwar economic expansion, but the many Japanese who lived near sites of concentrated industrial production were well aware of its deleterious effects on their health and the environment. By the late 1970s, however, Japan's program of environmental recovery begun earlier in the decade was having a striking effect. From the vantage point of residents in rural Oita prefecture, in southern Japan, Jeffrey Broadbent untangles the contradictory politics that produced progrowth policies followed by environmental turnarounds. He explores protest over industrial development projects and their environmental effects as this activism was felt at multiple levels of government and society. An ethnographic narrative grounded in comparative and theoretical concerns, Environmental Politics in Japan is a study of the emergence, progression, and influence of protest that spans more than two decades.

The book situates Japan comparatively with other advanced, capitalist, industrialized democratic societies that face the dilemma of how best to weigh and balance conflicting goals of sustained economic growth and environmental protection. Japan stands out among this group, however, in the extremes it has evidenced in the form of "miracles" (in both its economic growth and subsequent environmental cleanup) and "debacles" (including extreme environmental pollution and inability to carry out plans to decentralize industrial production). The author attributes this pattern of extremes to the comparatively high "social intensity" of pollution in Japan and to societal factors. Social intensity, or the proportion of the population affected, accounts for the political readiness to take action after degradation occurs, but societal factors—particularly social and institutional networks-have intervened to facilitate or obstruct that readi-

This heavily documented account draws on more than 500 interviews and extensive archival research to provide a comprehensive treatment of the evolution of issues, protests, and interactions among individuals at different levels of government and society. Research was conducted in the late 1970s, when rapid economic growth had given way to new dilemmas of slow growth but when policy initiatives had begun to ameliorate the egregious aspects of environmental degradation. Fluent in Japanese and very familiar with Japanese society, the author lived for two and one-half years in Oita prefecture, where he observed the complexities of local politics surrounding a new industrial complex. Environmental Politics in Japan does much more, however, than depict a single moment in the life of local politics over the environment. It covers the period from the late 1950s through the early 1980s, and it recounts and reflects on politics surrounding five major policy shifts. These include cases of both development planning and environmental protection as well as instances of prefectural and national decision making.

The book is an engrossing study of collective action that begins at the local level but hardly ends there. Throughout the volume, the author compellingly weaves theoretical reflections on the dynamic relationship between structural features and individual action into the narrative accounts of specific interactions among protestors, their allies, and their opponents. Chapter 5 is particularly effective, for it systematically compares protest in several neighboring villages along an array of potential material, social, and cultural factors to ask why the responses to plans for Landfill No. 8 varied. In concluding that traditional social values and networks were important for supporting "breakaway bosses," Broadbent demonstrates concretely the types of conditions that produce a "plastic zone" in which change becomes possible (pp. 178–9). Discussions of the vertical interactions among local, prefectural, and national actors in other contexts are similarly thought-provoking.

When the author shifts from a discussion of microlevel interactions to the national political system, however, his conclusions have less power. It is one thing to produce thorough analyses of specific political dynamics involving individuals and their institutional and social contexts, as do the empirical chapters, but quite another to develop from these a generalization that will adequately account for Japan's comparative performance. Broadbent reviews several general models of power (pluralist, elitist, and class models, among others) for their applicability to the Japanese case and rejects them. Instead, he poses an alternative framework meant to incorporate the dynamism and contingency seen in the specific cases, but at this level of generality it is hardly distinguishable from the familiar models of Japanese politics. That said, the effort to develop an analytic paradigm that combines dynamism and contingency with general structural features is to be commended.

Comparativists attuned to middle-range issues will find that certain questions remain unasked. How does the pattern and pace of economic development affect the social intensity of environmental damage and its related political pressures? Do the recently democratized states in the region with similar patterns of economic development show any similarities regarding environmental effects, protest, and political response? What about those states known to have strongly clientelist politics? Although comparing Japan with its advanced industrialized peers makes sense in terms of its current global standing, given the period under discussion and the model of politics developed, at least a mention of alternative comparative lenses and cases would have been welcome.

Environmental Politics in Japan is a major accomplishment, rich in empirical research and theoretical reflection. Besides being a comprehensive ethnography, the book is complex in its use of multiple theories and analytic perspectives—it can be read and reread from a number of viewpoints. Those with an interest in social movements, protest, or environmental politics should be sure to add this to their reading list.

Women and the State in Post-Sandinista Nicaragua. By Cynthia Chavez Metoyer. Boulder, CO: Lynne Rienner, 2000. 149p. \$40.00.

Richard Stahler-Sholk, Eastern Michigan University

When the conservative Violeta Chamorro became Nicaragua's first woman president after defeating the Sandinista revolutionary government in 1990 elections, the implications for women seemed mixed. In one of the few extended studies analyzing Nicaragua's political economy through a gender lens, Chavez Metoyer contributes to the growing literature on women and the state in developing countries. She culls available sources for evidence of the effect of postrevolutionary policies on Nicaraguan women, assembling data that are enriched by interviews conducted between 1991

and 1996 period. Her interpretation of this transition draws on structural feminist and poststructuralist theories of the state as a network of power relations, with particular focus on gender.

Chavez Metoyer addresses three themes: (1) postrevolutionary gains and losses for women, (2) the social costs of structural adjustment programs, and (3) the changing role of the women's movement in Nicaragua. On the first point, she argues that although socioeconomic conditions worsened for women in the post-Sandinista era, they gained social and political space. On the second, the neoliberal adjustment policies of the Chamorro government are shown to have socially regressive effects, particularly for women, which is consistent with feminist critiques of structural adjustment. On the third point, she suggests the women's movement became more assertive and autonomous following the abrupt 1990 transition, as it confronted orthodox neoliberal policies.

Nicaragua specialists and students of revolution will benefit from this opportunity to contemplate the gender blindness that was a major flaw in the Sandinista project. Chavez Metoyer acknowledges Sandinista progress in opening social and political spaces for women, but she also notes that party control over the women's organization Luisa Amanda Espinoza Association of Nicaraguan Women curtailed its ability to press for the priority of women's issues in many areas of state policy. The study shows how insensitivity to the household-level effect of economic austerity and to the military draft widened the gender gap in the Sandinistas' 1990 electoral defeat. This gender perspective reinforces Margaret Randall's critique of the Nicaraguan and Cuban revolutions (Gathering Rage, 1992).

Chavez Metoyer's case study also provides important support for the argument that structural adjustment programs hide a gender bias, notably by promoting market-defined efficiency, which shifts costs from the paid to the unpaid (household) economy. The data are punctuated by poignant interview excerpts regarding household survival strategies. This case is also presented in the context of a brief overview of the regressive effect of neoliberal adjustment programs in Latin America, which makes it accessible to a more general readership.

Perhaps because of the focus on the state, the book is more thorough in its discussion of socioeconomic policies and electoral politics than of civil society organizing. A chapter on women's organizations since 1990 discusses the experience of three out of about 200 groups and does not fully convey the dramatic resurgence and diversification of the movement, beginning in the last years of the Sandinista government. A more radical wing of the movement took root in the mid-1980s within the women's section of the farmworkers association (downplayed on p. 29), and it flourished with the formation of the 52% Movement of independent feminists (not mentioned here). In the late 1980s and early 1990s there was a proliferation of women's centers in poor neighborhoods, women's health and popular education projects, and nongovernmental organizations that addressed aspects of the feminization of poverty. The latter included revolving loan funds, some based on the Grameen Bank model and targeting poor women, contrary to the assertion that nonconventional sources of credit remain out of reach for poor small producers, particularly women (p. 71). Also overlooked is lesbian and gay activism, which came out of the closet alongside the women's movement. The pot was beginning to boil over, even before the lid of Sandinista party discipline was lifted in 1990.

The author's assessment of the permanence of revolutionary gains for women focuses on contesting public policy and elected office. It is true that domestic relations are slow to change, but perhaps more attention to ideological and social transformation would help explain the postrevolutionary revitalization of the women's movement.

Nicaragua's political transition raises important issues regarding women's interests and the intersection of class and gender. Chavez Metoyer deftly notes the irony of Chamorro's campaign speeches celebrating the nuclear family—which does not correspond to Nicaraguan social structure, especially among the poor (p. 45)—and twice quotes her statement that she is not a feminist and does not wish to be one. "I am dedicated to my home, as Pedro [her martyred husband] taught me" (pp. 46, 98). Yet, voters, particularly women, found Chamorro's traditional mother/widow image more appealing than Daniel Ortega's macho campaign image of the spurred fighting cock. Chavez Metoyer consciously chooses to characterize Chamorro's presidency as nonfeminist rather than antifeminist (p. 52), and the agenda of the successor far-right Alemán administration as gender neutral (p. 13), despite the adverse effect on women of the shift in many aspects of state policy. Perhaps voters talked themselves into a comforting vision of escape from war and economic crisis, which were differently experienced according to gender. Or perhaps women in particular decided to use their organizing experience and consciousness gained through the revolutionary process, shed the Sandinista and feminist labels and baggage, and take their chances directly confronting the new state.

Nicaraguan women's seemingly paradoxical choice might be understood in terms of what Maxine Molyneux calls practical rather than strategic gender interests ("Mobilization without Emancipation? Womens Interests, State and Revolution," in Richard R. Fagen, Carmen Diana Deere, and José Luis Coraggio, eds., Transition and Development: Problems of Third World Socialism, 1986). Operating within the real-world constraints of existing gender constructs—and, for that matter, international political and economic power structures-Nicaraguan women increasingly sought to define autonomously their social movement strategies. The grassroots Nicaraguan and wider Central American women's organizations, galvanized but constrained by the revolutionary struggles for state power in the 1980s, began charting a distinctive course within the Latin American women's movement (see Nancy Saporta Sternbach et al., "Feminisms in Latin America: From Bogotá to San Bernardo," in Arturo Escobar and Sonia E. Alvarez, eds., The Making of Social Movements in Latin America, 1992).

An ambitious agenda is laid out for such a short book, so this work necessarily paints with a broad brush, occasionally using anecdotes to depict trends. Overall, it makes a useful contribution to an understanding of the gender effect of structural adjustment. It also suggests the continued relevance of the Nicaraguan case for understanding how women and other social sectors are affected by drastic transitions in the form and orientation of the state.

Democracy without Associations: Transformation of the Party System and Social Cleavages in India. By Pradeep K. Chhibber. Ann Arbor: University of Michigan Press, 1999. 287p. \$18.00 paper.

John Echeverri-Gent, University of Virginia'

Political scientists have generally focused on one of two variables to explain the nature of party systems. Following Duverger, many have analyzed electoral rules to account for the number of parties. Others have drawn from Lipset and Rokkan to argue that social cleavages determine the number. In *Democracy without Associations*, Pradeep Chhibber contends that when associational life is weak and the state plays an important role in economic and social life, parties can alter politically salient social cleavages and ultimately transform the party system.

This forcefully argued volume combines the study of Indian politics with subsidiary, theory-affirming case studies of Algeria and Spain in the penultimate chapter. Chhibber demonstrates an extensive knowledge of the literature on Indian politics, and he uses sophisticated techniques to test his arguments. By situating his study of Indian politics in a comparative context, Chhibber not only boldly takes on the conventional wisdom of Indian specialists but also makes a useful contribution to the theory of comparative party systems.

An important premise of the book is that India is characterized by weak associational life. This contradicts the widely accepted view among specialists that India is a highly politicized society. Chhibber's restriction of associational life to formal organizations dissolves this apparent contradiction even though it excludes informal organization, particularly India's extensive social networks, such as those that develop around the caste system. If Chhibber were to deny the existence of informal organizational forms, he would neglect the important role that they play in political mobilization. He makes this analytical distinction not to dispute the importance of informal organization but to highlight the principal and largely unmediated role played by political parties in linking these social networks to the Indian state.

Given the importance and originality of his argument concerning the weakness of India's associational life, it is unfortunate that Chhibber did not support it more carefully. Central to his contention are data from 3,000 respondents to a survey in six Indian states: Only 13% said that they belonged to an association, and 84% said that they did not belong or refused to respond to the question (p. 58). In a developing country, many people are reluctant to answer survey questions with political implications, so it is important to report the breakdown between the last two categories. Given the problems of comparative survey research, Chhibber could have built more confidence in his argument had he discussed the comparability of the Indian survey and the World Values Survey, which was the source of data about associational life in other countries.

Chhibber contends that the Congress Party can best be understood as "a collection of state parties, with the Congress Party in each state representing interests unique to its region and with a weak national organization" (p. 51). He argues that the party's state-level focus was the consequence of (1) the localized nature of social cleavages, particularly those of the caste system; (2) the single-member simple plurality electoral rule, which compels cross-caste coalitions; and (3) the federal division of power, which allots a vital role for state governments and renders local government powerless.

Chhibber's characterization of the Congress Party organization as weak and decentralized revises the conventional wisdom that stresses its resilience until the split in 1969 and then its centralization beginning under the leadership of Indira Gandhi. Chhibber's description is useful in giving patronage and localized patronage networks a prominent role in the Congress's success as a catchall party during the first two decades of the postcolonial era. His observations lead to the more novel argument that the decline of the party is due to the exit of its supporters rather than to the entry of

new voters into the electorate; after its electoral setback in 1967, intensified political competition provided defectors from the Congress with alternative avenues to political success. This helps explain why the national Congress leaders increasingly resorted to manipulating the allocation of central government resources to control state governments. Chhibber does not consider the possibility that the growing competitiveness at the state level incited the party leadership to centralize power even as local leaders defected. The cause of the party's decline may well lie in its leaders' centralization of control—through the patronage and substitution of loyal followers for more popular state leaders—at a time when the increasing competitiveness and diversity of state-level party systems undermined the prospects for electoral success of this form of party organization.

Chhibber's analysis of the transformation of the party system in the state of Uttar Pradesh (UP) and the rise of the Bharatiya Janata Party (BJP) convincingly documents the role played by political parties in converting India's catchall party system into a cleavage-based one. Chhibber persuasively depicts how the 1990 decision of the National Front government to implement affirmative action for India's "backward classes" and the subsequent BJP decision to intensify its campaign for Hindu nationalism placed the UP Congress in a position in which each caste group believed that the party favored another. The Congress's forward class supporters fled to the BJP, and its backward caste supporters left for the Socialist Party and others whose backing was more narrowly based. Chhibber attributes the ascendance of the BJP at the national level to its ability to forge a coalition among religious groups, the forward castes, and the middle

Chhibber persuasively supports his argument that partisan competition made political cleavages between caste and religious groups politically salient, but he understates the importance of other factors. Growing corruption within the Congress drove middle-class support from that party to the BJP. Furthermore, the BJP was quicker to accept the new era of coalitional politics and to develop an alliance strategy that exploited partisan competition at the state level. Finally, in various states, the BJP was able to incorporate the support of backward castes and others at the bottom of India's social hierarchy. After all, not only did the BJP make a backward caste politician the leader of the UP government, but also in the last three parliamentary elections it won an average of seven of the thirteen seats reserved for scheduled castes and tribes in UP and Bihar, states where caste-based cleavages are strongest.

The BJP's appeal to diverse social groups strengthens Chhibber's argument about the agency of parties. It suggests that in pluralist societies such as India, parties affect the political salience of social cleavages by exacerbating some while building coalitions that span others.

In sum, this important volume makes a useful contribution to the study of Indian politics by highlighting the centrality of political parties to the dynamics of the Indian polity. Its pointed argumentation is controversial but nonetheless useful in the issues that it raises. More broadly, Chhibber's arguments contribute to the study of comparative party systems by suggesting that the level of associational life is an important variable in the evolution of party systems and by demonstrating that in the context of weak associational life and strong state activism, partisan competition can transform the nature of a party system.

Leadership Selection in Six Western Democracles. By James W. Davis. Westport, CT: Greenwood, 1998. 232p. \$65.00.

Robert Elgie, University of Nottingham

The aim of this book is to fill an information gap concerning leadership selection in the United States, Canada, Britain, Germany, France, and Australia. In this respect, the author is no doubt successful. After all, comparative studies of this sort are rare, although the appearance of a special edition of the European Journal of Political Research (vol. 24, no. 3, 1993) dedicated to this theme suggests that academics are at last beginning to pay it some much needed attention.

The main argument is that across all six countries leadership selection is characterized by a number of common elements. The evidence suggests that those who come to office are experienced political figures. There is still little room for young turks, political outsiders, or first-timers. Similarly, there has been a general move toward increasing the level of popular participation in the nominating process. Even the venerable British Conservative Party has taken steps to democratize selection procedures. Equally, more attention is now being paid to opinion polling in the choice of political leaders, and everywhere television has come to play a major part in leadership campaigns and nominating conventions. In Canada at the 1993 Progressive Conservative Party convention there were 2,150 officially accredited media representatives for 3,469 party delegates. Finally, parties have shown themselves to be willing to change the rules of the game so as prevent potentially divisive contests from happening too frequently. In particular, leadership selection conventions have been separated from policyrelated conventions, so as to focus attention on the successful candidate rather than party differences over substantive issues.

In sum, the evidence indicates that it is still possible to oust incumbents or for them not to be renominated, but electoral defeat is the main reason leaders lose power. Successful candidates are invariably ones who show persistence, who benefit from good satisfaction ratings, and who have already had the experience of high political office. In addition, at least in the United States, Canada, and France, money is a key factor. Moreover, the ability to raise substantial funds is not just an essential element of an election campaign proper. It is also a key component of a successful preelection campaign because it is the first demonstration that a candidate can appeal to important electoral constituencies. By focusing attention on issues such as these, there is no doubt that this book sheds some light on how leaders come to power from a cross-national perspective.

Whatever its merits, the book has a number of problems, including factual errors. I have expertise in French politics, and in that chapter alone it was easy to spot a number of rather startling mistakes. Chirac did not step down from the prime ministership before the 1988 presidential election (p. 147). The correct spelling is Brice Lalonde, not LeLonde (p. 149). Dominique Voynet is a woman, not a man (p. 149). President Mitterrand's biographer is Wayne Northcott, not Southcott (p. 197, n. 4). Similar errors were not immediately obvious in other chapters, but these raise doubt as to the factual accuracy of the text as a whole.

More important, the coverage of the chapters is not uniform. The chapter on the United States provides a good thematic overview of how presidential candidates come to be selected and how nomination procedures have changed over time. By contrast, the French case comprises little more than a potted history of recent presidential election campaigns. Even the chapters on Australia, Britain, and Germany tend

to tell the story of the rise and fall of individual parties and their leaders at the expense of systematically addressing a uniform set of themes. More important still, it is not always obvious that like is being compared with like. For the most part the book is concerned with the selection of party leaders, but in the United States the president does not formally hold this position. Moreover, there is little or no mention of U.S. institutions that might be considered more comparable to European or Westminster-style party institutions, such as the chairs of the Democratic and the Republican National Committee, or indeed the Speaker of the House or the Senate majority leader. Even when party leaders are clearly being examined, it is not obvious in some cases why certain parties have been chosen for study. The main criterion seems to be parties that have produced presidents or prime ministers in the countries examined, but in Australia the National Party is discussed, although its leader has never been elected prime minister, and in Germany the Greens are mentioned, even though they have never produced a chancellor.

This is an overarching and, for the most part, wellresearched text, but there is a strong sense that much more can be learned about leadership selection from a comparative perspective. To this end, is it possible to establish a comprehensive list of selection processes? To be sure, the author identifies a variety of such procedures, including ones that are confined strictly to legislative representatives, those that make provision for a more or less inclusive comprehensive electoral college, and those that are much more open to popular participation altogether. The question still remains as to whether it is possible to construct a general typology of cross-national selection processes. In a similar way, do particular forms of selection procedures apply to only certain types of countries? Does the difficulty of introducing a system of presidential primaries in France indicate that this sort of procedure is applicable only to presidential systems, rather than to semipresidential countries such as France, never mind parliamentary countries such as Britain? All told, the failure even to address let alone answer these questions, and others like them, suggests that there is still ample room for a comprehensive book on leadership selection from a comparative perspective.

Reconstructing the State: Personal Networks and Elite Identity in Soviet Russia. By Gerald M. Easter. Cambridge: Cambridge University Press, 2000. 221p. \$54.95.

Judith Kullberg, University of Michigan

The collapse of Soviet communism intensified the long simmering debate over the nature of Soviet political reality and prompted a reexamination of the major models of the Soviet system. Although many scholars have contributed to this exchange, relatively few have devoted themselves to the next task: creating new, theoretically informed interpretations that exploit the wealth of information in previously inaccessible Soviet archives. Reconstructing the State is one of the few attempts to apply a fresh approach to new sources of information. In laying out a possible path of inquiry, the book takes up the serious work of theoretical reconceptualization of Soviet politics.

To explore anew the essential nature of the Soviet polity, Easter turns to the formative period from the mid-1920s through the late 1930s, the tumultuous era that encompassed Stalin's consolidation of power, the revolution from above, and the great purges. His central question is how the new, infrastructurally weak Soviet state rapidly expanded its reach

and radically transformed social and economic life in the period of the first five-year plan. He argues that previous answers to this question, which emphasized the organizational strength of the Communist Party, role of the secret police, or the mobilization and exploitation of social groups, are incomplete. In Easter's view, personal networks within the party leadership are the long overlooked piece of the puzzle that can account for the growth in state capacity and the success of economic transformation.

The empirical support for this argument is an examination of the role of the twenty individuals who held the position of first secretary in regional administrations during the Stalin era. According to Easter, these officials, whom he dubs the "Provincial Komitetchiki," or provincial committeemen (hereafter PK), constituted a distinct elite grouping. A number of factors linked the PK: humble social origins; similar or shared experiences as underground party workers before the revolution; strong personal bonds forged in the civil war; and a common heroic identity derived from Bolshevik victory in that conflict. Easter asserts that after the civil war the informal elite networks to which the PK and their wartime comrades belonged intersected with the formal organization of the Soviet state. The networks expanded outward from the local level, as network members were appointed first to positions within various regional administrations and later to high leadership positions in Moscow. Upon taking office, they in turn assigned fellow network members to positions within regional and local administration. Personal networks thus served as a sort of trellis upon which the state administration grew. Furthermore, by compensating for the lack of a communications infrastructure and weakness of local administration, networks enhanced the reach of the central state. The PK, for example, used their informal networks to transmit central directives to the local level, coordinate action, and rapidly allocate critically needed resources.

Although personal networks within the party elite thus facilitated the achievement of the regime's goals, Easter emphasizes that they also constrained the actions of central officials. He contends that in the early to mid-1930s the PK used the power resources of their informal networks and shared status as heroes of the civil war to impede temporarily Stalin's development of a despotic, command-administrative system. From this he concludes that the PK preferred a "protocorporatist" (p. 132) order that would accord them substantial autonomy in their domains and a right to participate in the formulation of national policy. Precisely because of this preference, however, the "central state" destroyed the PK along with virtually the entire old Bolshevik party in the purges of the late 1930s.

The chief value of Easter's study is that it illustrates the possibility of connecting the study of Soviet politics to the comparative mainstream. The elite network approach fits squarely within the theoretical tradition established by Mosca and Pareto, which has sought to demonstrate the profound effects of elite group characteristics on a nation's politics. The study also is part of a new genre of work on elites, exemplified in the work of Catherine Boone, Robert Vitalis, and Francis Hagopian, that attends to the ways in which intra- and interelite alliances and loyalties influence state formation and development. Because the book also contributes to the rich body of empirical research on elite structure and patronclient relations in communist nations, it lays the foundation for one of many possible bridges between comparative theory and communist and postcommunist studies.

Yet, however theoretically promising the elite network approach, the book falls short of persuading the reader that it offers a superior interpretation of Soviet politics or a more complete explanation for the success of Stalin's "great change" than those offered by such respected scholars as Stephen Cohen, Robert Conquest, Merle Fainsod, Sheila Fitzpatrick, or Moshe Lewin. The reason is that the author does not operationalize his key independent variable, the personal network. He neither precisely explains what constitutes a personal network nor describes how the presence of personal bonds between particular individuals can be ascertained. A table of the purported links between the PK and various individuals in the Politburo is presented (p. 81), but there is no discussion of the criteria and data used to identify the links. Moreover, Easter does not discuss how such networks are analytically separable from similar phenomena, such as the links between individuals that arise purely from location within the same or related organizations.

The lack of operationalization deprives the author of the analytical tools he needs to make a convincing case. The lines between formal organizations and informal networks in the accounts of events are blurred. Personal networks are everywhere, but institutions, other than those of the central state in Moscow, hardly seem to be present. Without a means of distinguishing between institutions and personal networks, Easter cannot demonstrate precisely how networks contributed to the strengthening of the state or constrained the actions of the central state leaders. He is also unable to assess the significance of elite networks relative to the particular institutions that played a major role in the revolution from above, namely, the party and the secret police.

A related problem is that the author repeatedly identifies personal networks as the primary or only cause of important historical events even when the facts suggest otherwise. A prime example is the interpretation given for the "Georgian affair," in which Stalin and Ordzhonikidze used strong-arm tactics to purge the Georgian Bolshevik party. Easter depicts the incident as a struggle for control between two rival Georgian Bolshevik networks, Ordzhonikidze's and another led by Mdivani and Makharadze. In contrast, most scholars have interpreted the purge as a move by Commissar of Nationalities Stalin (who is not even included in Easter's account as a major actor in the incident) to curb Georgian autonomy. This interpretation was first given by Stalin's contemporaries, including Lenin, who recommended in his deathbed "Testament" that Stalin be removed from the position of general secretary in large part because of his treatment of the Georgian Bolsheviks.

Although the empirical support for the claimed significance of personal networks is not compelling, this is not due to the absence of key evidence but to the lack of scientific rigor common in exploratory studies. Easter leaves little doubt that personal networks within the Soviet Communist Party existed and certainly puts forward enough evidence, much of it recently culled from party archives, to suggest that they played a key role in the formative period of state development. Because it offers a theoretically sophisticated approach that may yield important insights into the nature of Soviet political reality, the book deserves scholarly attention.

The Real Worlds of Welfare Capitalism. By Robert E. Goodin, Bruce Headey, Rund Muffels, and Henk-Jan Dirven. New York: Cambridge University Press, 1999. 358p. \$59.95 cloth, \$22.95 paper.

John D. Stephens, University of North Carolina, Chapel Hill

The first two sentences of the preface succinctly state both the breadth of analysis and the goals of this study. "This book represents an unusual collaboration between a philosopher, a political scientist, an economist, and a sociologist. The preoccupation we share is with the central question, 'What are the best institutional arrangements for promoting each of a variety of social, political, and moral values which the welfare state has historically been supposed to serve" (p. vii)? With Gøsta Esping-Andersen's Three Worlds of Welfare Capitalism (1990) as a point of departure, the authors attempt to investigate empirically which of the three welfare state types best meets its own self-proclaimed goals, as well as those of the other two types. They argue that it is not adequate to examine social rights of citizenship, that is, the formal entitlements of welfare states, as Esping-Andersen or Walter Korpi and Joakim Palme have done. Not only may these be misleading—because, for example, take up rates may be low—but also claims to superiority of liberal welfare states, for example, are based precisely on the contention that by minimizing government intervention with the market, economic growth and thus long-term material welfare are max-

The authors want to know how citizens really fare in these regimes over the long run. The long view is necessary not only to include effects of growth on material welfare but also because, as they rightly argue, for goals such as poverty reduction one should be primarily concerned not about whether a proportion of the population is poor in one year but about whether they remain so over an extended period. This concern with the long run leads the authors to choose panel data as the appropriate basis for their investigation. Adequate panel data over ten years are available for the United States and Germany, which are typical, even prototypical, of a liberal and corporatist state and a Christian democratic welfare state, respectively. Unfortunately, no such data exist for any of the Nordic countries, the obvious candidates for the social democratic type, so the authors are forced to select the Netherlands as their representative. The Dutch welfare state is the best candidate outside the Nordic countries, but it does deviate from the social democratic type in important ways (noted below), which could significantly affect some of the conclusions of the analysis.

In the initial section of the book, the authors outline the moral values and social and economic theories advanced by the proponents of the three types. For example, advocates of the liberal welfare state value economic efficiency and propose to maximize welfare by limiting government intervention. Their solution to poverty is to maximize growth, encourage private philanthropy, and limit government intervention to transfers tightly targeted at the truly needy in a fashion that will not reduce work incentives. The authors take the goals of each regime at face value: It is possible, they say, for each to maximize its own goals and not those of the others and thus to succeed on its own terms. This section identifies six goals that are valued differently by proponents of the three types: promote efficiency, reduce poverty, foster equality, enhance integration, promote stability, and encourage autonomy.

The middle section of the book is devoted to evaluating the success of the three countries in meeting the six goals. The authors devise multiple tests for judging success. In most cases, they compare market (pretax, pretransfer) outcomes to postintervention outcomes, as these differences figure importantly in the social and economic theories that underlie the three regimes. Since there are limitations on what can be gleaned from the panel data on income development, these tests vary in face validity. Few will dispute their results on poverty or inequality, but many might question whether the measures of autonomy or integration tap very accurately what social scientists, political philosophers, or politicians

mean by these terms. Nonetheless, I found the chapters on integration, stability, and autonomy elucidating, and the chapters on efficiency, poverty, and equality, which did not suffer from measurement problems, were quite convincing.

The authors then test whether the regimes met their goals, to the extent that they did, for the reasons posited in the underlying social and economic theories. I found this third section to be the most original and interesting part of the analysis. The basic finding is that any success of the liberal and corporatist regimes was not due to the reasons posited in the theories. In liberal theory, the market should have accomplished much more, and in corporatist theory, organizations in civil society much more, whereas in both cases, to the extent that goals other than economic growth were met, success was accomplished through government action. By contrast, in social democratic theory, government action is central, and the goals were achieved through the predicted mechanism. The authors' very provocative conclusion is that "the social democratic welfare regime is 'the best of all possible worlds' . . . no matter which of [the six] goals you set for your welfare regime, the social democratic model is at least as good as (and typically better than) any other for attaining it" (p. 260). This does not mean that every test favored the social democratic regime. For instance, the liberal regime did best in providing everyone with work, as labor force participation was highest there. Taken as whole, however, the data support the authors' conclusion.

It may be somewhat surprising that the liberal regime, represented by the United States, did not fare the best in increasing average material welfare. The period chosen, 1983-92 for the United States and 1985-94 for Germany and the Netherlands, certainly affects the results for growth in per-capita income, which was very similar for the three countries; in a more recent period, the Netherlands still maintains parity with the United States, with Germany lagging behind. Moreover, growth in per-capita income is not the authors' favored indicator of improvements in real standard of living. They argue, correctly in my view, that real equivalent (i.e., adjusted for household size) household income is a better measure, and on this measure, expressed as the change in the median or in proportion of households showing improvement, the Netherlands and Germany perform much better than the United States.

I see two major flaws in the analysis. The authors are aware that the choice of the Netherlands as exemplar of the social democratic regime is problematic. Since the panel data confirm in most cases a pattern found in annual surveys, I would have added one of the Nordic countries to the analysis as a better example, albeit one with deficient data. This would have led to different and more positive evaluations of social democratic regimes on at least three indicators: total labor force participation, pregovernment poverty, and welfare dependence. All these are related to a key difference between Nordic and Christian democratic welfare states, with the Netherlands being solidly Christian democratic in this case: The Nordic regimes are characterized by extremely high labor force participation among men and women, whereas Christian democratic welfare states have the lowest levels of the three regime types, due mainly to low labor force participation by women but also to such reduction strategies as early retirement or disability pensions for elderly workers (i.e., "welfare dependence" by the authors' measure).

The distinctiveness of the Nordic countries with regard to women in the labor force takes me to my second objection. The book overlooks more than a decade of work by feminist comparative welfare state scholars and presents an almost completely ungendered analysis. In the past three decades,

gender equality has become a central goal of the social democratic regime, and it should have been added as a seventh goal. The outcome would have been the same as for the other goals: We know from the work of Barbara Hobson, Diane Sainsbury, and others on Luxembourg Income Study (LIS) data that the social democratic welfare states would come out on top as the most gender egalitarian.

Despite its flaws, The Real Worlds of Welfare Capitalism is a major contribution to the comparative social policy literature. It compellingly joins the normative and empirical in an explicit fashion, and it throws down the gauntlet to the neoliberals. I sincerely hope that they will attempt to refute the authors' analysis empirically, but I fear that we will be treated to the same litany: The book's results on growth and efficiency cannot be right because they violate what we "know" about work and investment disincentives imposed by generous welfare states.

The Faces of Janus: Marxism and Fascism in the Twentieth Century. By A. James Gregor. New Haven, CT: Yale University Press, 2000. 256p. \$30.00.

Phoenix Fascism in Our Time. By A. James Gregor. New Brunswick: Transaction, 1999. 208p. \$32.95.

Franklin Hugh Adler, Macalester College

A. James Gregor occupies a singular place in the study of fascism because he has always gone against the tide of conventional wisdom in advancing two fundamental claims. First, Italian fascism possessed an ideological unity and political coherence unacknowledged in the largely misinformed professional literature. Second, Italian fascism represented a paradigmatic form of developmental dictatorship that would serve as a model for subsequent "progressive" movements and revolutions in the twentieth century. During the 1970s Gregor articulated these claims in a number of major monographs that tended to be poorly received or dismissed. With the unanticipated collapse of the Soviet Union in the wake of 1989 and the appearance of "fascist" tendencies where Marxism-Leninism had once reigned supreme, Gregor might very well have the last laugh, and he has begun to update his earlier work accordingly. The two books in question are part of this revision.

The Faces of Janus begins by frontally attacking the familiar Left-Right distinction (the Left is unambiguously progressive, and the Right is unambiguously reactionary). This distinction not only is historically suspect, according to Gregor, but also from the very outset was ideologically instrumentalized by partisan Marxist intellectuals and their "mesmerized" colleagues, intent on privileging purported movements of the Left and demonizing movements of the Right. Following the lead of Renzo De Felice, Italy's most distinguished historian of fascism, Gregor decouples Mussolmi's regime from that of Hitler, stressing not only its progressive, developmental tendencies but also its undeniable and distinctive origin as a heretical movement of the revolutionary Left. Much of the book is devoted to demonstrating that Marxist analyses of fascism, in different epochs and in different forms, were myopic, contradictory, and fundamentally erroneous.

The tone is often shrill, the style bombastic, the claims exaggerated; in short, vintage Gregor. There are curious absences: nothing whatsoever on the Frankfurt School or Antonio Gramsci. Least satisfactory is his treatment of critical, independent neo-Marxist and New Left scholarship of the late 1960s and 1970s, reduced by Gregor to Maoism, dependency theory, or the work of Nicos Poulantzas. Little

substantive account is given to contributions by the German and American New Left that had nothing to do with these positions, analyses articulated in such journals as Das Argument, New German Critique, and Telos (where Gregor obtained some of his most interesting material, such as Mihaly Vajda's 1971 and 1972 essays on the progressive nature of Italian fascism). Notwithstanding the polemical nature of Gregor's critique, however, his analysis is substantially correct, especially the untenability of traditional class analysis or later structuralist accounts inspired by Poulantzas and Louis Althuser.

In the most interesting part of the book, Gregor turns the tables on Marxism-Leninism and uses "paradigmatic fascism" as a model to explain the evolution of Russian politics, from the genesis of Stalinism through the post-1989 rise of what he calls "reactive, developmental nationalism" in Russia, especially the "proto-fascist" turn from orthodox communism toward what Marxists themselves used to call "national chauvinism," with its explicit rejection of internationalism and stridently emotional appeal to "nationalpatriotic forces." Gregor refers here to positions adapted by Gennadi Ziuganov, leader of the Communist Party of the Russian Federation, as well as Sergei Kurginian and Alexander Prokhanov in their publication, "Word to the People." An important source of this new nationalist program is the "ethnogenetic Eurasianism" of Lev Nikolaevich Gumilev, a form of ethnobiological "racism" that Gumilev claims is fully compatible with historical materialism. In fact, his major work, Ethnogenesis and the Biosphere, was issued by a state publishing house before the definitive collapse of the Soviet Union. Even if one is not totally convinced that the cunning of historical reason is fully on the side of Gregor, it is nevertheless the case that he offers us a powerful alternative explanation of the tortuous relationship between Marxism and fascism.

Phoenix: Fascism in Our Time is a theoretical reformulation of Gregor's earlier work on fascism as a developmental dictatorship, adding a new emphasis on "collective humiliation" as a psychological factor that both precipitates and pervades a reactive nationalist response. Gregor draws extensively on Italian intellectuals (Enrico Corradini, Alfredo Rocco, Roberto Michels, Sergio Panunzio, Giovanni Gentile, Giuseppe Bottai) who alchemized separate strands of nationalism, revolutionary syndicalism, idealism, and corporatism into a purportedly coherent, unitary developmentalist ideology that Gregor calls "paradigmatic fascism." As in The Faces of Janus, Gregor then applies a model of paradigmatic fascism to postsoviet Russia, where the emphasis on collective humiliation, resentment toward the West, and restoring the nation to the rank of a major power is especially striking.

Methodologically, Gregor commits the same error that haunted his earlier work, the tendency to conflate ideology with actual regimes and their political histories. At one level, the various aspects of fascist ideology can certainly be made whole, but an examination of the dynamics of the Italian fascist regime and its political trajectory yields a muddled conclusion at best regarding both the regime's unity and its commitment to developmentalism. More often than not, nationalists, revolutionary syndicalists, and technocratic corporatists were at one another's throats, moderated and restrained by Mussolini and his distinctive personal agenda. Alfredo Rocco, as Minister of Justice, was a confirmed statist at odds both with Edmondo Rossoni, leader of the fascist labor syndicates, and Giuseppe Bottai, Undersecretary of Corporations. Rossoni, an unreconstructed revolutionary syndicalist, promoted an autonomous, anticapitalist syndical state (sindacalismo integrale). Bottai sought to replace the

existing state apparatus with a new, revolutionary corporatist order based on functional hierarchy and technical competence. He ridiculed the authenticity and the representative pretenses of the fascist labor syndicates, which could only function when nonfascist unions were made illegal, as well as the political ambitions of the plebeian, self-aggrandizing Rossoni, with whom he actually came to blows.

The technical aspirations of early fascism (e.g., technical councils), as Gregor underscores, were never minimally realized, much to the dismay of Bottai and Ugo Spirito, who both conceded that the new corporatist order was effectively stillborn. This, too, was the view of Mihail Manolesco (Le siècle du corporatisme, 1938), a leading authority on interwar European corporatism. As for development, the most impressive growth rates occurred during the early "liberal" phase of fascism (1922-28), before an integrally fascist state was institutionally articulated. Obviously, the effect of the Great Depression complicates any definitive judgment regarding the regime's economic performance during the 1930s, although the norms that guided state intervention became increasingly more bureaucratic, cumbersome, and sclerotic. In fact, Renzo De Felice, to whom Phoenix is dedicated, depicts the 1930s as a period when fascism (as a distinctive ideology and as a distinctive regime type) became progressively marginalized by ducismo and the creation of a personal dictatorship. Such a record offers little support to Gregor's claims regarding Italian fascism's unity and developmentalism. Alberto Aquarone, in measuring the regime's revolutionary claims against actual performance, commented that these were più fumo che arrosto (more aroma than beef).

Although not fully convincing, Gregor's alternative conceptualization of fascism certainly merits further theoretical refinement and empirical application. For this to happen, Gregor must better specify its comparative range and denote its limits. One sometimes has the impression from his work that all oppositional movements are fascist, or potentially so; that fascism necessarily accounts for or exhausts all revolutionary possibilities. This hyperextension of the concept, in fact, is the reverse side of the analytic problem with which Gregor began, confining the concept of fascism to manifestly reactionary movements and exaggerating the Left-Right dichotomy. With Gregor's overly expansive concept of fascism, we run the risk of purchasing generality at the expense both of comparative taxonomy and historical specificity. Gregor has yet to prove persuasively that the product is worth the price.

Political Corruption. Edited by Paul Heywood. London: Blackwell, 1997. 230p. \$75.00 cloth, \$35.00 paper.

Arnold J. Heidenheimer, Washington University in St. Louis

This collection of twelve articles is the product of a workshop held at the University of Nottingham to which British political scientists invited contributors from several other countries and disciplines. The focus is partly on recent national experiences with problems of corruption and partly on theoretical models that analyze its nature and effects. Economists provide more of the latter, and political scientists offer more of the former with regard to selected countries in Western and Eastern Europe and in Asia.

Dawn Oliver delimits the British experience in combating corruption to parliamentary self-regulation of standards of conduct. Examining the scandals that led to the formation of the Nolan Committee and how its recommendations were implemented, she concludes that the MP transgressions under scrutiny were "relatively minor in-

stances of struggles for resources in politics," and she tends to view them as instances of "slackness" rather than corruption (p. 131). But Oliver concedes that the 1997 election showed that public confidence "cannot be taken for granted" (p. 131).

Donatella Della Porta and Alberto Vannucci analyze the recent Italian corruption landscape via a broader focus on how clientistic practices erode institutional autonomy, holding that "clientelism and corruption converged to form a spiral, the diffusing of one aiding that of the other." (p. 118) The question of how much they are prerequisites or correlates is not systematically explored. It might be asked whether, if clientelistic or other exchange relations are an essential correlate, there can be such a type as "autogenic" corruption, which involves abuse of office by just one person.

Is it more worthwhile to focus on what Della Porta and Vannucci call the "vicious circles" of clientelism-corruption-clientelism or to develop the corruption cycles framework of Christina Bicchieri and John Duffy? The latter presuppose that "corrupt incumbent politicians will compensate contractors for being in a prisoner's dilemma until they run out of funds" (p. 78). Cyclical effects are alleged to flow from the proclivity of politicians to offer the contractors too much excess compensation.

The corruption and clientelism paradigms are related to the rent paradigm in a theoretical discussion of development problems in the Third World by Paul Hutchcraft. He traces the ideological affinities of corruption scholars from their early base in modernization theory and argues that it is wrong to regard corruption as merely a "primitive way of conceptualizing rent-seeking" (p. 226). He goes on to tackle such questions as whether a political system generates an internal sense of limits able to mitigate the extent of corruption.

Two other articles deal with corruption phenomena in postcommunist Russia (Federico Varese) and Ukraine and the Czech Republic. For the latter a team led by William L. Miller builds on focus group discussions in which participants were asked to rank bribery along with other means of persuasion in dealings with officials. They found that allegations about the need to use bribery were more than twice as frequent in reports based on personal experience.

The subsequent European Commission scandals are anticipated in an article by John Peterson, which found the EU relatively corruption free but dogged by extensive fraud and pathological noncompliance. Evidently, the working rule of putting the internal monitoring mechanisms in the hands of Scandinavians and redoubtable Dutchmen such as Florus Wijsenbeek did not work quite well enough, thus leading to the forced exit of many commissioners.

Unlike the legion of economists who have recently entered corruption research, and who avoid problems by simply ignoring questions of definition, Mark Philp takes these seriously. He doubts whether the conventional way of ascribing the work of many economists to "market-based" definitions is really well founded. He pleads for the elimination of this category, since "what defines an act as corrupt is not that it is income maximizing, but that it is income maximizing in a context where prior conceptions of public office and the principles for its conduct define it as such" (p. 28).

This collection enhances corruption research through its numerous examples of sophisticated analysis and model testing.

In the Name of Liberalism: Illiberal Social Policy in the United States and Britain. By Desmond King. Oxford: Oxford University Press, 1999. 340p. \$65.00 cloth, \$19.95 paper.

Lawrence M. Mead, New York University

Desmond King is a British policy historian who focuses on British and American work and welfare programs. In Actively Seeking Work? (1995), he argued that government agencies in both countries have focused too much on enforcing the requirement that benefit recipients look for work and not enough on enhancing claimants' skills and improving opportunities.

In the Name of Liberalism broadens that argument to suggest that liberal democracies have inherent tendencies to illiberalism. They are supposed to value freedom and treat individuals equally, but leaders also want people to possess the self-reliance needed to live independent lives. Therefore, policies sometimes try to exclude the incompetent from the full rights of citizenship or to inculcate correct attitudes in the less functional. Such policies are illiberal but claim to serve liberal ends. They are also promoted by experts who influence politicians with the authority of science.

King offers three instances. The first is the eugenics movement of the early twentieth century. In both Britain and the United States, scientists who thought dysfunctional traits were inherited urged governments to promote sterilization of the unfit, lest their progeny burden society and adulterate the race. The second instance is the work programs set up by both governments during the Depression to provide work for jobless youth and so maintain their morale (the American case is the Civilian Conservation Corps). The third example is the effort by both governments to require able-bodied adults on welfare to work in return for their support. "Workfare" goes back at least to the British New Poor Law of 1834. It took on new life in the 1980s and 1990s when, after several decades of laxity, both countries imposed stiffer requirements on means-tested and programs. In the United States, the movement was led by Republicans, who in 1996 passed the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA). In Britain, Conservatives began the reform, but the New Labor government has continued

The cases are grounded in solid documentary research. King is correct that eugenics and workfare, if not the Depression work programs, had the weight of expertise behind them. But his presentation of the three cases is often wordy and repetitive, and the idea that they manifest an illiberal tendency in democratic polities fails to persuade. The views of the eugenists, which King quotes at length, were certainly illiberal, but he cannot show that they had much actual influence on policy. Although most states in the United States permitted or even imposed sterilization on the mentally impaired for a time, the reason may have been the practical one that incompetent adults cannot take care of children, not that they were viewed as subhuman. In Britain, political opposition was strong, and the government refused to recommend even voluntary sterilization. King claims that the movement helped bring about the curbs Congress imposed on immigration to the United States during the 1920s, but he admits other causes, such as rivalries among existing ethnic groups. As for the work camps, to threaten liberal democracy they would have had to be permanent and compulsory as well as indoctrinate their inmates, as in Nazi Germany.

But they were temporary and voluntary and made little attempt to shape attitudes.

The workfare movement is more important, because it is longstanding and makes clear-cut demands on aid recipients. The revival of work tests is the leading recent change in social policy in both countries. It also is the case that animates King the most. But is workfare really illiberal? It may appear to be, but in the end it may serve liberal ends. Work requirements seem to coerce people, but even a polity dedicated to freedom must engage in some social control. It must enforce some set of minimal social competencies, above all working, just as it enforces the criminal law. Otherwise, the mutual trust essential to a free society would decay. King seems to regret this, but he does not contest it openly. He actually admits that workfare has "broadened" liberal democracy by bringing its obligatory aspects into view (p. 308).

Workfare also may seem illiberal because it is invidious. Government enforces work on benefit recipients but not on other people. As King says, work tests "create differential experiences of citizenship" and "foster distinctions" among citizens (p. 290). But government faces a dilemma. Prolonged, nonworking dependency among the able-bodied already creates differences between citizens. The dependent, if employable, are looked down upon precisely because they are nonworking. Workfare aims to minimize distinctions by balancing support with a work obligation within welfare, just as the nondependent must work to obtain income outside government. Enforcing work ultimately vindicates the liberal value of equal citizenship. Full disclosure: My arguments for this in Beyond Entulement (1986) and other books are the main butt of King's argument.

Perhaps the objection is not to the demands of workfare but its paternalism. Workfare tells some people to work, when self-interest should already lead them to do so. It thus demeans them. King is exercised that the eugenists reasoned in so superior a way about how to solve the social problems of the less fortunate. But the moral is only that a liberal society must undertake its social control with passion and authenticity, not antiseptic detachment. Elected politicians must finally be in charge, not experts, and they must not condescend. But that is exactly how work requirements arose in both countries: Reformers imposed them out of anger, less at the recipients than at a permissive regime that had failed to enforce the common obligations of citizenship.

A surprising fact stands behind this book, and King is candid about it. Government has embraced workfare, at least in these two countries. Its success to date is clear. Coupled with a good economy, the policy has driven down the welfare rolls in the United States by more than half with "no direct evidence of mass impoverishment" (p. 284), although evaluations are incomplete. Yet, almost all British academics who study social policy are opposed. They remain wedded to the view, championed by T. H. Marshall and Richard Titmuss. that social programs should extend only rights and benefits to claimants, without behavioral conditions (pp. 222-5, 246-7). King could have added: Almost all American academics think the same. The expert influence of which he speaks comes almost entirely from "mavericks in think tanks" and other advocates "at the margins of the welfare reform debate and marginalized in the social policy intellectual community" (pp. 224-5, 235). There would appear to be something wrong, not with liberal democracy, but with the academic establishment.

Crafting Coalitions for Reform: Business Preferences, Political Institutions, and Neoliberal Reform in Brazil. By Peter R. Kingstone. University Park: Pennsylvania State University Press, 1999. 284p. \$60.00 cloth, \$22.50 paper.

Jeffrey Cason, Middlebury College

An enduring puzzle for Latin Americanists is how freemarket, neoliberal economic reform was possible. To the extent that such reforms harm labor or the poor, there is no puzzle, since these groups have not wielded a great deal of political power in the region. But to the extent that such reforms harm business interests, the puzzle becomes interesting. Even though business in the region has not always presented a coherent or consistent political agenda, it does have substantial influence over politics in most countries. Therefore, business support for neoliberal reform, which undid decades of inward-oriented economic policies from which business benefited, is more difficult to explain. In this excellent new book, Peter Kingstone addresses the puzzle directly and effectively. He frames it as follows: "After fifty years of protection and nurturing by the state, observers of Brazilian political economy would have expected Brazilian industrialists to actively oppose the reform process" (p. 1). In the end, however, Kingstone documents that business was much more flexible when facing reform, and under certain circumstances it was quite willing to endorse and support economic reform.

"Under certain circumstances" is a key qualifier in Kingstone's analysis, particularly since the author explores a number of instances in which business mobilized politically against reform. When it supported reform, however, this occurred for clear reasons. Chief among them were (1) the untenable nature of the status quo, (2) the credible promise of long-term benefits for business in exchange for going along with reform, and (3) predictable and regular access to policymakers. If any of these conditions did not hold, then it was much more difficult, if not impossible, for Brazilian governments to get business support for a reform agenda.

Kingstone makes his case by telling two quite different stories. The first (and most interesting for the nonexpert in Brazil) is the story of economic reform in the 1990s. Kingstone has a good feel for and understanding of the political dynamics of Brazil in the previous decade, and he tells the tale of reform failures and successes quite well. Throughout, the focus is on the relationship between business and the state.

The second story revolves around the responses of three industrial sectors—auto parts, pulp and paper, and machine tools and equipment—to the changes imposed by neoliberal reform. The sectors had very different levels of competitiveness and thus responded quite differently. Pulp and paper was quite prepared for economic opening, but the other two sectors faced more serious problems. In the end they were forced to adjust rapidly, and Kingstone illustrates the variety of strategies available to firms, which included selling out to foreign capital or domestic competitors, joint ventures with foreign firms, or going bankrupt and exiting the sector. Kingstone makes an obvious but sometimes overlooked point in his sectoral analyses: We cannot talk about business generally if we want to understand business preferences; we have to talk about sectors. Even one of the more generalizable conclusions about neoliberal reform—that it has led to an increased importance of multinational capital-does not apply across the board, as Kingstone shows in the pulp and paper sector.

In both his macro and micro analyses, Kingstone uses literature from both economics and political science. In the

end, however, he relies on political explanations to explain support for and opposition to neoliberal reform in Brazil. This is a wise choice, since undertaking such reform is an inherently political process, as many have noted. In Brazil, as Kingstone points out, the political problems associated with imposing neoliberal reform are particularly daunting: a constitution that makes it difficult for the central government to control expenditures and gives states (in Brazil's federal system) a great deal of leeway in using federal resources; political parties that are hardly well disciplined; and a tradition of political patronage that sucks resources from the national treasury and demands a high financial price for political support. Nonetheless, in spite of these challenges, Brazil has been able to carry out reform, even if this has occurred in fits and starts. Kingstone does an outstanding job of showing how these fits and starts have been conditioned by leadership styles, international economic shocks, and business strategies.

One of the most important points Kingstone makes is that business is quite ambiguous in its general approach to economic reform. That is, it is quite "available" for political mobilization in favor or in opposition to neoliberal reform. At particular times it both supported and opposed Fernando Collor's economic reforms (chap. 5), and it has both supported and opposed current president Fernando Henrique Cardoso's reforms (chap. 6). In other words, business cannot be taken for granted, and skillful leaders will be able to pull business in particular directions. Kingstone (rightly) concludes that Cardoso is simply a better politician than Collor; he more effectively "crafted" the necessary coalition to see reform through.

Finally, Kingstone does not simply provide impressive empirical detail on the Brazilian economic reform process; he makes an important methodological contribution as well. He attempts to bridge the unfortunate divide between more deductive formal research strategies and more empirical and comparative-historical work. As Kingstone points out, deductive work can help clarify our analyses and expose our assumptions, and he uses this work precisely for this purpose. At the same time, Kingstone's empirical analysis of the behavior of Brazilian business elites demonstrates that business preferences for policy are quite malleable and subject to political manipulation. At any given point, most business actors have more than one business strategy available to them. Therefore, it is their interaction with the political world that determines which option they choose. Kingstone does an admirable job of making this point, and many others, in Crafting Coalitions for Reform.

Bicycle Citizens: The Political World of the Japanese Housewife. By Robin M. LeBlanc, with a Foreword by Saskia Sassen. Berkeley, Los Angeles, and London: University of California Press, 1999. 263p. \$40.00 cloth, \$24.95 paper.

J. A. A. Stockwin, Nissan Institute of Japanese Studies, University of Oxford

Few commentators on the politics of Japan are impressed by the intensity of engagement with politics of Japanese citizens. There has been a tendency in the past decade for voting turnout rates to decline and for ordinary people to dismiss the "antics of politicians" as largely irrelevant to their real concerns. According to Ronald J. Hrebenar (Japan's New Party System, 2000, p. 22), "in the Japanese system, an important characteristic is weak identification with the political parties in the system." In a recent Asahi poll, 40% of respondents declared no party preference. Japanese politics

has even been dubbed "spectator democracy" (Hrebenar, p. 28).

Robin LeBlanc argues that the avoidance of politics by sections of the citizenry is a protest against the often corrupt and hypocritical attitudes of politicians and a system that seems to corrupt even those who enter it with the best of intentions. She contrasts the deliberate avoidance of political involvement as a form of protest with the argument of de Tocqueville that grassroots participation in politics contributes to democracy in a society based essentially on market principles. The central metaphor of the book pits those who travel by bicycle against those who travel by taxi. The latter are politicians, whereas the former are shufu, a word usually translated as "housewife." The term, however, has rather complex historical connotations, and today it tends to connote shallowness and lack of ambition among women who are content to stay at home and engage in rather aimless pursuits.

LeBlanc examined groups of shufu in a crowded suburb of Tokyo, full of high-rise apartment blocks, where husbands commute on average an hour to their place of employment and typically return home late at night. She found that many of the women she studied were involved in volunteer activities, and much of the book focuses on an organization set up by local shufu, called Sagrosho (literally, "work place"), where goods are bought and sold for the benefit of handicapped people, who themselves participate. In the course of LeBlanc's fieldwork, Sagyosho developed from a small-scale and extremely informal operation to a more sophisticated center that benefited from local government subventions. For the shufu, this transformation was a source of tension. LeBlanc succinctly describes their dilemma: "In order to defeat politics, the Sagvasho volunteers made a pact with the enemy" (p. 91).

The author also discusses the now well-known Network Movement and Seikatsu (livelihood) Clubs, which sell organically grown produce to members and have elected candidates to some local assemblies, largely in cities. (See Lam Peng-Er, Green Politics in Japan, 1999, an excellent book not cited by LeBlanc.) The final chapter examines in fascinating detail the successful campaign of a woman candidate affiliated with the ruling Liberal Democratic Party (LDP) in the 1992 House of Councillors elections. The author is at pains to show how, although the candidate ran on a platform of engagement with the interests of shufu, LeBlanc's own shufu respondents saw the woman largely as representative of the "elite politics" they rejected.

This book has excellent features but also some weaknesses. In many ways the most interesting parts are those rich in ethnographic detail. The world of volunteer groups and parent-teacher associations, run overwhelmingly by women in a society characterized by the "absent husband," is well described. To these women, national and even local politics are unimaginably alien, but the temptations to become embroiled in that world can be, for a few ambitious women, hard to resist. Yet, it is difficult for them to remain engaged with their core constituency, who, in LeBlanc's view, tend to reject them.

This is the first really serious account of the role of women in Japanese politics since Susan Pharr's Political Women in Japan (1981). Whereas Pharr was principally concerned with political activism, LeBlanc focuses on the rejection of politics. It is refreshing to read a book that challenges ingrained North American and European ideas about the desirability—and inevitability—of inducing marginalized groups to participate in mainstream politics. The notion that the game of politics as it is actually played is in itself alienating, and that a nonpolitical space (not alternative politics) needs to be

created, is intriguing. The enemy is not politicians, parties, or the political system but politics per se.

I have some doubts about the argument. The rejection of "elite politics" by many of the shufu interviewed by LeBlanc seems to have persuaded her that it was not necessary to give much more than a fragmentary account of what was occurring in Japanese politics at the time. Yet, her fieldwork in the early 1990s took place at a time of great political upheaval. The LDP lost its House of Councillors upper house majority in 1989 (and never regained it) and fell from power temporarily in 1993, and a series of coalition governments of varying composition have ensued up to the present. Parties have come and gone, and there is political confusion not unconnected with economic crisis. The LDP remains the central element in government, but the parameters of the system are bending alarmingly. The fact that LeBlanc treats this in a perfunctory manner suggests that she does not think it is terribly important. It is clear that many Japanese women (and men) are alienated by the way politics is conducted in their country, but what eventually emerges from the political turmoil of the past decade is assuredly important for their interests.

Although nonparticipation may be a political strategy of a kind, it is possible to find plenty of Japanese women with the determination to overcome cultural and institutional obstacles in order to help change the face of a political system desperately in need of reform. The dangers of being corrupted in the process are considerable, but for the future of all Japanese citizens, the dangers of a mass opting-out appear much greater. Tocqueville's insights may be relevant to the shufu of Japan after all.

Rethinking Party Systems in the Third Wave of Democratization: The Case of Brazil. By Scott P. Mainwaring. Stanford, CA: Stanford University Press, 1999. 390p. \$60.00 cloth, \$22.95 paper:

Barbara Geddes, University of California, Los Angeles

Scott Mainwaring combines a terrific description of the current Brazilian party system with an argument that classifying party systems in terms of their level of institutionalization deepens our understanding of democratic politics. An institutionalized party system, in Mainwaring's language, is one in which parties survive over a substantial period and remain committed to approximately the same ideological positions; voters have stable party loyalties; political leaders and voters see parties as central to the democratic process; and parties themselves are well organized, with substantial resources and routinized internal procedures not susceptible to manipulation by individual party leaders. Mainwaring argues that "weakly institutionalized party systems function differently [from] well-established systems, with significant implications for democracy" (p. 21). Much more of the book is taken up with showing, however, that party systems do actually differ with regard to levels of party longevity, voter loyalty, and party discipline than with showing the effects of these differences.

A description of the contemporary Brazilian party system and its effects on current politics and policy illustrates Mainwaring's argument and takes up much of the book. This description adds a great deal of useful and interesting concrete detail to the standard view of most Brazilian parties as fluid, personalistic, undisciplined, and decentralized. I found the discussion of candidate selection and the importance of patronage in the mobilization of support at party conventions especially thought provoking. The analysis of the

Brazilian party system displays Mainwaring's usual care, thoughtfulness, and insight. In addition, it contains memorable quotations and anecdotes about particularly egregious behavior by certain politicians, which make this section of the book lively reading. It could be assigned in either graduate or undergraduate classes.

Mainwaring's explanation for the lack of institutionalization, to use his term, in Brazil's party system is extremely eclectic, tactfully giving some credence to every argument on the subject ever proposed. To the older arguments about structural causes and the current institutionalist conventional wisdom, however, Mainwaring adds several ideas that I have not seen so well developed elsewhere. It is common to cite malapportionment as one of Brazil's problems, but Mainwaring shows in very convincing detail why and how it changes the party system. He also shows how the very decentralized candidate selection process undermines party discipline. And he notes the destabilizing effect on parties and party loyalty of manipulations of the laws governing the party system by the previous military government. These are all important contributions to our understanding of politics in Brazil.

The usefulness of lumping electoral volatility, party fluidity, and party discipline into a catch-all concept called institutionalization and used to classify whole party systems, however, remains to be established. Although the argument that disorganization, volatility, and unpredictability in the party system should have deleterious effects is inherently plausible, Mainwaring does not actually demonstrate their effects. In the one extended example of the effects of institutions on government performance, the struggle over stabilization and state reform in Brazil, the main characteristics of the political system to which Mainwaring attributes Brazilian difficulties in carrying out needed reforms are: party fragmentation, which tends to reduce support for the president in the legislature; ideological polarization, which can increase the ideological distance between the president and the median voter in the legislature; bicameralism, which increases the number of actors with potentially different preferences who have to agree to a reform; the need for supermajorities in order to revise the constitution before certain reforms can be passed; federalism, which makes it possible for governors to undermine reform implementation; and some presidents' lack of commitment to reforms. None of these characteristics are elements of the concept of institutionalization as defined by Mainwaring.

The concrete elements that contribute to party system institutionalization actually play an ambiguous role in explaining the tortuous path to reform in Brazil. Low party discipline can, as Mainwaring notes, in some circumstances make it easier for a president to put together a coalition supporting reform. Party fluidity is only important as one of several contributors to party fragmentation, which would tend to strand the president with little legislative support regardless of its causes. Electoral volatility is not even discussed in the chapter. Thus, the chapter on state reform and stabilization makes a strong and persuasive case that Brazilian political institutions have slowed the course of economic reform, but it does not show that weak institutionalization affected outcomes.

Despite these limitations, the book makes a major contribution to the understanding of Brazilian politics and to the comparative study of party systems. The field has been dominated for decades by research on a rather small number of West European countries and hence on the issues of greatest salience in well-established parliamentary systems. Mainwaring's book, in contrast, focuses on issues such as party discipline and party loyalty, which have clear impor-

tance in the currently consolidating posttransition party systems but have received relatively little attention in the Europeanist literature. The book takes a large step in the direction of building a truly comparative body of knowledge about party systems. I am sure the insights and ideas in this book will inform the work of everyone who subsequently studies party systems in new democracies.

Presidentialism and Democracy in Latin America. Edited by Scott Mainwaring and Matthew Soberg Shugart. Cambridge: Cambridge University Press, 1997. 493p. \$74.95, cloth, \$29.95 paper.

Presidential Institutions and Democratic Politics: Comparing Regional and National Contexts. Edited by Kurt von Mettenheim. Baltimore: Johns Hopkins University Press, 1997. 303p. \$45.95 cloth, \$15.95 paper.

Fabrice Edouard Lehoucq, Wesleyan University

Whereas Juan Linz ("The Perils of Presidentialism," Journal of Democracy 1 [Winter 1990]: 51-69) argues that presidential systems undermine democratic stability, Scott Mainwaring and Matthew Shugart's collection suggests "it depends." That is, whether presidents actually confront legislatures dominated by their opponents hinges upon particular ways of structuring public authority in presidential systems and the broader socioeconomic environment. More critically, the contributors to Kurt von Mettenheim's book believe that the debate setting presidentialism against parliamentarism is fundamentally miscast. In both volumes, the chapters shed light on whether electoral laws, party systems, and the internal organization of legislatures facilitate or hinder the ability of presidents to obtain legislative approval of their bills.

One set of essays deals with constitutional systems that furnish presidents with extensive powers but have electoral laws and congressional structures that promote undisciplined, multiparty systems. In a Presidentialism chapter a bit overloaded with constitutional details, Ronald Archer and Shugart contend that Colombian presidents do not get very much done, despite the fact that the constitution endows them with a vast set of powers. Because electoral laws encourage the proliferation of party factions, presidents are typically stymied by locally oriented legislators when proposing far-reaching reforms. In his chapter on Brazil, Mainwaring also argues that electoral laws and the internal organization of Congress prevents executives from confronting the country's many social and economic problems. Von Mettenheim, in contrast, asserts in Presidential Institutions that the Brazilian presidency has been a force for change; his contentions are difficult to assess, however, because he does not address the arguments made by Mainwaring and other critics of Brazilian presidentialism.

A related group of chapters analyze systems with powerful executives whose election is decided by the legislature if no candidate obtains an absolute majority of the vote. Eduardo Gamarra's two insightful chapters (he has essays in each volume under review) on what he calls "hybrid presidentialism" in Bolivia, curiously enough, uphold Linz's contention, even though Gamarra opposes replacing this system with a parliamentary one. Yet, his analysis suggests that the congressional selection of presidents means that prospective executives face powerful incentives to negotiate with rather than confront Congress, at least at the beginning of their term. Perhaps establishing a full-blown parliamentary system would simply promote higher levels of executive-legislative cooperation.

In Presidentialism, Julio Faundez takes on proponents of parliamentary solutions to Chile's political problems. Indeed, he argues that Chilean democracy survived for so long (1932-73) precisely because the presidential system moderated the centrifugal tendencies of a fractious multiparty system. He dismisses Arturo Valenzuela's claim ("Party Politics and the Crisis of Presidentialism in Chile," in Juan Linz and Arturo Valenzuela, eds., The Failure of Presidential Democracy, 1994) that a parliamentary system would have weathered the storm provoked by the socialist Allende government without, however, outlining why his counterfactual proposition is more plausible than Valenzuela's. In his chapter, Peter Siavelis argues that the demands of placating a powerful military have moderated political tensions in Chile with the fall of Pinochet. But as the transition fades from memory, an even more powerful presidency will create problems because, Siavelis suggests, the party system is still fundamentally multiparty in

Another set of essays in Presidentialism and Democracy in Latin America discuss presidents whose formal powers are less extensive. In his insightful chapter on Costa Rica, John Carey shows how relatively weak presidents get a lot done because electoral laws and legislative structures conspire to generate sympathetic congressional majorities for their policies. Similarly, Brian Crisp argues that the influence possessed by Venezuelan presidents stems from their predominant position within one of two large parties; like their Costa Rican counterparts, Venezuelan presidents do not have broad constitutional powers. Jeffrey Weldon nicely reveals that Mexican presidents were powerful not because of broad, constitutional powers but because they were the unquestioned leaders of a party that held a monopoly of state power in Mexico for more than sixty years. In all three cases, citizen dissatisfaction with established parties reduced the president's share of legislative seats and thereby created divided governments and gridlock. At least in Venezuela, governmental paralysis contributed to the electorate's recent embrace of a Bonapartist solution to the country's severe institutional and economic problems.

Several of the strongest essays in Presidential Institutions and Democratic Politics—those by Charles O. Jones, B. Guy Peters, and Bert A. Rockman-focus solely on the United States, the most prominent case of presidents who possess both few formal and partisan powers. In contrast to other chapters, these tend to emphasize the importance of style and other informal ways in which U.S. presidents overcome the obstacles generated by the separation of powers and divided government. A theme running through these essays is that presidentialism does not appear to make the United States more ungovernable than parliamentary systems in other industrial societies. This is a provocative hypothesis, but I wonder whether a more useful research strategy would be to compare the United States with other presidential systems to understand how particular ways of structuring public authority make these systems more or less ungovernable. Perhaps we should test hypotheses about whether and how institutions help make, for example, the Costa Rican and U.S. presidential systems stable by collecting evidence from relevant systems, regardless of where they are located.

Another set of articles in *Presidential Institutions and Democratic Politics* deal with semipresidential systems: those that combine independently elected presidents with parliaments that invest the prime minister with substantial executive authority. John T.S. Keeler and Martin A. Schain remind

us that there are several such models in France. De Gaulle's near dictatorial use of presidential power contrasts with Mitterand's measured use of authority during cohabitation, when he governed with a conservative prime minister who possessed a parliamentary majority. In her essay on Eastern European systems, Valerie Bunce sees many advantages to adopting variants of the French system. In divided societies undergoing a transition from state socialism, Bunce believes that powerful executives may be the only ones capable of advancing necessary reforms. In their respective chapters, Michael Bernhard and Jonathan Harris discuss the evolution of semipresidential systems in Poland and Russia, respectively. Bernhard, in particular, does a nice job of explaining the political choices that rather quickly led to the construction of the system in Poland.

In conjunction, these essays suggest several important qualifications to Linz's hypothesis. First, while the longevity of democracy is greater in parliamentary than in presidential systems, Mainwaring and Shugart are right to caution that this may be a function of the fact that a disproportionate number of parliamentary systems are located in former British colonies. That most presidential systems exist in Latin America also should alert us to the fact that something else these places have in common (autonomous military establishments?) may be responsible for the instability that has afflicted many (but not all) Latin American countries.

Second, there may be ways to improve the performance of presidential systems short of far-reaching constitutional reforms. These essays suggest that attention should focus on redesigning electoral laws to reduce party fragmentation and legislative rules that discourage majority party legislators from opposing the president. For constitutional reformers, then, the message is that democratic stability is enhanced by keeping the legislative powers of presidents restricted, especially if their partisan powers are weak. An alternative is the Argentine example. As Mark P. Jones explains in Presidentialism, gridlock is not a problem in Argentina because electoral laws generate stable congressional majorities that allow chief executives to obtain quite a lot of what they want. Furthermore, executives can rely upon a vast set of powers, especially the infamous "decrees of urgent necessity," to circumvent legislative opposition.

Third, we need to consider values other than stability when assessing the merits of alternative ways of structuring public authority. What makes the essays by Jones, Peters, and Rockman distinctive is that they are not principally evaluating constitutional systems according to whether chief executives get what they want from legislatures. It also might be useful to begin assessing whether institutional arrangements promote accountability (and how they do so), accessibility, and the other criteria we use to judge whether a democracy has "good government."

Fourth, it may very well be the case that presidential systems have certain advantages. A theme that runs through the essays by Archer and Shugart, Bunce, Rockman, and von Mettenheim is that presidents may be in a better position than other elected officials to push reform. Chosen by a majority of the national electorate, they may be among the few officeholders who can rise above locally based legislative parties to pursue the common good. Whether presidents are any more likely to propose reform—and win legislative approval of such policies—than prime ministers is a question for future research.

On the Fringe: Gays and Lesbians in Politics. By David Morton Rayside. Ithaca, NY: Cornell University Press, 1998. 384p. \$49.95 cloth, \$19.95 paper.

Donald P. Haider-Markel, University of Kansas

David Rayside adds to a growing literature on lesbian and gay politics and policy. His comparative study of lesbian and gay politics in England, Canada, and the United States focuses on several highly salient legislative issues and provides profiles of elected gay officials in each country. The cases are often dramatic, even riveting. Rayside's thesis is both empirical and normative. He seeks to understand the importance of mainstream and radical political action in efforts to secure civil rights for lesbians and gays as well as how groups can use the political system to make the lives of all citizens better. Rayside conducted 350 interviews with activists, journalists, elected and nonelected officials, and legislative aides to provide the main data component of this original project.

The book is divided into three sections, one for each country, with details on legislative actions, institutional structure, party politics, political culture, public opinion, and openly gay politicians. For the case of England, Rayside explores the dynamics of passing the antigay Section 28, which prohibited local officials from "promoting" homosexuality, and the later efforts to lower the age of consent for gay sexual relations. The Canadian case covers the successful efforts to add sexual orientation to the Canadian Human Rights Act as well as to provincial human rights laws. Interestingly, Rayside argues that the decentralized Canadian system simultaneously helped and hurt the efforts of gay activists. It seems that Canada has been the most receptive to gay civil rights at the national level, due in large part to a tolerant public.

The final section tackles politics in the United States, with particular attention to the debate over gays in the military. Rayside makes it clear that American gays and lesbians have won significant victories at all levels of government, and they enjoy more financial resources and political access than their counterparts in England and Canada, but they also face a larger, more vocal, and better funded organized opposition. Rayside demonstrates that groups on both sides of this issue have benefited from the multiple access points in the American system.

The author details the trials and tribulations of three openly gay politicians: British MP Chris Smith, Canadian MP Sven Robinson, and U.S. Representative Barney Frank. Rayside examines how each official chose to "come out" and under what circumstances, with a particular focus on legislative debates of the time. He shows how each struggled with a gay identity in public life and dealt with the sometimes conflicting demands of social movement activists, political parties, and a largely heterosexual constituency. Although these detailed accounts are illuminating, Rayside could have devoted more attention to comparing the problems faced by gay officials to those faced by minorities and women, and he relies little on the literature in this area.

Perhaps the greatest contribution of the book is the detailed analysis of gay politics in Canada and England, both of which have received less scholarly attention than the United States. Although Barry Adam (*The Rise of a Gay and Lesbian Movement*, 1995) provided a good base for understanding gay politics in these countries, Rayside's work is truly comparative in that it provides a richer analysis of political systems and institutions as well as the dynamics of gay politics in each country, and it directly compares how conditions and institutions within each society shape politics generally and gay politics specifically.

The author's contention that gays and lesbians in England, Canada, and the United States have obtained a high level of access to the political system but little real influence is confirmed by a number of other authors (e.g., Adam, The Rise of a Gay and Lesbian Movement, 1995; Robert W. Bailey, Gay Politics, Urban Politics, 1999; James W. Button, Barbara A. Rienzo, and Kenneth D. Wald, Private Lives, Public Conflicts, 1997; Donald P. Haider-Markel, "Redistributing Values in Congress: Interest Group Influence under Sub-Optimal Conditions," Political Research Quarterly 52 [March 1999]: 113-44; Ellen D. B. Riggle and Barry Tadlock, eds., Gays and Lesbians in the Democratic Process, 1999; Urvashi Vaid, Virtual Equality, 1995; and Kenneth Wald, Craig Rimmerman, and Clyde Wilcox, eds., The Politics of Gay Rights, 2000). Indeed, at times Rayside's arguments closely track those made by Vaid, but Rayside rarely references the important points made by Vaid and largely ignores previous empirical research in this area (see Timothy E. Cook, "The Empirical Study of Lesbian, Gay and Bisexual Politics: Assessing the First Wave of Research," American Political Science Review 93 [September 1999]: 679-92).

Although Rayside states that he is not concerned about the generalizability of his findings (p. xv), one has to suspect that any comparative research of this scope hopes to have some meaning outside the specific cases under study. What I found most troubling in this regard was Rayside's failure to note that perhaps there is nothing special about gay politics. He devotes so much effort to suggesting the uniqueness of gay politics that he may understate one of his most important findings: Gay politics often resembles everyday politics that existing theories can effectively explain, even across countries, institutions, and issues. Indeed, his research confirms recent journal articles that suggest the patterns of gay politics often fit those of other, less controversial political issues. Also, the gay movement has undergone many of the same strains and pressures as other social movements as it has become more institutionalized, which suggests that many social movements may be subject to the same processes. Furthermore, the factors that determine legislative and policy outcomes in gay politics, such as partisan control, public opinion, media attention and tone of coverage, interest group lobbying and resources, and the extent of a mobilized opposition, do not appear to be any different for most other policy

Rayside could have said much more about politics generally and the policy process in each of these countries. My point is not that gay politics is unworthy of scholarly study. In fact, I contend the opposite: Gay politics should be studied as a means for understanding political systems and politics generally. Gay politics intersects broad questions, such as civil rights and liberties, institutional structures, and the role of parties and interest groups, issues with which all students of politics should be concerned. Rayside simply could have been more explicit in this regard and in the nature of his contribution.

A central focus of On the Fringe is the tension between grassroots activists and mainstream interest groups within the gay social movement, and it is here that a problem surfaces with regard to understanding social movements generally. Rayside consistently refers to "a gay movement" in each of the three countries, but he never fully explains what a social movement is or is not. Instead, he focuses on formal gay and lesbian interest groups, which could be more or less than a social movement. Even though the author consistently notes the increasing need for "inside" lobbyists to develop a grassroots network that can be mobilized for legislative campaigns, he downplays this as a fundamental component of

modern interest group politics generally (see Ken Kollman, Outside Lobbying, 1998). But Rayside does note that the problems of social movement institutionalization are apparent throughout each of the cases. Activists have gained a seat at the table, but without much influence, there is wide disagreement over tactics, and movement groups themselves often reflect gender and race disparities in the larger society. Each of these conditions has surfaced in many post–World War II movements, which suggests a common social movement phenomenon.

Even with its shortcomings, the book would make an excellent secondary text for undergraduate courses in gay politics, American politics or policy, and comparative politics or policy. It is unlikely that *On the Fringe* would fit well with most graduate-level courses in those areas. Rayside has laid the groundwork for comparative research in gay politics, as well as for richly described cases that provide needed background on lesbian and gay politics.

Comparative Democratization and Peaceful Change in Single-Party-Dominant Countries. Edited by Marco Rimanelli. New York: St. Martin's, 1999. 454p. \$49.50 cloth, \$18.50 paper.

T. J. Pempel, University of Washington

Democracies enjoyed banner years during the last decade of the twentieth century. Worldwide, both military and communist dictatorships were in retreat, while numerous countries on all continents gave enhanced preference to the ballot box as the basis for selecting their political rulers. Nearly 120 of the world's roughly 190 states can now plausibly clarm to be democracies, and for the first time in history the proportion of the world's population living in such countries exceeds 50%. Of course, many of these countries may revert to authoritarianism on short notice. Furthermore, many citizens in nominally democratic regimes still lack the full political, social, and economic freedoms taken for granted in the few highly industrialized countries with deeply institutionalized democratic principles. Nevertheless, despite the obvious limitations, democracy is on the rise.

The book under review makes a sweeping effort to capture a key slice of this democratization within three types of regimes. The first is formerly Marxian regimes, such as the Soviet Union, Poland, Hungary, Czechoslovakia, and Nicaragua, in which a single ruling party has been replaced by electoral pluralism. The second is so-called Third World countries, such as Mexico, Brazil, Argentina, India, Taiwan, and South Africa, where authoritarian regimes have lost power. The third is Western nations that have experienced enhanced electoral pluralism. Broadly summarized, the book contends that all three types are commonly characterized by previous rule under the disproportionate, if not total, sway of a single political party. In most cases, the party was the state. Such rule gave way to greater electoral competitiveness and often to coalition governments.

This focus on the creation and loss of single-party rule gives the book its measure of internal coherence. The individual chapters loosely pursue a common agenda, asking why a particular country came under its specific form of dominance, why that dominance gave way, and what happened subsequently. In the introduction, Rimanelli endeavors to synthesize both the commonalities and differences within the particular cases.

Collectively, the essays reach several broadly common conclusions. First, long-term rule by most single parties is itself the result of an effort to topple previous power holders

and to establish a new basis for political control. Whether in the case of communism replacing tsarism in Russia, black self-rule under the African National Congress replacing white control in South Africa, the Congress Party of India, Mapai in Israel, or the Institutional Revolutionary Party in Mexico ending colonialism and forging a "new nation," the general pattern was the same. A populous political power base sufficiently strong to oust a formidable power was mobilized. Then, once state control was in hand, electoral dominance strengthened the new regime. Continuity of control took priority over concerns about whether new or old opposition forces were frozen out. Also, in most one-party regimes there was a "positive cycle" of dominance: State power became a basis from which to generate additional power resources that reinforced the electoral appeal of the ruling party, which in turn reinforced the party's grip on state

Equally clear is the fact that long periods of rule without a viable alternative to the party in power usually contributes to high levels of corruption. This is as true in democratic Japan or Italy, with the long-term but highly corrupt rule by the Liberal Democratic Party and the Christian Democrats, as in semiauthoritarian Mexico, with the introduction of narcofunding to ensure the continued rule of the PRI. Such cycles can be, and have been, broken. In a few cases, such as the Christian Democrats in Italy or the Socialist Workers Party of Spain, collapse came rapidly as a result of internal causes. But as was clear in Nicaragua, single-party control collapsed only after outside intervention and massive injections of foreign capital. Moreover, as the replacement of white by black single-party rule in South Africa, the toppling of communism in Russia, or the end of the Congress Party's rule in India have all made clear, a new and broader political base by no means eliminates a country's underlying problems of race, ethnicity, language, or economic transformation. New rules may address a nation's problems from a new base of power, but the problems remain the same as those confronted by their predecessors.

Such commonalities aside, the diverse countries considered, combined with most authors' efforts to capture the most recent historical developments in the regimes they examine, virtually guarantees that country specificities will outweigh analytic commonalities. For the most part, the authors focus on political parties and single-party dominance. But several chapters, such as those on Russia, Taiwan, Portugal, Nigeria, Brazil, and Argentina, devote little attention to single-party rule; instead, the concern is with the broader transition to democracy. Several other chapters give political parties more attention, such as those on Greece, Spain, and Nicaragua, but it is hard to make a good case that any single party ever gained long-term dominance. In at least one case, Japan, dominance by a single party receives only modest attention within the broader effort to examine postdominance reform processes. The greatest appeal of the book will be to readers interested in the individual countries and the light each chapter sheds on the detailed and complex factors that have shaped each regime in its own specificity.

People's Power: Cuba's Experience with Representative Government. By Peter Roman. Boulder, CO: Westview, 1999. 284p. \$70.00.

Susan Eckstein, Boston University

Against all odds, Cuba escaped the domino collapse of Soviet bloc communism, despite its heavy dependence on the superpower for trade and aid. Although Peter Roman provides little information on the dramatic effects of the USSR's dissolution on all aspects of life in Cuba, he implicitly enhances our understanding of why Cubans did not follow suit and demand systemic change. He shows how the political system works fairly effectively at the local level.

Roman traces the roots of Cuban politics to the philosophies of Rousseau, Marx, and Lenin and to the historical experiences of the 1871 Paris Commune, the 1905 and 1917 soviets, and the Russian revolution and its aftermath. He describes ways that Cuba borrowed institutional practices from the Soviet Union, such as the so-called mandate system, bases for delegate recall, reliance on nonprofessional politicians for representatives, the structure of assemblies, and the leading role assigned to the Communist Party in policymaking. Yet, he also highlights how the Cubans were, from a Marxist vantage point, democratically innovative, such as the constitutional requirement for competitive municipal elections removed from Party control and residence by municipal delegates in the districts they represent.

Roman focuses on the formal structure and functions of the political system. He draws upon available survey data as well as his own interviews and participant observations. Few other studies give as broad an overview of the system of so-called popular power at the national, provincial, and local level.

Nevertheless, Roman leaves important aspects of the contemporary Cuban polity theoretically unexplained and empirically undocumented. First, although he analyzes Cuban politics primarily within a Marxist-Leninist conceptual framework, he paradoxically reveals how nonideological the system is, especially at the grassroots level, where governance is most representative and most accountable to the citizenry. There, governance centers on everyday bread-and-butter issues, such as electricity and housing, candidates for office compete on the basis of their biographical profiles, not ideas, visions, and policy platforms; and ordinary people understand the limits to the issues they can address through the political system.

Second, Roman underplays the growing importance of nationalism to regime legitimacy. He hints at this in noting that national elections in the 1990s were, for all intents and purposes, framed as plebiscites, a "yes" for Cuba, and that the role of the party declined somewhat. But a content analysis of *Granma*, the party newspaper, would reveal how the Cuban leadership has come to emphasize patriotic heroes and events over Marxist-Leninism. Roman's theoretical framework provides no means to capture the shift or to understand its significance.

Third, Roman overlooks important signs that the political system is ineffective in channeling societal discontent. He makes no mention of the first-ever mass protest in Havana in 1994, the subsequent exodus of more than 33,000 boat people (stemmed only by the signing of a new U.S.-Cuba immigration accord), or the legal emigration of about 100,000 islanders since 1994. In Albert Hirschman's terms, the government is more tolerant of "exit" than "voice."

Fourth, Roman neither describes nor explains why Cubans increasingly are turning from politics to religion. Castro himself seems less blinded and bound by Marxism than is Roman. Or perhaps Castro understands his Marxism better: Religion is an opiate that may not be so bad at a time of extreme moral, economic, and political decay. In analyzing the political system nearly sui generis, Roman offers a level of welcome political detail, but much of contemporary Cuba that is politically consequential and that transpires outside formal political channels is undocumented, unanalyzed, and unaccounted for in the book.

Participation and Democracy, East and West: Comparisons and Interpretations. Edited by Dietrich Rueschemeyer, Marilyn Rueschemeyer, and Björn Wittrock. Armonk, NY: M. E. Sharpe, 1998. 304p. \$73.00 cloth, \$32.95 paper.

Jack Bielasiak, Indiana University

The aim of this volume is to provide a "fresh" theoretical and empirical approach to the study of participation in democratic settings by concentrating simultaneously on countries with long experience of democracy and on states newly engaged in the process. The motivation for addressing the issue in this manner, however, is founded upon contrasting developments. In the former communist world of East Europe, the primary push is to enhance participatory norms and civic behavior in order to entrench the democratic system. This comes at a time when observers of established Western democracies point to an erosion of public commitment and value support for civic activity. The contributors hope to understand these participatory impulses by bringing to bear a common theoretical prism on the "old" habits of the West and the "new" practices of the East.

The book's focus is on the role of intermediary social and political organizations as the foundation of democracy. The theoretical framework looks primarily to the self-organization of the political space between the state and the individual and to the extent and density of associational life as the answer to the puzzle of building and sustaining democracy. Such a perspective allows for an empirical study of participatory patterns in both East and West. This comparative approach is a departure from the prevailing trend in recent studies. In the field of transitology, for example, the focus is on the experience of democratization in the global South and East, where the common thread is how to attain the democratic ideal, whether in southern Europe, Latin America, or East Europe. By refocusing attention on the West, Participation and Democracy provides a benchmark that uses the knowledge of enduring democracies as relevant to the nascent pathways of participation and social organization in societies entering a democratic era.

There are a number of advantages to the East-West comparison. First, it allows a shift from the uncertainty of what is likely to happen in emerging democracies to a narrative of how associational life has developed in the mature democracies. Second, the case studies of the West reveal that there is no single answer to the dilemma of civic participation. Indeed, the West European and U.S. experiences examined in chapters by Bo Rothstein, Per Selle, Bernhard Wessels, and Robert Putnam showcase the diverse ways to create and maintain popular participation in civic organizations. The clear message to the East is that no singular transitional path is bound to assure the growth of associational life needed for democracy. Third, recent trends in the Western democracies reveal that even the most entrenched habits of self-organization and participation are subject to erosion and may lead to crises in the practice of democracy.

Even though not altogether new, these are important insights, and they are placed in greater relief by the East-West comparative undertaking. At the same time, the case studies fall prey to difficulties because of the comparison between developed and transitional democracies and the use of the same analytical tools across the East-West divide. We cannot overlook the theoretical relevance of regime transformation as a critical variable in the participatory modes shaping democracy. The problem is most clearly reflected in the very concepts used by the authors.

Contributors from the East use the idea of "civil society" as

the organizing concept for their studies. That ideal was instrumental during the collapse of communism and the subsequent transition, a concept that transcended the world of Vaclav Havel or Adam Michnik to penetrate academic discussions about the demise of the communist way of life. Andrzej Rychard, Michal Illner, Ferenc Miszlivetz, and Jody Jensen retain civil society as the analytical prism for understanding democratization in East Europe. In general, they concentrate on the shift from the mobilized society of communism, which preempted social selforganization, to the overriding task of inducing new participatory norms and organizations in the architecture of democracy. Not surprisingly, the experience of Poland, Hungary, and the Czech Republic speaks to the prevailing need to create new collective identities and find the organizational resources necessary to remodel social and political life in a democratic vein. The primary task is to entrench civil society as an intermediary space between the individual and the state.

In contrast, contributors from the West hardly mention civil society. Rather, their theoretical construct is built around "social capital," a term popularized by Robert Putnam, who is one of the authors in this volume. It is a concept that also aims at understanding participatory and associational life under democratic regimes, but its theoretical assumptions and conceptual applications differ considerably from the idea of civil society. In many respects, the different terminology used in this volume echoes the distinct vocabulary current among different branches of the political science discipline.

Crvil society is primarily the province of political theorists and area comparativists, whose main concerns are normative commitments to democracy and the re-creation of a participatory community after years of authoritarian suppression. Social capital is more the language of rational choice, which is concerned foremost with coordination and cooperation issues as they enhance or detract from equilibrium solutions, in this case voluntary participation in civic organizations and democratic endeavors. Norms and trust are important considerations, but the social capital idea concentrates on personal benefits and strategic calculations.

Different analytical concepts and the distinction between building and sustaining democracy render difficult a systematic explanation for the role and import of associational life in democracy in the Western and postcommunist worlds. This is evident in the book from the small place given to true comparative analysis and findings in relation to the individual case studies. Attempts at comparison are found in Rueschemeyer's theoretical introduction and in the editors' summary conclusion. The first is a clear exposition of societal self-organization as the foundation of democratic politics, enumerated in seven basic propositions. The last chapter is too brief a summary of the main findings, reflecting the difficulty of situating a common theoretical and comparative perspective.

The editors are to be commended for an ambitious undertaking that seeks to reconcile approaches to emerging and developed democracies, to East and West, and, consciously or not, different disciplinary approaches. Certainly, the individual chapters are solid analytical contributions that enrich our understanding of the relationship between participation and democracy, and especially its dynamic, evolving character.

Ambiguity, Coping, and Governance: Israeli Experiences in Politics, Religion, and Policymaking. By Ira Sharkansky. Westport, CT: Praeger, 1999. 216p. \$59.95.

Eva Etzioni-Halevy, Bar-Ilan University

This is a book about ambiguity as a policy tool; its message is that ambiguity is pervasive and that it works. The author not only analyzes but also unambiguously advocates ambiguity. This mechanism is portrayed as a mode of coping with problems rather than solving them. Whenever issues are contentious and the demands of various parties are irreconcilable, the search for an overall solution is futile. By contrast, "the fog of ambiguity... facilitates agreement" (p. 11).

Sharkansky follows in the tradition of Charles Lindblom (e.g., The Policy Making Process, 1968) and others who prefer "muddling through" over more systematic policymaking. Yet, Sharkansky goes even farther. For him, ambiguity is not only pragmatic and frequently inevitable but also humane and valuable (p. 23). The author applies this argument to Israel, particularly to the Israeli-Palestinian peace process. Ambivalence permeates this process, where it also proves its utility. "According to a narrow view, leaders may be lying to their constituencies. . . . By a larger view, however, the fudging of the truth may allow ... negotiations to move forward, and agreements to be made" (p. 90). Moreover, in this case "a lack of clear resolution of major problems is better than concerted efforts at a just solution" (p. 171). Indeed, the author goes so far as to claim that Israel's survival in the face of various threats owes something to its leaders' tolerance of. and skill in, ambiguity (p. 30).

Because the exposition of this thesis is not sufficient to fill an entire book, Sharkansky fleshes it out with a description of the many areas of politics, governance, religion, and so on, in which ambiguity prevails. He reviews instances of fuzziness not only in Israel but also elsewhere (e.g., among the Mormons in Utah) and even in the social sciences—such as the Coleman Report (James Coleman et al., Equality of Educational Opportunities, 1966). These descriptions, although interesting in their own right, are excessively wideranging—if not rambling—and most of them are superfluous to the book's chief point. Moreover, their roving nature makes the argument less tightly knit than it might have been.

More important, however, is the validity of the thesis. There can be little doubt that ambiguity is a widely prevalent strategy. It may even be, as Sharkansky states (p. 19), the essence of politics. But does it inevitably have the beneficial effects he claims for it? Does it always promote accommodation, or can it occasionally have the opposite result? And does it indeed prove itself with respect to the peace process?

In support of his view, the author first takes up the issue of the negotiations over the intractable problem of Jerusalem. He identifies the multiple meanings of "Jerusalem," and he shows that the city's boundaries are unclear; these two ambiguities, Sharkansky convincingly argues, may further a settlement. Since most Jews object to the division of Jerusalem whereas the Palestinians demand that it become the capital of their state, the said ambiguities may constitute the only hope for a compromise. They may enable Israel to hand over to the Palestinians certain areas that are not clearly within the city, where a capital could be established; it could be called not Jerusalem but Al Quds (the Holy City), a vagueness that both sides may be able to accept.

But does obfuscation work with respect to the overall Israeli-Palestinian peace process? According to Sharkansky, this process survives—despite overall dissatisfaction—because of ambiguity (p. 130). Indeed, for a time, this has been

true: Both sides have been equivocal about their intentions as a way to keep things going. In the long run, however, this strategy may fail. The Palestinians demand that Israel withdraw from the territories occupied in the Six-Day War, a demand that most Israelis reject. Yet, unlike the borders of Jerusalem, the pre-1967 borders of Israel (the "Green Line") are only too clear.

If an accord is reached that includes redrawing Israel's boundaries and withdrawal into them, then the chickens may come home to roost: A state's borders cannot be nebulous, and a retreat into them cannot be hazy. Part of the present disagreement concerns Israeli settlements in the territories claimed by the Palestinians, and there can be no ambivalence with respect to those. They cannot simultaneously be dismantled and kept in existence. Sooner or later double-talk must give way to clarity, and problems that have been kept submerged must surface. To my mind, the book's major drawback is that it does not sufficiently clarify this point.

In general terms, Sharkansky admits that ambiguity has a dark side, yet he understates its implications: "The dark side is not so much its disadvantages as the realization that many... practitioners of the craft seem unwilling to recognize its advantages" (p. ix). But is that the whole story? The author should have stressed that in some cases ambiguity may turn from an advantage into a disadvantage. What may have favorable results in the early stages of negotiations, or even in the drafting of a preliminary set of intentions, may have the opposite effect when a final contract needs to be signed and implemented. In Sharkansky's view, part of the dark side of ambiguity is that policymakers who embrace it at an earlier stage often shy away from it later. Could it be that this is not by mere chance but because of the constraints of reality?

The shortcomings of ambiguity may not surface in every case, but they pose a sufficiently widespread problem to raise questions about the overall validity of Sharkansky's position. His argument is challenging and well worth presenting, and it certainly should be noted, but couching it in more modest and balanced terms would have made it more convincing.

Mass Politics and Culture in Democratizing Korea. By Doh C. Shin. Cambridge: Cambridge University Press, 1999. 335p. \$59.95 cloth, \$22.95 paper.

Michael A. Launius, Central Washington University

Doh C. Shin has made a most welcome and long overdue contribution to the literature on both contemporary Korean politics and comparative democratization by focusing on the recent experiences of the Republic of Korea (hereafter Korea) as it institutionalizes its democratic transformation. The author asserts that democratization progresses through phases, and from the late 1980s through the 1990s, Korea moved through two of these: the disintegration of bureaucratic-authoritarian rule and the emergence of a pluralist and democratic order. Specifically, Shin's study emphasizes that the ongoing democratic transformation of mass culture and politics is the defining characteristic of the third or consolidation phase of the process.

The major contribution of this book is to orient scholarly focus toward the increasing importance of public opinion in analyzing and evaluating political change in contemporary Korea. Because public opinion matters rather little in strong-state authoritarian regimes, until the recent demise of the authoritarian regime in Korea, it was of marginal interest or

import to students of Korean politics. In the past, scholarship focused on elite behavior, regime type, political economy dynamics, and linkages with the global system. Recent events, such as the unprecedented publication of a political black list by a broad range of Korean civic groups before the 2000 National Assembly elections, clearly validate Shin's viewpoint and demonstrate that changes in mass culture as evidenced by public opinion sampling warrant our increased attention. This work stands alone as a comprehensive resource on this emerging and vital area of Korean studies and comparative democratization.

In terms of approach, the author seeks to define and distinguish the Korean model of democratization in terms of what the people themselves experienced during the late 1980s and early 1990s. The approach is exceedingly ambitious-perhaps too much so-for the author attempts to integrate and balance such complex factors as societal culture, regime structure, institutional and procedural substance, and cross-cultural/cross-national experiences in constructing an explanatory model of democratic change that works at the individual, institutional, and systemic levels of analysis. In his own words, "all the significant changes taking place at the macro as well as micro levels are considered in this study to offer a comprehensive and balanced account of democratization" (p. xxv). By trying to integrate and explain all significant micro and macro level changes of an entire society, the author may be attempting to explain/infer too much from the data at hand. Nonetheless, Shin tackles the challenge admirably and makes a strong and compelling argument for his point of view.

The study is grounded in the conviction that the most effective way to assess the dynamics of democratic opinion and behavior is to use survey research instruments that directly ask people not only what they think but also how they choose to act in the political process. The empirical basis of the study consists of six parallel surveys of the Korean mass public conducted over nine years (1988, 1991, 1993, 1994, 1996, 1997) by the Institute of Social Sciences at Seoul National University and the Korea-Gallup organization. The specific questions are included in an appendix. By jointly analyzing the six surveys the author minimizes the limited value of any one-time snapshot of public opinion and traces the trajectories, direction, and magnitude of institutional and cultural democratization over time. In addition, Shin assembled comparable data on countries in Asia and Europe that are considered functionally equivalent with the Korean case for the purposes of cross-national and cross-cultural compar-

Mass Politics and Culture in Democratizing Korea consists of five parts and nine chapters, but readers would be well advised to devote close attention to the meaty and essential preface, wherein Shin outlines the broad scope and method of his project. Part 1 discusses the political system and charts popular opinion trajectories concerning the transformation from authoritarianism, vestiges of regionalism, the developing practice of democratic politics, and the empirical evaluation of how changes in the quality of everyday life affect democratic transformation.

Part 2 focuses on the attitudinal and behavioral changes of average citizens since the late 1980s and may be the most interesting and compelling section of the book. Shin analyzes the degree to which democratic ideas and behaviors have become rooted in the citizenry, which is the true measurement of consolidation. There is clear evidence of the development of a civic consciousness as well as strong commitment to the general idea of democracy, but there is considerable disagreement over what kind of democracy the country

should pursue. The author identifies as many as six politically distinct groups (from fully committed supporters to opponents of reform) of nearly equal strength who support divergent democratic agendas, none of which is numerically powerful enough to lead the rest (pp. 76–9). This is a disturbing portent of continuing factionalism and dissensus for the future.

Part 3 deals with representative institutions, such as the National Assembly and political parties. Shin's findings are rather modest and more or less confirm the conventional wisdom: Although popular opinion of legislative performance was "at best lukewarm," democratic commitment did precede and shape support for the legislature (p. 163). In the chapter on political parties, the author notes the failure of the system to generate truly democratic and representative party politics. Shin essentially sees this as a consequence of the moralist Confucian culture that saturates Korean society and emphasizes personal qualities of candidates over their organizational affiliation. Yet, this seems to neglect the consistent structural failure of highly personalist elitist parties to root themselves in constituencies. Low popular support may, indeed, be a rational citizen response to obviously dysfunctional and unaccountable electoral and party systems.

Part 4 deals with non-Western roads to democratization and develops the notion that a viable Korean democracy need not necessarily conform to Western ideals. This is a recurring and controversial proposition in the literature and will be viewed with skepticism by many readers. It is clearly an area that needs further investigation and theory development. Chapter 7 deals with democratic consolidation during the Kim Young Sam era (1992-97), and it is a tribute to the volatility of Korean politics that his regime already seems like ancient history. It is noted that Kim Young Sam was quite successful in dismantling the power base of authoritarian government and delegitimating authoritarian rule, but he was not effective in consolidating democratic institutions and legitimating democratic values. This infuriated those who favored authoritarian policies but at the same time disenchanted those who supported increased democratization. It is unfortunate that the survey data end with the Kim Young Sam administration, for public reactions to the twists and turns of the Kim Dae Jung government might provide further understanding of the posited consolidation phase of Korea's democratization.

In part 5 the problems and prospects for Korean democracy are discussed. According to the data, authoritarianism persists in the beliefs, values, and attitudes of voters and officials socialized by previous regimes. As a consequence, most Koreans are only diffusely attached to the ideals of democracy as the preferred political system and basis for civic community. The author draws the very sobering conclusion that it may take several generations to remold authoritarian cultural codes, foster democratic behaviors, and liberalize Korea's Confucian values. According to Shin's findings, the path to a genuine democratic mass culture may be very long indeed.

This is an ambitious and provocative book that should be read by anyone with an interest in contemporary Korean politics and comparative democratization. Shin successfully demonstrates that rich material can be gleaned from public opinion surveys and that movement toward the consolidation of a democratic mass culture is well under way in contemporary Korea. That movement may be proceeding in fits and starts and may have systemic and cultural dimensions that are unique to the Korean context, but nevertheless it is steadily leading society away from its authoritarian past. This book

goes a long way toward clarifying and explaining the dynamics of that transformation.

The President and Congress in Postauthoritarian Chile: Institutional Constraints to Democratic Consolidation. By Peter M. Siavelis. University Park: Pennsylvania State University Press, 2000. 245p. \$55.00 cloth, \$18.95 paper.

Katherine Hite, Vassar College

A full decade has passed since Chilean dictator General Augusto Pinochet handed the presidential sash to a democratically elected civilian. The country has held two successive presidential elections, and despite both anticipated and unanticipated tensions in military-civilian relations, there is no mortal threat to the stability of democratic political institutions. In fact, the new millennium began with nothing less than a president from the Socialist Party, the very party the military ousted and brutalized little more than a quarter of a century ago. Chile has been held as a model country of transition from authoritarian rule, in which political leaders have fundamentally behaved consensually, the economy has essentially remained healthy, and civil society has been fairly quiet regarding divisive political and social issues.

Nevertheless, as Peter Siavelis persuasively argues in his study of executive-legislative institutional relations, such peace and stability are not rooted in the institutional framework designed by the military dictatorship and upheld in the redemocratized regime. Rather, the democratic process of lawmaking and limited reform during the 1990s was the product of an exceptional transition in which members of government and the opposition saw it in their interests to be exceedingly cooperative. In what Siavelis terms a system of "exaggerated presidentialism," the executive generally forwards proposals within the accepted boundaries of the congressional opposition's political, social, and institutional interests, and the congress willingly performs as a rubber stamp. The center-left governmental alliance has been cautious—overly cautious according to many—to assuage those who supported the authoritarian regime's neoliberal, antipolitics agenda. In addition, governance in the 1990s took place in the context of a society traumatized by the instability of the immediate preauthoritarian years and the repression of a seventeen-year dictatorship.

Siavelis examines a key question for Chileanist scholars: As democracia de acuerdos, or democracy by agreement among elites across the political spectrum, wears thin, will the governing institutions provide the appropriate framework for conflict resolution, thereby avoiding the stalemates and backlashes of the past? He suggests not. Moreover, he argues that without fundamental and quite viable reforms to strengthen the congress, the authoritarian framers' "protected democracy" design will certainly prove inadequate for further democratization because it stymies full political representation as well as the incentives to tackle collaboratively the continuing constraints to a more equitable and tolerant society.

In the tradition of Arturo Valenzuela, Siavelis offers a very readable account of the history of preauthoritarian Chilean congressional performance as an effective mediator of both national projects and parochial demands. He traces the unusual strength of the Chilean political system at that time, in which several parties achieve congressional representation and no one party could effectively govern alone. Accommodationist practices in the legislature, with no small dose of

clientelism or pork, contributed to the endurance of democratic rule in a region historically short on democracy. Presidentialist reforms in the late 1960s began to alter such practices, which Siavelis argues ultimately helped set the stage for the 1973 authoritarian response.

Once in command, the dictatorship sought to eliminate future politicians' "excesses." The 1980 constitution expressed true disdain for democratic political representation through its extreme presidentialist design; a bizarre, binomial majoritarian electoral system; and a set of powerful non-elected governing bodies. Although the democratic opposition negotiated and approved some reforms before assuming office in 1990, the core of the authoritarian design remained intact.

Siavelis details the workings of executive-legislature relations through the first transitional government of Christian Democratic leader Patricio Aylwin (1990–94). He gives Aylwin high marks for his leadership skills, a man who garnered support for his proposals through broad consultation and who explicitly recognized the legitimacy of the congress as representative. Nevertheless, Siavelis suggests that consultants outside congress, from select members of the nonelected party leadership to the major economic players, had a larger influence in lawmaking than the parliament itself, and under the Frei administration (1994–2000) the president made little attempt to disguise this fact.

A main concern for Siavelis is that the Chilean institutional balance of power severely undercuts a democratic opposition that may represent a significant percentage of society. There is little incentive for this opposition to cooperate or, for that matter, for the parties allied behind the president to remain so closely allied from their positions in congress. Siavelis demonstrates that, contrary to their expectations, the military constitutional framers did not design a system that decreased the number of competing parties. In addition, the power balance does little institutionally to encourage the executive to work closely with the congress, much less grant room for legislative initiative.

The question Siavelis inherently raises in the immediate term is a counterfactual one: What if, in the most recent presidential election, right-wing candidate Joaquín Lavín had won? Although the center-left alliance, led by Socialist candidate Ricardo Lagos, won its third presidential election last year, the race required a run-off, and Lavín garnered 48.7% of the vote. Moreover, Lavín has a powerful political operation, but his image is that of a one-man "doer," distanced from traditional political party machinations. The center-left's governing behavior over the past decade suggests that President Lagos will be quite cognizant of a significant opposition that must be respected. A Lavín victory may very well have affirmed the worst concerns Siavelis raises.

Siavelis could have provided more about contemporary internal party organization as well as the public's identification with and attitudes toward political parties. To support his concern about the legislature's weakness, Siavelis alludes to the general lack of interest and negativity Chileans express for congressional representatives. Yet, such negativity is not limited to the congress. According to national surveys, Chileans harbor even greater animosity for political parties in general, viewing them as self-interested employment agencies. Many perceive that the flip side of political elites' extremely consensual behavior over the last decade has been an insular, elitist politics, a lack of interest in civic engagement.

Moreover, congressional discourse and action in the 1990s was intensely conservative in the cultural sphere, particularly

regarding the family (there is no divorce law), gender (the term itself was officially banned from congress, a world first), sexuality, and reproductive rights. Together with such issues as considerable social inequality and the human rights atrocities of the past, Chilean politicians face a series of conflicts that are increasingly bound to test state-society relations. Siavelis underscores institutionalists' need to weave the dynamics of institutions and social and political context together, but in this he is less concrete.

This book is a very useful contribution both to understanding the political institutional dynamics of contemporary Chile and to relating such dynamics to the broader literature on democratic institutional design. Complementing quantitative data with a range of interviews with political elites, Siavelis provides a textured analysis that is often missing from institutionalist works. He extends institutionalist approaches beyond the presidentialist-parliamentary system debates to examine what kind of presidentialism in relation to what kind of congress would make for a more representative democratic politics.

Chinese Urban Life under Reform: The Changing Social Contract. By Wenfang Tang and William Parish. Cambridge: Cambridge University Press, 2000. 388p. \$54.95 cloth, \$25.95 paper.

Marc Blecher, Oberlin College

Mobilizing survey data from the late 1980s and early 1990s, Wenfang Tang and William Parish have crafted a textured, subtle, careful, balanced, and comprehensive picture of urban Chinese political sociology. Their work is located squarely in several comparative contexts, ranging from advanced industrial societies, to other former state socialist systems, to East Asian newly industrialized countries (NICs) (focusing on Taiwan). They treat the systemic changes in China since 1949, and the comparison with Taiwan, as offering possibilities for a grand social scientific experimental study, in which culture and history can be held relatively constant so that the effects of fundamental structural shifts from preindustrial, precapitalist society to radical state socialism and then to a more market-based system can be scrutinized. The main analytical foci are two: "how different social contracts shape people's lives" and "the winners and losers" (p. 4). Substantively, they address the distribution of education, employment and income, popular opinion about reform, labor-management relations, bureaucracy, political participation, and, through the collaboration of Parish with Sarah Busse and James Farrer, respectively, gender in relation to work and family.

The book is an invaluable contribution of the greatest importance and interest to students not just of China but of broadly gauged social change everywhere. It will also go far in informing future scholarship, for there is much here to be tested in interviews and in studies focused on particular localities in China as well as work on other countries. Chinese Urban Life under Reform develops a number of important overarching arguments. Although industrialization-modernization theory has some purchase on explaining the outcomes and reactions to reform in China, overall the authors find, somewhat to their surprise, that structural theories emphasizing the fundamental differences between capitalism and state socialism are far more powerful in accounting for the differences between Maoist- and Dengist-era China as well as between the latter and other market-based and marketizing countries. They find that class matters deeply in accounting for the effects of reform and the ways people understand and react to those effects. The authors are impressed by how malleable society proved to be under both state socialism and market-based structural reform.

On this last point, the collapse of radical state socialism and the stunning rapidity and success with which China has embraced structural reform have, understandably, induced many analysts to develop a profound appreciation for the power of the market to reshape and reorganize social and economic (if, in China's case, not quite yet political) life. In some quarters, the Maoist period is now regarded as little more than a fascinating, wrenching interlude that may have accomplished a good deal of ground-clearing and took the first painful steps toward industrialization and national independence, but that left behind scant legacy of its radical vision. The Right may offer a triumphalist view of Maoism's putative inconsequentiality, and the Left may be moan the reversals of China's noble experiment, but many in both camps agree with the wag who wrote that "the Party is over."

Tang and Parish disagree. Decades into reform, they find, for example, that Chinese women are still far more equal with men in employment and the home than are women in any other country. Looking at the Dengist-period labor market, they argue that the underlying structural changes accomplished in the Maoist period—such as extraordinary gains in educational attainment, nearly universal employment outside the home, and the precipitous drop in fertility—continue to undercut the effects of patriarchal recidivism and the pronounced employment discrimination they document for the 1980s and 1990s. These changes even extend to the home, where men do a greater share of housework, and where women have more power in family economic decisions than in other societies.

Turning to more general distributive issues, Tang and Parish find that Dengist China still has a highly egalitarian distribution of income. Compared with their counterparts in Taiwan and the former state socialist countries, not to mention the advanced industrial nations, Chinese factory managers, technocrats, bureaucrats, and even many private entrepreneurs have an income and standard of living that is significantly closer to that of manual workers. Although income returns to education are rising, they continue to be offset by significant returns to seniority. At the same time, the highly egalitarian distribution of education and health care across classes and between town and countryside, which was a hallmark of the Maoist political economy, is being eroded. In particular, the working class is losing out both from the new market-based mechanisms as well as the increasingly meritocratic criteria for school and university admissions.

Tang and Parish argue that in the Maoist period workers appreciated the close interpersonal ties with other workers fostered by the way that work was organized on the factory floor and by the comprehensive community life provided by most industrial enterprises, including shared housing, health care, and, often, schools and dining and entertainment facilities. They also believe that, ironically, radical state socialism in China produced a double dystopia: the alienation from work that Marx attributed to capitalism combined with the personalized dependency on superiors characteristic of precapitalist social formations. On these points they rely not on new data—given the political constraints, Chinese surveys generally stay away from the Maoist period—so much as previous studies, which, as my own more recent interviews with older workers suggest, may overstate the gravity and particularity of these problems.

For the Dengist period, Tang and Parish do have important new findings. They show that workers are, portentously, China's most dissatisfied urban class. The foci of discontent are declining income, welfare, and political status (as state policies and ideological priorities shift against them, and as they lose representation in the workplace and the party) as well as increasingly draconian workplace discipline. Their data, the latest of which come from the early 1990s, are a bit too early to capture workers' profound concern about employment insecurity that would soon develop and is predominant today.

Workers are not at all passive in the face of all this, although their political participation, at least in the late 1980s and early 1990s, still took the form of individual, particularistic contacting of workplace officials far more than use of approved institutional channels or collective action, such as slowdowns, not to mention protests and strikes. Here, too, there have probably been marked changes since the early 1990s. The authors anticipate some, such as the increased use of arbitration committees, but give short shrift to others, such as the rapid rise of strikes and protests in recent years. Yet, in their very interesting finding that worker reactions to China's structural reforms are conditioned more by their views of the national situation as a whole than by individual situations, Tang and Parish may offer an intriguing clue to why working class anger and protest are not even greater and more threatening. When combined with their finding that workers are the most dissatisfied class in urban China, Tang and Parish offer a cautionary tale about the potential for a significant social movement that could, especially in the event of an economic crisis, pierce the fragile, tense calm that prevails as of this writing.

The overall picture that emerges from Chinese Urban Life under Reform is of change that has been as incremental and controlled as it has been profound. The authors remind us that in the West, hegemony based on personal dependency relations was eradicated only by protracted labor-based resistance. The Chinese state, they also point out, is trying, with no small degree of success thus far, to short-circuit such resistance through a variety of cooptive, corporatist, diversionary, and repressive measures. When all is said and done, they portray China as in the midst of a Polanyi-esque "great transformation" that evinces, in a phrase hallowed in both China's twentieth-century revolutionary and reformist movements, decidedly "Chinese characteristics."

Ambiguities of Domination: Politics, Rhetoric, and Symbols in Contemporary Syria. By Lisa Wedeen. Chicago: University of Chicago Press, 1999. 230p. \$48.00.

Fred H. Lawson, Mills College

First-time visitors to Damascus invariably notice two things: the groups of armed men clustered at key points throughout the city and the massive portraits of President Hafiz al-Asad that loom over them. Scholarship on the politics of contemporary Syria focuses almost exclusively on the former. Lisa Wedeen urges that greater attention be paid to the latter, not because clusters of armed men are politically inconsequential, but because explanations for Syrians' obedience to the present authorities that are couched in terms of outright coercion "remain incomplete. They fail to account for the ways in which language and symbols mediate, structure, define, and continually reassert political power and obedience" (p. 156).

Among the various symbols that have served to buttress the predominance of the military wing of the Ba'th Party over the last three decades, none has proven more potent and durable than the depiction of President al-Asad "as omnipresent and omniscient" (p. 1). This image stifles dissent in at

least two ways. By "cluttering public space," it makes it impossible for "alternative symbols, discussions, and language" to be articulated (pp. 33–49). By underscoring the president's presumptive role as head of Syria's "national family," it is a trope that promotes "understandings of obedience and community in terms of a chain of filial piety and paternal authority that culminates, and stops, in Asad" (pp. 49–65).

Nevertheless, and despite the government's best efforts to maintain the status quo, Syrian citizens have learned to undermine the legitimacy of Ba'thi rule by constructing novel reformulations of permissible slogans and imagery. One prominent artist who was commissioned to design a poster to commemorate the October 1973 war, for example, opted to superimpose the president's face over a scene from the twelfth-century Battle of Hittin, in which Salah al-Din drove the Crusaders out of Jerusalem; "the artist claims that his point, however, was to draw people's attention to the contrast between Salah al-Din's triumph and Asad's 'victory'" (p. 3).

Popular stage plays and television serials regularly lampoon the corruption that pervades state agencies, the leadership's evident failure to deliver on its grandiose promises, and even the instruments of physical torture that the security services routinely employ against suspected enemies of the regime (pp. 92–107, 112–20). Moreover, as in virtually all Arab countries, a wide assortment of jokes poke fun at almost every aspect of politics, including the president (pp. 120–9). Public compliance in Ba'thi Syria therefore turns out to be inherently "complicated" (p. 143). "On the one hand, the [regime's use of the al-Asad] cult is risky, as it invites the very subversion it seeks to manage. On the other hand, the methods of resistance open to most Syrians, although frequently courageous and ingenious, are by themselves incapable of significantly altering 'the order of things'" (p. 159).

Wedeen reaches these conclusions on the basis of what she calls "a 'political ethnography' of power in contemporary Syria. By insisting on the importance of ethnographic detail," she asserts, "I hope to explore the advantages, costs, and political significance of public rituals, while at the same time supplementing those concerns with a symbolic interpretation of the actual content of Asad's cult" (p. 25). But the book is a peculiar sort of ethnography indeed. It offers no systematic, detailed account of the daily lives of the population under observation. Instead, it presents a handful of anecdotes (pp. 65-6, 67, 161 n. 6), one of which is subjected to exhaustive commentary (chap. 3). It recognizes no distinctions between rich and poor, Muslims and Christians, 'Alawis and Kurds. Rather, it generalizes about Syrians as if they constituted a uniform mass: "Syrian families," for instance, "not unusually, tend to be hierarchically organized and stratified on the basis of sex and age: the young are subordinate to the old, women are subordinate to men" (p. 51). In fact, the study pays no attention at all to "people's experience of their political lives" (p. 160). On the contrary, it scrutinizes a selection of texts produced by the regime's supporters—including a newspaper editorial (pp. 42-4), a short story for children (p. 53), a panegyric to the president (pp. 54-5), and a poem honoring the memory of his mother (pp. 58-60) without analyzing the effect of such writings on different audiences inside the country.

Ambiguities of Domination illustrates both the attraction and pitfalls of the new cultural studies for political scientists. Investigating public spectacles, stage plays, cartoons, and jokes can undoubtedly tell us something useful about Syrian politics. But anecdotal, superficial "ethnographies" almost certainly distort more than they clarify. And it would make little sense to embark upon an inquiry into the rhetorical and

symbolic complications associated with compliance and transgression in Ba'thi Syria had an earlier generation of scholars not exposed the complex dynamics whereby powerful actors and institutions exercise physical coercion in the domestic arena.

The Political Economy of Dictatorship. By Ronald Wintrobe. Cambridge: Cambridge University Press, 1998. 390p. \$65.00 cloth, \$24.95 paper.

Susanne Lohmann, University of California, Los Angeles

Political science typically covers the modern state, which emerged around 1776, and mass democracy (regimes with mass suffrage), which emerged around 1870. Most states today are democracies of some form or another, and most were founded after World War II. Thus, much of what political scientists know and teach about government is based on our study of democracies and regimes that have been around for a comparatively short time. And, of course, much is based rather parochially on our study of post-World War II American institutions and American political behavior.

Recorded history covers 5,200 years (3200 BC to 2000 AD). Political science should be about all, or at least the most important, states and governments in history. It should cover various types of states and government, not just the modern state and mass democracy. It should draw on states and governments that have lasted centuries or even thousands of years, not just a few decades. There is such a thing as innovation in government, and political science should show how government has improved over several thousand years, but it also should deal with, and explain, discontinuities, large-scale breakdowns, and dark ages.

S. E. Finer's three-volume comparative History of Government from the Earliest Times (1997) does all this, and more, and does it brilliantly. Finer selects regimes according to the following criteria: "first, the historically great and mighty polities, next the archetypal polities, thirdly innovators great or small; and, finally, the vivid variant" (vol. I, p. 1). He analyzes these regimes according to a standard format and assesses their general character, strengths, and weaknesses using a standardized set of criteria. He develops a typology of regimes (territory: city-state, generic state, empire; decisionmaking personnel: élite, masses; and decision-implementing personnel: bureaucracy, armed forces). He identifies recurrent themes (e.g., religion, or more generally belief-systems, and the legitimation of power; constraints or limitations on government). He identifies elements in these various forms of government that can be described as "inventions" (e.g., simple and supermajority rule; representative assemblies). He deals with stable and unstable regimes, breakdowns, and

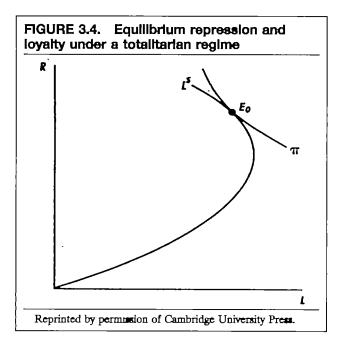
It is against this background that I read Ronald Wintrobe's Political Economy of Dictatorship and was disappointed. In all fairness to the author, there is probably no single article or book published in political science in recent decades that would not come across as a lesser contribution in comparison with Finer's work. Let me first explain why, by the standards I would have applied if I had not read Finer's book, I would have said that Wintrobe makes an important contribution to the study of an important type of regime (dictatorship) that has been understudied by public choice and rational choice theorists. Then I will suggest that Wintrobe's analysis of dictatorship is impoverished in a peculiar way, but my discussion should be understood as an indictment of public choice and rational choice scholarship rather than as a critique of Wintrobe.

Wintrobe is an economist, but his contribution should be judged on its substance and method, not the Ph.D. label of the author. In terms of substance, the topic (dictatorship) is political science. In terms of method, the public choice approach Wintrobe employs originates in economics but is now widely used in political science.

Wintrobe asks big, and good, questions about dictatorship. Under what conditions is it stable and longlasting, and when does it collapse? Does dictatorship promote economic growth? What economic and political constraints do dictators face, even those whose powers are nominally unlimited? What factors influence the degree of repression, and what "tools," other than repression, do dictators employ to stay in power? Under what conditions are dictators popular, or are forced to care about their mass appeal, what do they do to improve their popularity, and why are they often popular? How do dictators inspire loyalty among their subjects and in particular their supporting bureaucracy? What can we say about the complicity of mid-level bureaucrats in a regime that does evil? How do dictatorships interact with other governments in the international arena, both on the security dimension (war) and the economic dimension (international trade)? How does economics (free markets) interact with politics (democracy)? How does nationalism enter into the

Wintrobe covers a number of recent dictatorships, some of them in detail, others in passing: Nazi Germany, the communist government in the former Soviet Union, the apartheid system in South Africa, the Pinochet regime in Chile, the ancient Roman empire, Mobutu's Zaire, and Haiti under Papa Doc Duvalier.

The public choice analysis in this book is valuable for several reasons. First, precisely because dictatorships often are "accounted for" in terms of the irrational and mysterious behavior of their leaders and followers, it is quite interesting to see a public choice account that tries to make sense of it all in terms of rational, goal-oriented, and constrained behavior. Second, precisely because Wintrobe offers an account that parallels the rational choice account of democracy, his analysis allows us to see parallels between dictatorship and democracy. For example, like democratically elected politicians, dictators have to respect economic and political con-



straints. They do not fear getting voted out of office, but they do fear getting "rioted" or "military couped" out of office. Just as democratically elected politicians do certain things to improve their chances of re-election, dictators do certain things to reduce the likelihood of riots and military coups. Third, precisely because Wintrobe is an economist, the background literature he uses differs (with some overlap, obviously) from the literature a mainstream political scientist would draw upon, and for this reason he taps into ideas and ways of thinking about dictatorship that are relatively unusual and original.

In terms of method, Wintrobe's approach is the standard microeconomics of an earlier generation of public choice theorists (as represented by Hal Varian's microeconomics textbook), rather than the game theory employed by today's generation (as represented by David Kreps's microeconomics textbook). Wintrobe's theory does not reflect the methodological progress that has been achieved as theorists have moved away from public choice theory and embraced rational choice theory: It is impoverished in a peculiar way.

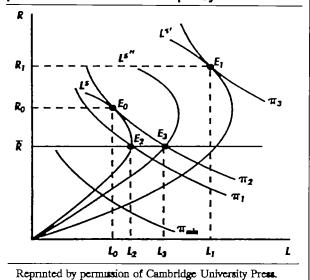
To see this, consider his Figure 3.4 (Equilibrium repression and loyalty under a totalitarian regime), which depicts the political constraints on the dictator's power. The x-axis reads \hat{L} for loyalty, the y-axis R for repression. The dictator maximizes power, which depends on loyalty and repression. The isopower line in Figure 3.4, labelled π , captures the combinations of repression and loyalty that generate the same level of power; that is, the dictator is indifferent between combinations of loyalty and repression on a given isopower line. The backward-sloping line in the figure, the loyalty/repression curve, captures the "supply of loyalty" constraint under which the dictator is laboring. It shows what happens as dictators increase the degree of repression. Initially, their subjects respond with more loyalty but eventually with less. The optimal degree of repression is achieved at the point at which the loyalty/repression curve touches the highest attainable isopowerline; this combination of loyalty and repression maximizes the dictator's power subject to the supply of loyalty constraint.

The uninitiated may consider this silly but political scientists who have suffered through Microeconomics 101 will know better. Wintrobe "derives" his story with reference to the substitution and income effect in economics. According to standard consumer theory, people's utility decreases with the number of hours worked (or, equivalently, increases with the number of leisure hours), and it increases with the goods and services they can buy with their income from working. As wage rates increase, people respond by increasing their labor supply. This is the substitution effect: They substitute labor for leisure because work has become, quite literally, more "worthwhile." As their income increases, however, they eventually decrease their labor supply. This is the income effect: They substitute leisure for labor because the marginal utility of the additional things they can buy with additional income declines and eventually drops below the marginal utility derived from additional leisure time.

There is something "only an economist" about the way Wintrobe relabels the standard microeconomics axes (leisure and labor become loyalty and repression), bases his assumptions on empirical references to repression under the Nazi regime and the Stalinist purges of the late 1930s, and calls the whole thing a theory of dictatorship. But we need to look at what the theory predicts.

Suppose the dictator's polity is hit by an exogeneous shock, such as an increase in the rate of economic growth, as depicted in Wintrobe's Figure 3.5 (How equilibrium levels of repression and loyalty under a totalitarian regime are af-

FIGURE 3.5. How equilibrium levels of repression and loyalty under a totalitarian regime are affected by changes in economic performance and Western policy



fected by changes in economic performance and Western policy). Under certain conditions (assumptions about the mathematical properties of the underlying functions), the dictator can move onto a higher isopower curve. At the new point of tangency, E1, the dictator represses more, gets more loyalty in exchange, and becomes more powerful. It is hard to know what to make of this prediction, since the story does not provide an account of the mechanism by which things happen (and the analogy to the income and substitution effect in consumer theory is not helpful).

The deeper problem is that Wintrobe makes use of the standard microeconomics verbiage, which invokes individual maximization, but does not derive the response of the dictator's subjects to repression from individual maximization. Instead, he works with a "reduced-form" loyalty/repression function. This kind of analysis was typical of an earlier variety of rational choice theorizing (called public choice theory), but today it seems wanting.

Furthermore, rational choice theory itself, of whatever variety, is an impoverished framework for understanding dictatorship. To see this, consider one of the most fundamental questions we can ask about dictatorships that rely on an extensive bureaucracy and some degree of mass loyalty to do evil (the Nazi regime obviously comes to mind). What is the criminal responsibility of the mid-level bureaucrat who, in worauseilendem Gehorsam, implemented the orders the dictator never put down in writing? Why did so many ordinary people go along, even though they were often not coerced, and in many cases people who refused to go along met with no punishment?

According to Wintrobe, the Nazis used the principle of competition to organize terror and murder: A competitive entrepreneurial bureaucrat embedded in a competitive bureaucracy ends up going along with evil. I believe there is something to this hypothesis (competition has a way of suspending people's moral sense, as in "everyone else is doing it"), but rational choice theory does not explain this phenomenon. (Within rational choice theory these questions are meaningless: what is the moral sense? what does it mean for it to be suspended?) Rational choice theory can say

something about the incentives ordinary people face, and at the margin it can say a little bit about why ordinary people on some occasions do evil and on other occasions recoil from evil or even do heroic things to topple evil. In other words, the benefits and costs of doing one thing or the other change over time or differ across situations. But it cannot explain why different people respond differently to the same incentives, or why ordinary people do evil out of indifference or inattention, or why they act heroically when the probability that their enormously costly action will make a difference is close to zero. People are driven not only by incentives; to understand why ordinary people do evil, and so on, there is no alternative to studying the psychology and biology of people.

As in Finer's comparative history of government, we need to expand our horizons by looking at nondemocracies from the earliest times. Wintrobe's rational choice account of dictatorship comes across as a "thin simplification" when it is read in the context of 5,200 years of government. Again, the problem lies not so much with Wintrobe; rather, much of rational choice theory could be improved quite a bit by an infusion of history.

Between the Flag and the Banner: Women in Israeli Politics. By Yael Yishai. Albany: State University of New York Press, 1997. 292p. \$54.50 cloth, \$17.95 paper.

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This is an excellent textbook for the reader who is not familiar with the subject of women and politics in Israel. It presents, clearly and concisely, a coherent picture of the situation of women in Israel from a liberal feminist perspective. The book is unquestionably a vital source for data about women in Israel. It offers a broad survey of the current situation but also sets it in the perspective of the changes that have occurred since the state's establishment and does not neglect the prestate situation. My remarks cannot describe the wealth of findings and the nuances with which they are analyzed. Interested readers are invited simply to peruse the book, and I am certain they will be amply rewarded. My focus is on the theoretical aspect of Yishai's study and its contribution to furthering our understanding of the situation of women in Israel.

The democratic-liberal paradigm, although not declared, underlies the author's theoretical presuppositions and dictates the logic by which the material is organized, presented, and analyzed. The first section provides a comprehensive picture of the place of women in political institutions and associations, based on the thesis that the interests of organized groups are represented in political institutions and organizations. The guiding question is whether women are represented at all levels of Israeli politics and whether the political organizations represent women's interests. The picture that emerges is that the level of women's representation in the political elite is low, and that women who enter politics are not committed to furthering the interests of women.

The second theoretical element in the democratic-liberal paradigm is the electorate. The assumption here is that the interests of the voters, their collective identity, and the scope of their participation will, ultimately, determine who is elected and the interests they will represent. In other words, the size of the gender gap affects the degree of women's representation in the elite. Therefore, the second part of the book examines the scale of women's participation in politics as compared to men and reaches the conclusion that women

are less involved in politics than men. This disparity is more pronounced in Israel's Arab population than among the Jewish population. As for identity, the findings show, in a rough generalization, that women define themselves primarily on the basis of national rather than gender affiliation. At the same time, the author identifies a deepening of the gender gap in various spheres, a development she calls "feminism without feminists." The concept of equality is trickling down, but gender-based organizing and the rise of a feminist identity show few signs of intensification.

Policy and legislation are the output of the democratic thrust and constitute the third element in the paradigm Yishai has adopted. The subjects examined are legislation in the spheres of labor, the family (particularly laws relating to marriage and divorce, which in Israel are geared to the demands of religious bodies), violence against women, and abortion. The interesting finding is that while not responding to feminism, the authorities have been increasingly aware of the problems exercising feminists. This finding remains unexplained, mainly because the author deals with formal political order and its manifestations.

The theoretical framework goes a long way toward dictating the explanations offered. According to Yishai, along the continuum on which democracies extend in the Western world, ranging from "service state" to "visionary state," Israeli democracy is situated on the side of the visionary state. In her conception, the Western democracies are more compatible with the model of the service state, which is guided by the need to respond to the public mood and assuage grassroots pressures in order to stay in power. Israel fits more closely the model of the visionary state, which emphasizes general goals and is based on mobilization and socialization. "In a visionary democracy," the author explains, "there is constant tension between the collective imperatives of society and particularistic needs of subgroups within it, including women" (p. 7).

From this point of departure, the title of the book encapsulates its entire theory and thesis. Women in Israel are trapped between the flag of the missions that the state has set itself, namely, security and the "ingathering of the exiles," and the banner, which represents the goal of equality for women. In the terms of political theories, then, women in Israel face the alternative of integration within the existing frameworks or mobilization in the cause of their distinctive interests. The book's general conclusion is that between the national flag and the feminist banner women have opted for the former, and it is here that the source emerges for the gender inequality that prevails in Israel.

Faithful to her theoretical approach, Yishai presents findings that support her explanations. Her documentation of the dominant rhetoric that excludes women from politics in Israel is so successful that she herself falls captive to it. Many feminist theoreticians have noted that the clash between two ostensibly opposing conceptions—the interests of the society and the interests of women—is a product of social construction of reality and ideological manipulation. Why are the needs of women, who constitute half the population, consid-

ered particularistic needs of a subgroup? In accepting the political system as a given, the author acknowledges the division between the private and the public as self-evident and ignores the claim that it is socially constructed and historically constituted. As many scholars have indicated, the private-public dichotomy serves as a good excuse to exclude women from politics, as does the perceived conflict between society's needs and women's needs. Both are sociopolitical frames that enable this exclusion to take place.

Yishai's description and her explanations are fine for limning the picture of the gender inequality that exists in Israel, but what is missing is a critical analysis. If this book had been written at the end of the 1970s or even during the 1980s, I would spare no praise for the achievement. In light of the ramified literature that has been added to the bookshelf of political science and political sociology, however, one can only wonder about the author's disregard of the theoretical criticism that views acceptance of the existing order as self-evident as the main barrier to social change. Such studies exist both in the wider academic community and in Israel. Moreover, Yishai quotes such works but ignores their basic theoretical thesis. For example, she refers to the eminent theoreticians Susan Okin (Women in Western Political Thought, 1978; Justice, Gender and Family, 1991) and Carole Pateman (The Disorder of Women: Democracy, Feminism and Political Theory, 1989) in order to describe the dichotomy between the public and the private, but she ignores their critique of the gendered structure of that dichotomy. In the same vein she ignores the critical aspects of works on women in Israel (e.g., Hanna Herzog, Realistic Women: Women in Local Politics, 1994, and the Hebrew version of Herzog's Gendering Politics, 1999; and Deborah Bernstein, The Struggle for Equality: Urban Women Workers in Prestate Israeli Society, 1987, and Pioneers and Homemakers: Jewish Women in Prestate Israeli Society, 1992).

It is apparently no accident that the author frequently uses the term "sex" in place of the appropriate word, "gender." The book takes no notice of the shift that has occurred from sex to gender in scholarly and social thinking. It bears recalling and emphasizing that the term "gender" signifies that the distinction between the sexes is first and foremost a social construct, which becomes institutionalized and creates a gendered regime of thought and of social order. The potency of this social construct, which is founded on a hierarchical dichotomy of power relations between men and women, derives from the fact that it is presented as "natural" and "congenital," and hence as self-evident. The assertion that the gendered distinction is a product of the social construction of reality voids the ideological argument that the existing order is universal or necessary. The questions that arise are how the gendered world is structured and what are the social and political mechanisms that reproduce it. Yet, the way women in Israel think about themselves and their society and the way Yishai tries to cope with her findings are still captive to the old conceptions. The result is that the book, like its subjects, remains trapped between the flag and the banner.

International Relations

The German Problem Transformed: Institutions, Politics and Foreign Policy, 1945–1995. By Thomas Banchoff. Ann Arbor: University of Michigan Press, 1999, 217p. \$44.50.

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Applying an historical-institutionalist framework, Banchoff posits that the Four Power regime, North Atlantic Treaty Organization (NATO), the European Union (EU), and the Commission on Security and Cooperation in Europe/Organization for Security and Co-operation in Europe (CSCE/ OSCE) developed a capacity to shape West German foreign policy outcomes well beyond the "circumstances of their creation" (p. 2). Chapters address "The German Problem Transformed," "The Cold War and Integration," "Detente and the New Ostpolitik," "The New Cold War and the Intermediate Range Nuclear Forces Struggle," "Post-Reunification Foreign Policy," and "History and German Foreign Policy." The book ends with a mere six-page appendix on "The GDR and the German Problem," although a major constraint the FRG faced between 1949 and 1989 was the realization that any action undertaken vis-à-vis Moscow or Washington held consequences for its eastern "Siamese

The central construct of the book, foreign policy path dependency, merits extensive theoretical exploration, but the author supplies only one footnote (three sources, p. 2, p. 11; the term then disappears until pp. 165-6). There is little new empirical evidence that when Germany really was "free" to choose its own path, it consciously violated its national interests by not going it alone, due to external constraints. A continuous stress on the great men of history, for example, the "different perceptions of international realities and the lessons from the past" and "priorities espoused by rivals for the chancellorship" (p. 16), seriously obscures the focus on institutionalism. One wonders what "institutional constraint" led to the fact that Hans-Dietrich Genscher was "not even informed" (p. 159) in advance of Kohl's November 1989 Ten-Point Plan for Germany, although he had served as foreign minister for more than ten years, across Social Democratic Party of Germany/Free Democratic Party (SPD/ FDP) and Christian Democratic Union/Free Democratic Party (CDU/FDP) coalitions. Was the SDP deliberately excluded from unification negotiations merely to provide the Christian Democratic Union/Christian Social Union Party (CSU) with an electoral advantage in 1990 (p. 157) when something as important as the Nation was at stake?

Historical memory is reduced here to what select politicians say it is at any given point, and foreign policy "meaning" emerges out of contestations among party officials and government leaders. Collective-qua-national memory is actually a messy constellation of selective, subjective, and even counterfactual reconstructions of experience, such as Germans "remembering" themselves as victims and survivors rather than as war perpetrators or collaborators. Adenauer and Schumacher experienced two world wars; Brandt and Schmidt participated only in World War II, as a resistance fighter and Wehrmacht soldier, respectively. Kohl's experience was limited to attending Nazi-dominated schools; Scharping, Lafontaine, and Schroeder have no experience with real war, or real nationalism, at home. Yet, the effect of generational change is hardly mentioned.

The text takes extraordinary liberties with established terms of German political discourse. The author refers to the (confrontational) Ostpolitik of the Adenauer era, the New

Ostpolitik of the Brandt era, and the second New Ostpolitik of the post-1990 period. None of the classical studies I checked supports this conceptual stretching, which suggests policy continuity from Adenauer to Brandt. Adenauer's Deutschlandpolitik and exceptional, high-level appeals to Moscow were deliberately grounded in the politics of nonrecognition, no experiments, and negotiation through strength. Ostpolitik (not "the New Ostpolitik") was Brandt's explicit attempt to bring about change through recognitionqua-rapprochement (one has to accept the status quo in order to change it, a view initially opposed by the United States). Four years after Schmidt's resignation, the SPD initiated the second stage of Ostpolitik, grounded in its controversial Common Paper with the Socialist Unity Party (SED) of Germany (see Ann Phillips, "The West German Social Democrats' Second Phase of Ostpolitik in Historical Perspective," in Peter Merkl, ed., The Federal Republic of Germany at 40, 1989, pp. 408-41). SPD chief Scharping called for a new Ostpolitik after unification, not for "a second New Ostpolitik" (p. 159).

The treatment of sovereignty is equally problematic. On page one Banchoff declares: "The Germany that emerged after the [1989] collapse of the German Democratic Republic (GDR) is neither fully sovereign nor faced with a fluid balance of power abroad." But he asserts, without qualification, that "the Western allies granted the FRG its sovereignty as a member of NATO and the West European Union" (p. 26) following Adenauer's 1954 pledge to create a German army that renounced the production of ABC weapons. A country still militarily occupied until 1955 is not sovereign. Banchoff writes that the final withdrawal of armed forces by the "wartime anti-Hitler coalition" occurred after ratification of the 1990 Two-Plus-Four Accord, when "Germany finally secured sovereignty over its own affairs. It did not, however, exercise full sovereignty as a matter of practice" (p. 140). Here and elsewhere, the author conflates Germany's de jure status before/after 1990 with questions of strategic choice. He stresses Kohl's "readiness to include German Pershing 1A's in the December 1987 treaty" (p. 128), but this was symbolic politics, not a real choice. The Pershing 1A's were not the Germans' to concede, given the ban on ABC weapons. Grassroots peace groups knew that NATO's 72 1A's were obsolete, headed for replacement. Indeed, legal debates over the INF deployments (1983-84) produced a curious consensus at far ends of the political spectrum that Germany could not have blocked the Pershing II deployments no matter how the Bundestag voted, owing to the Reserved Rights of Allies.

The chapter on INF leaps from Franco-German ties, to Washington-Moscow relations, to the Historians' Controversy (which came after the deployments), to the biographies of Schmidt and Kohl. Yet, it virtually ignores new pressures imposed on external actors/institutions by German citizens themselves; the 1981 demonstrations impelled Reagan to shift to the double zero option, for example. FRG decision makers faced a sudden break in the 30-year taboo on public debate over NATO/military security dictates, triggered by the rise of counterexperts, including former General Kurt Bastian. Banchoff credits the INF outcome with having "spawned movement toward a greater foreign policy consensus at the level of German domestic politics" (p. 128), but surveys assessing the public's openness to arms control, Ostpolitik, and FRG-GDR relations show that a positive consensus had become firmly entrenched ten years before the INF treaty-Christian Democratic Union (CDU) hardliners were the ones who still had to be brought into the fold by 1987. The "polarization" constantly sensed by Banchoff stems from his heavy reliance on conservative politicians and historians as sources; of ten interviewees, seven are Christian Democratic Union/Christian Social Union (CDU/CSU) members, two are Social Democrats, and one is an Free Democratic Party official. Fixated on national soul-searching among conservatives, the author misses the real news emerging from the INF struggle: Despite the peace movement's "failure" to stop the Pershing II/Cruise deployments, Kohl and the German defense establishment had no choice thereafter but to lobby heavily for the 1987 INF treaty, which went farther in eliminating whole classes of intermediate nuclear weapons than PM-participants dreamed possible in the early 1980s. The SPD, for the record, did not officially terminate its support for double-track deployments until its Extraordinary Congress of November 17–18, 1983, three days before the Bundestag vote.

Subsequent treaties, one reads, render a "smaller Bundeswehr a more independent force than it had been in previous decades" (p. 141). In what respects? The Constitutional Court did sanction the use of German forces in "out of area conflicts" in 1993, but only within the framework of collective security systems and with the direct consent of the Bundestag. The court's institutional role in legitimizing yet limiting the application of the 1971 Basis of Relations Treaty, the 1991 Maastricht Treaty, and Gulf War involvement is barely mentioned; yet, in no other NATO or EU state do we find judicial institutions so powerful in dictating the parameters of foreign policy to a nation's commander in chief—and its allies.

The author stresses "continued restrictions on German sovereignty" (p. 140) resulting from the EU's common foreign and security policy after 1990, as if Germany's voluntary surrender of sovereignty by way of Common Foreign and Security Policy (CFSP) were comparable to forty years of restrictions imposed by way of conditionless surrender in 1945. Germany's response to conflict in the Balkans is said to have transpired within the context of the CFSP, when in fact its preemptive recognition of Croatia in December 1991 dragged the rest of the EU into reluctant acceptance of the Yugoslav secessions (the Maastricht Treaty/CFSP did not take effect until 1992).

We are told from page 2 onward that this is an "enduring transformation of the German Problem," only to confront the concluding question: "Will the postwar transformation of the German Problem persist in the future" (p. 175)? Something "enduring" must, by definition, "persist." This text posits a paradox where none existed by insisting that "no concerted effort to define and pursue specifically national interests took place" (p. 165). National interest in postwar Germany was never a question of sumply restoring "national unity" after 1949. Citizens recognized early on that their quintessential national interests—peace and prosperity—were best pursued within an international, mutilateral, and supranational context, and that a strategy of going it alone (Sonderweg) inevitably violates the national interest of Germans as a whole.

Condemned to Repetition?: The Rise, Fall, and Reprise of Soviet-Russian Military Interventionism, 1973–1996. By Andrew Bennett. Cambridge, MA: MIT Press, 1999. 387p. \$35.00 cloth, \$17.50 paper.

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In this rich, dense book Andrew Bennett develops learning theory (LT) and uses it to explain Soviet and Russian military interventions from 1973 to 1996. In advancing LT, Bennett joins other scholars who emphasize ideational rather than material factors in foreign policy (e.g., Jack Levy, "Learning and Foreign Policy: Sweeping a Conceptual Minefield," International Organization 48 [Spring 1994]: 279–312). Indeed, the ideational concept "military interventionalism" (defined as the propensity of an actor to use force in varying contexts) is crucial to Bennett's argument that the USSR/Russia learned from intervention (and nonintervention) experiences (e.g., from successes and failures) in the form of raised or lowered interventionalism, which in turn shaped subsequent interventions and noninterventions (p. 14).

The major contribution of Condemned to Repetition? appears to be an outline of a comprehensive LT. Learning is defined as "change in cognitive structures as the result of experience or study"-on the individual, organizational, governmental, and international levels (p. 81). Bennett summarizes key dynamics of learning on each level of analysis and employs the first three levels to explain intervention. In showing how individual belief systems change, Bennett dwells on attribution biases, mechanisms of causal inference, processes of change in interrelated beliefs, and sources of variations in individual learning. Organizations, in contrast, learn when inferences from history are encoded into "routines that guide behavior" (pp. 98-9). Moreover, Bennett points out how organizational learning models differ significantly from bureaucratic process models. Finally, governmental learning involves change in the "content or robustness of the beliefs embodied in a state's behavior and institutions" (p. 105).

Generally, LT maintains that the lessons of history are debated among leaders seeking to build a governing coalition, and the winners in this debate will be those whose views appear to be validated by recent experiences (p. 357). Moreover, Bennett develops a model of learning to determine whether governmental learning can take place in reaction to a particular experience. The model yields sixteen possible paths or combinations depending on four variables: Do top leaders learn a particular lesson, do implementing organizations learn that same lesson, is the state strong on the issue in question, and do societal or opposition leaders draw the lesson (pp. 108–12). Bennett also adapts the model to the Soviet and Russian governments.

Bennett concedes that his LT yields outcome predictions of Soviet and Russian interventions "in only a very general way" (p. 124). For example, divergence between officials' expectations and their experiences should have led to learning, and this learning should have contributed to associated changes in behavior. At the same time, LT appears to be more specific in predicting processes through which learning should have taken place, a finding Bennett considers "not inconsequential" because this provides evidence on the causal mechanisms at work (p. 124). Among process predictions concerning Soviet and Russian interventions are expectations that Soviet peripheral or tactical beliefs (e.g., on the use of force) should have changed before core beliefs (on whether US relations were zero sum); once core beliefs changed, there should have been sweeping changes in many related beliefs. Another is that one should expect to find considerably more private learning by policy experts before Gorbachev's rise than was publicly acknowledged. Most important, according to Bennett, differences in lessons individuals learned should be traceable to their differing information sources and biases as well as to their material interests.

Bennett also makes a methodological contribution to the study of international relations by refining the method of "structured focused comparison of cases," pioneered by Alexander George, and by proposing a rigorous method for

testing a learning hypothesis (p. 12). This includes carefully defining the variables; selecting which cases to study among the opportunities for Soviet and Russian intervention, which to develop briefly for comparison, and which to leave fallow, asking the same questions concerning ideas about military intervention across the cases; using both written sources and interviews with Soviet and Russian leaders; comparing the explanatory power of LT with that of alternative theories; and employing process-tracing within cases to confirm the validity of the learning explanation (p. 31). Moreover, for ease of exposition Bennett defines three main schools of thought about intervention in the Soviet period (p. 30, Table 1.5), and three major schools of thought on the use of force in the Russian period (p. 306, Table 8.1).

A third contribution is Bennett's detailed study of several cases of Soviet/Russian intervention: Angola in 1975, Afghanistan (entering in 1979, continuing during 1980-84, and withdrawing in 1989), and Chechnya during 1994-96. The cases form the foundation for Bennett's key contentions that the learning explanation of intervention (1) outperforms the alternatives on key points where they disagree and (2) helps resolve these alternatives' anomalies on points of comple-

mentarity (pp. 5, 351-67).

The case studies are on the whole admirable, but the study of Russia's decision to intervene in Chechnya in December 1994 suffers from some weaknesses. First, much less information is available than on the other cases, which must render many of Bennett's conclusions tentative. Second, the author mistakenly states that the hawkish colonel General Andrei Nikolayev was promoted to Minister of Security in July 1993 (p. 325), when in fact Nikolayev was named Chief of Border Troops, which he continued to head after December 1993, when it was reorganized as the Federal Border Service (Amy Knight, Spies without Cloaks: The KGB's Successors, 1996, pp. 67, 82). Finally, Chechnya was arguably not a foreign country, as is required by Bennett's definition of intervention: "the use of military force in an intrastate or interstate conflict of a foreign country with the objective of influencing the makeup, policies, stability, or strength of the regime in that country" (p. 14). Moreover, two of the book's briefly developed cases involved intervention against quasistates or nonstates within the intervener's borders: the Soviet limited intervention in the Baltic republics in early 1991 and the Russian small-scale intervention in North Ossetia in fall 1992. Consequently, a reformulation of this book's dependent variable may be in order.

These reservations notwithstanding, Bennett makes a major theoretical and a minor methodological contribution to the study of international relations, particularly by developing a LT and showing how it both complements and contradicts major theories of international relations in explaining Soviet and Russian military interventions. Henceforth it will be difficult, if not impossible, to explain interventions without taking into account both learning and interventionalism.

Community under Anarchy: Transnational Identity and the Evolution of Cooperation. By Bruce Cronin. New York: Columbia University Press, 1999. 176p. \$49.50 cloth, \$18.50 paper.

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Springing now from constructivist impulses is a strong interest in the nature, scope, and influence of identity in international politics. Various studies, such as Bill McSweeney's Security, Identity, and Interests (1999), and the book under review attempt to bring the concept of identity to bear on

problems of security, descending from Alexander Wendt's speculation on the development of international identities in a hypothetical state of nature ("Anarchy Is What States Make of It: The Social Construction of Power Politics,' International Organization 46 [Spring 1992]: 391-425).

Cronin's theme is the way political elites construct transnational communities to enhance security in international politics through cooperation, a departure on their part from supposedly standard behavior under anarchy. The construction of community amid anarchy is old territory, investigated in integration theory years ago and in even older forms of liberalist analysis. Here it is reexplored via a constructivist approach that extrapolates to states some perspectives on individuals and groups offered in social identity theory (SIT) in psychology and symbolic interaction sociology (SIS). The argument is straightforward and very clearly set forth: Like individuals, states develop multiple identities, and some are more salient than others at any one time. The identities grow out of interactions with "others," which is how identity is formed. When states share an important characteristic, have some distinctive or exclusive feature in their relationship, have a strong and very positive interdependence, and gain substantial experience in acting together, they are likely to develop a transnational identity. Within such an identity they find norms, other boundaries to behavior, and goals; in such a context states develop recognizable roles, and cooperation is facilitated, often through institutionalized arrangements. Cronin explicitly challenges Jonathan Mercer's contention (Reputation and International Politics, 1996) that SIT explains why states will normally be suspicious of each other and will find it very difficult to cooperate.

Cronin suggests that the type of transnational identity essentially determines (identifies is perhaps a better word) what transnational community emerges. Two security arrangements display minimal community: a state of nature and a balance of power. There are five true security communities: a great power concert, either amalgamated or pluralistic security communities, a common security organization, and a collective security system. Each rests on a different transnational identity shared by the members. To illustrate how this approach should be used, Cronin provides three nineteenthcentury case studies: the formation and operation of the Concert of Europe (and the Holy Alliance), plus the coalescence of many actors into what became Germany and Italy.

All this is laid out simply and clearly. The writing is accessible and confident. Cronin makes no claim that other perspectives are irrelevant or that international politics has but one pattern. "Constructivist theories do not attempt to refute material and rationalist-based explanations as much as they seek to expand them" (p. 131). The book is a fine resource for anyone seeking to illustrate how constructivist analysis works or how to use analysis at one level to derive insights at other levels.

Brevity has its drawbacks, of course. Many elements deserve more extended treatment or a more extended defense. An example is the central notion of transnational identity. Such a complex phenomenon must defeat resistance from domestic identities, plus the effect of sovereign autonomy and anarchy, so we might expect it to form only gradually. Yet, Cronin cites examples of its rapid emergence, which is hard to explain. One possibility is that, because interaction is so important for stimulating a transnational identity, operating a great power concert (for instance) has as much to do with generating a congruent identity as the reverse. This might help explain why security communities can emerge so rapidly at times; they do not always have to be preceded by a transnational identity, just the seeds for one. If so, then we

would need to search for what sets off the interactive process. Creative, courageous statesmen? A fortunate conjunction of events? Shifts in the sociocultural context?

Similarly, more work is needed on variation in transnational identities. For a concert, the author cites a corresponding identity based on great powers' mutual recognition, but this does not readily distinguish a competitive from a cooperative great power set. A common security association is said to rest on an identity in shared regime type or ideology, but this might well apply to effective long-term great power concerts and perhaps to most pluralistic security communities. Cronin cites a sense of "regional identity" as the basis of a pluralistic security community, but this has been important to European integration, has been cited as one basis for the Concert of Europe, and is almost surely a contributor to a regional collective security system. This problematizes the hypothesized links between specific security associations and transnational identities.

Cronin suggests transnational identities are likely to emerge during social and political upheavals, as creative adaptations in times that allow for new thinking and new social arrangements. This is a very interesting contention, because the standard concern now is that such times drive people to turn to primordial ties either domestically or on an international plane in ways that often diminish prospects for a security community. Thus, we need to consider when upheavals produce which sort of identity and why.

Is a security community the product primarily of a suitable transnational identity? Or, as integration theory or some theories on alliances and multilateralism suggest, does it emerge alongside, reinforced by, other transnational communities (political/ideological, economic, cultural) or even descend from them? At least two broad lines of thought offer alternatives to the Cronin analysis. The standard liberalist view, now reinvigorated (Etel Solingen, Regional Orders at Century's Dawn, 1998; Jeffry Frieden and Ronald Rogowski, "The Impact of the International Economy on National Policies," in Robert Keohane and Helen Milner, eds., International and Domestic Politics, 1996; Helen Milner, Interests, Institutions, and Information: Domestic Politics and International Relations, 1997; and others), holds that high levels of interaction, particularly economic, build overlapping national interests reinforced by the rising political leverage of domestic groups that are the main beneficiaries. Alternatively, in the democratic peace thesis, transnational community results from a shared type of political system regardless of the level of interaction, which seems relevant, since so many successful security communities have arisen among democratic nations.

In short, this book can help open up students' minds and our own. It is too brief a treatment of a very large and very compelling subject these days to offer all the answers, but it provokes interesting questions.

Paths to State Repression: Human Rights Violations and Contentious Politics. Edited by Christian Davenport. Lanham, MD: Rowman & Littlefield, 2000. 248p. \$69.00 cloth, \$24.95 paper.

Conway W. Henderson, University of South Carolina at Spartanburg

This book joins a legacy of quality edited works that have focused on the study of political repression. Each volume, at its time of publication, has indicated how far this study has advanced in approaches and methods. Christian Davenport recruited eight original and useful essays mostly by political scientists, some of whom are among the leading researchers

in repression studies. These essays range from case studies of one or two countries to aggregate projects at the regional and global level. The primary question that guides the contributors is why governments repress their own peoples. A secondary question concerns how a reciprocal relationship, between governors and governed, affects repression.

Together, Davenport's collaborators help bolster recognition of political repression as a discrete phenomenon that deserves status as a subfield of both human rights and conflict studies. Specifically, these authors help clarify that repression is the violation of the integrity of the person. When opposition rises, many governments respond with tools ranging from intimidation to political detention, torture, and murder. Some of the writers also reinforce the conclusion reached by others that structural conditions, especially democracy and economic level, as well as social forces, such as war and rapid development change, are now standard control variables that discourage or encourage repression.

Several special innovations deserve highlighting. James Scarritt and Susan McMillan find evidence in African cases that long-term democratic traits are better for a future stable democracy than a quick transition from an authoritarian government to full democracy. Kathleen A. Mahoney-Norris, in an exemplary comparative study of Cuba and Nicaragua, discovers not only that ideology influences repression, but also that different versions of the same ideology, socialism in this case, can have differential outcomes for repression. In an interesting twist, the focus of George Aditjondro et al. is not on targeted opponents of government but on the part of the government that the opposition selects for protest. Finally, a more realistic portrayal of repression is available, according to John C. King, if we can statistically account for the interaction of variables as they perform in a model rather than rely on the usual additive model. He promises more explanatory power with the same variables.

A recent edited book can indicate the level of accomplishment in the study of political repression and at the same time reflect what is not accomplished. Except for particularized gains, the editor and contributors do not move us much beyond the loose repression research framework already existing. The articles are as eclectic in nature, regarding approaches and methods, as is the broader study of repression. Given the book's title, the editor may have welcomed eclecticism and then received it. A useful addition by Davenport would have been a final chapter setting out a research agenda. Researchers need not grope about blindly, however. Short of a theoretical breakthrough that would permit a simple model of a few variables, the best option might be to follow the template of Steven C. Poe et al., who report on the most comprehensive repression models to date regarding the number of variables and cases. Their contribution in the Davenport book is a refinement of their earlier work. In the present effort, they demonstrate that human rights abuses tend to persist for a time beyond the life of the threats perceived by the government.

A small caveat is in order, however, if we follow the worthy example of Poe and his colleagues. Sometimes articles, heavily dependent on quantified data and statistical analysis, appear to benefit only a small academic community. It is important to find ways to condense some of the more technical aspects of research and focus on effective description if we are to reach beyond political scientists. From a normative perspective, specialists in political repression should want research results to serve as useful intelligence for the policymaking community, made up of international lawyers, staffs of foreign ministries and international government

organizations, and human rights activists. It is these people, after all, who do the everyday work to stop repression.

One volume cannot be expected to cover all the questions about repression, but several other topics easily could have been added. Although the predator-prey model of Chris Lee et al. is intriguing, a more thorough investigation of instinctual theory and repression could be useful. A model based on instinctive behavior would be a direct challenge to the assumption of a rational decision maker that operates behind most models of repression. Perhaps repression is so difficult to explain and predict because leaders act out of fear based on a drive for survival rather than make careful calculations we identify with war and chess. Or perhaps culture can explain much of the variance in repression that today's models are missing. Precolonial African chiefs sometimes applied a maintenance dosage of repression even in quiet times. Torture has had a historical role in Iran regardless of the type of government in power. Inducing respect for authority by means of fear may be a continuous cultural motive.

Another question concerns whether repression travels well across borders, as do civil disobedience tactics, corporate restructuring, and military weaponry. For instance, when one Latin American country turns to military rule and repression, does it influence other countries to do likewise? Also, did repression reach a plateau with twentieth-century totalitarianism and is now in historical decline? Finally, a contribution from someone associated with Ted R. Gurr's minorities-atrisk project would have been highly useful. With ethnic politics so contentious, one ethnic group in power and others in the opposition is a confrontational dyad ripe for repression, if not genocide.

Although this edited work does not offer bold advances in either theory or methods, it is a treasure-trove of useful articles right with references. Most repression research of the future will undoubtedly cite *Paths to State Repression*. This book will further enhance Davenport's reputation as a leading scholar in the field of political repression.

In the Shadow of the Garrison State: America's Anti-Statism and Its Cold War Grand Strategy. By Aaron L. Friedberg. Princeton, NJ: Princeton University Press, 2000. 362p. \$69.50 cloth, \$21.95 paper.

Benjamin O. Fordham, University at Albany, SUNY

Most international relations scholars would probably agree that a relatively greater ability to mobilize and direct societal resources makes a state more likely to prevail over its international rivals. From this perspective, the outcome of the Cold War is a paradox. Compared to their American rivals, Soviet leaders were better able to extract national resources and direct them to military purposes, a task they pursued assiduously throughout the Cold War. In spite of-or perhaps because of-their efforts, the United States emerged victorious. Aaron Friedberg argues that the American state ultimately drew strength from its inability to mobilize and direct societal resources to the extent that some argued was warranted by the Soviet threat. He attributes this curious source of power to the structure of American institutions, the power of entrenched antiinterventionist interests, and above all to a long-standing ideological tradition of antistatism.

Friedberg begins with Alan Milward's concept of a "strategic synthesis" that balances military needs against the

economic and political costs of mobilization (War, Economy, and Society, 1939-1945, 1977). He examines how the American state extracted (and failed to extract) money and manpower and directed (or failed to direct) industrial development, arms manufacture, and technological development in order to confront the Soviet threat. Friedberg devotes a chapter to each of these areas of direction and extraction. In each case, he finds that executive branch policymakers were unable to enhance the power of the state to the extent that many of them believed necessary. The divided structure of the American government and the interests of powerful groups unwilling to bear the costs of further mobilization contributed to the opposition, but Friedberg assigns the greatest weight to an antistatist ideological tradition. Critics of greater state power were able to use the symbols of this tradition not only to build broad support for their position but also to raise doubts in the minds of state leaders themselves about the wisdom of greater extractive and directive authority. In the end, many of the U.S. arrangements turned out to hold substantial advantages over the more statist alternatives the Soviets pursued.

Friedberg does not question the seriousness of the Soviet threat, but his account raises some crucial questions about treating U.S. Cold War strategy as a straightforward response to it. He shows that decisions about resource extraction for military purposes directly affected many other policy matters, and he makes it clear that international threats were only one influence on these choices. Although the response the Soviet threat really required was generally ambiguous and contested, the implications of national security policy choices for matters such as taxes and budget deficits were usually very clear. Moreover, domestic concerns like these often predominated even when there was a broad consensus within the executive branch that the international threats demanded more direction or extraction by the state. Dwight Eisenhower's acceptance of a strategy that assumed a war with the Soviet Union would be short—a premise he did not really believe—is a particularly striking and important example (pp. 130-3, 222-5). Similarly, continuing fiscal constraints and other spending priorities helped prevent great increases in the military budget when the Kennedy administration shifted to a strategy of flexible response (pp. 139-48).

Friedberg shows that, in addition to the inevitable intrusion of other policy matters into strategic decisions, strategy was often an outcome of domestic political bargaining rather than strategic calculation by state decision makers. Key elements of the strategic synthesis were compromises that did not really satisfy any of the major parties to the debates that produced them. For example, selective service emerged from a political struggle between those who wanted universal military training and groups opposed to all forms of conscription (pp. 149-98). Neither side wanted selective service, but it persisted for more than twenty years. Similarly, Friedberg shows that the United States developed competing institutions for scientific research, an arrangement that turned out to have unexpected advantages, more by accident than by design (pp. 296-339). In general, Friedberg persuasively argues that American strategic decisions had as much to do with their domestic implications as with their international purposes and are best described as "resultants" rather than 'choices." In doing so, he calls into question accounts of foreign policy that treat it as the result of rational calculation by the state and strategic interaction with international adversaries.

Despite its strengths, the book is not without limitations. Its stress on the role of antistatist ideology in limiting the development of the national security state explains the first

half of the Cold War better than the second. The fact that avowed antistatists like Ronald Reagan later supported the expansion of state power for military purposes is puzzling from this perspective and requires further explanation. Also, Friedberg's praise for the American strategic synthesis in the final chapter relies on the assertion that the only likely alternative was more statist, not less. This contention rests uneasily with the book's evidence that those who wanted to give the state even fewer resources and prerogatives were politically strong. (Indeed, without a number of well-timed international events, these antistatists might have prevailed.) In view of American success in the Cold War, it is hard to argue that the state required additional resources and authority in order to compete with the Soviets. The book does not demonstrate that the same outcome could not have been produced with less extraction and direction by the state. These limitations do not obscure Friedberg's contribution. This is a very good book, one that is sure to be read with interest by anyone interested in the domestic politics of foreign policy, the linkage between war and state-building, and American foreign policy.

Doing Good and Doing Well: An Examination of Humanitarian Intervention. By Stephen A. Garrett. Westport. CT: Praeger, 1999. 213p. \$55.00.

Herbert K. Tillema, University of Missouri, Columbia

Humanitarian intervention is a controversial old idea that attracts new interest in the post-Cold War era. The notion justifies some foreign military operations that purport to arrest harm to human life. The conception is fundamentally controversial because it claims exception to the general assumption that modern international law and diplomatic practice proscribe coercive interference among sovereign states. Specific applications of the label often provoke controversy due to disagreement about the motives and consequences of particular military operations. Nineteenth-century apologists for British foreign policy were among the first to proclaim a general legal right to intervene for humanitarian purposes. Some twentieth-century statesmen have claimed similar rights upon occasion, frequently in an effort to justify otherwise questionable foreign military ventures. The issue has gained increased attention lately, especially in the context of numerous actual and proposed multinational military missions that are supposed to express global and regional community interest in peace and other human rights.

Humanitarian intervention and international peacekeeping are not necessarily the same. Most peacekeeping missions authorized by the United Nations or other regional organizations profess impartiality with regard to all issues other than peace; they typically restrict military operations according to the wishes of established governments within whose territories they deploy. Humanitarian intervention, in contrast, often openly opposes abusive policies of established governments and does not necessarily abide by governmental restrictions. Garrett defines humanitarian intervention as "the injection of military power—or the threat of such action—by one or more outside states into the affairs of another state that has as its purpose (or at least as one of its principal purposes) the relieving of grave human suffering" (p. 3).

According to this definition, humanitarian interventions are relatively rare. Garrett further concentrates attention upon a small number of recent events in which intervenors

explicitly claim humanitarian purposes. These include U.S. intervention in Panama in 1989; U.S. intervention in Haiti in 1994; French interventions in the Central African Republic in 1979; French intervention in Rwanda in 1994; Indian intervention in East Pakistan in 1971; Tanzanian intervention in Uganda in 1979; and Vietnamese intervention in Cambodia beginning in 1979. A few recent multilateral operations are also discussed, such as efforts to secure safe havens for Kurdish refugees in northern Iraq beginning in 1991, UNapproved food supply in Somalia in 1992, and intervention in Liberia beginning in 1990 under authority of the Economic Community of West African States. Other missions aimed merely at promoting impartial peace are conspicuously and deliberately excluded from this analysis, including foreign military operations within former Yugoslavia during the 1990s.

One strength of Garrett's work is a plausible perspective upon foreign policy decision making. Whether a state resorts to humanitarian intervention requires an opportunity in the form of a serious affront to humanity to which the state is capable of mounting response. Whether a state acts upon its opportunity may depend upon numerous and often competing situational incentives as well as various underlying factors. Self-interest and altruism do not necessarily define diametrically opposed alternatives. Weighty choices by modern governments, including decisions to intervene, seldom reflect merely a single purpose.

Normative evaluation of individual cases gains relevance by taking account of mixed motives among intervenors as well as by recognizing the frequently impure plight of victims. such verisimilitude complicates analysis, however. Garrett considers several moral standards. He particularly recommends a "principle of proportionality," which he attributes loosely to traditional just-war doctrine. The reader should be warned that this is not necessarily the same principle of proportionality as customarily claimed under modern international law to restrict other forceful actions, such as acts of anticipatory self-defense.

There is heightened interest lately in humanitarian intervention under multilateral authority. Several proposals under international discussion imagine a United Nations standby military force capable of ready action for humanitarian purposes. In February 2000, after this book was published, the European Council established the Interim Military Authority under the European Union and charged it to create a ready European intervention force. Garrett subscribes to the view that multinational intervention is generally preferable to unilateral action in most circumstances. He argues this position on both moral and practical grounds. Multilateralism arguably helps guarantee legitimate humanitarian claims against perceived or actual abuse by a single self-interested state. Similarly, collective judgment may be superior to that of a single state regarding whether a given situation warrants intervention. Multilateral humanitarianism is also said to maximize likely support, or at least likely acquiescence, among most interested states. Finally, it is asserted, multilateral intervention may be less likely to provoke and more likely to deter violent local resistance.

Garrett is ultimately sympathetic to the conception of humanitarian intervention and to its practice. He foresees increased reliance upon this instrument, particularly multilateral intervention, despite apparent misgivings within the United States.

Doing Good and Doing Well offers a useful introduction to an important current issue in international relations. It is not a rigorous treatise in international law comparable to Ian Brownlie's International Law and the Use of Force by States (1963). It does not include rigorous moral argumentation comparable to Michael Walzer's Just and Unjust Wars (1977). Also, it does not include, as it might have done, either a systematic historical survey or detailed comparative case studies. Nevertheless, Garrett provides competent and reasonable synopses of key elements of the issue, including moral evaluation, legal analysis, and historical commentary, combined within a consistent analytical framework. This book is a good place from which to begin serious evaluation of recent national and multilateral claims and proposals regarding humanitarian intervention.

Revolution and World Politics: The Rise and Fall of the Sixth Great Power. By Fred Halliday. Durham, NC: Duke University Press, 1999. 402p. \$59.95 cloth, \$19.95 paper.

Mark N. Katz, George Mason University

Many of the "great books" on revolution have been written by scholars who were primarily theorists and only secondarily (at best) area experts. These books usually begin with the exposition of a theory about revolution followed by three or more case studies that, the author claims, validate the theory. The principal criticism of this type of study is that the author has engaged in selective history, highlighting events or trends in the case studies that support his or her theory and ignoring those that do not.

Fred Halliday does not do this. He is not only a theorist of revolution but also a genuine area expert on the Persian Gulf region, and he is cognizant of the literature on other regions as well. Instead of being organized around case studies, the chapters in this book examine various themes, such as the export of revolution, revolutionary foreign policy, counter-revolution, war and revolution, and the systemic constraints on revolutionary regimes.

An extraordinary knowledge of the history of modern revolutions allows Halliday to demonstrate the shortcomings of other theories. In response, for example, to the argument by Stephen Walt (War and Revolution, 1996) about how revolution in one country can lead to international war, Halliday demonstrates that causation often occurs in the opposite direction: International war often creates favorable conditions for successful revolution. In this and other instances, Halliday appears less interested in proving existing theories wrong than in pointing out cases they do not adequately explain and providing theorization that does account for them. Although the book is not organized around case studies, the discussion is replete with examples from a plethora of cases.

What does not emerge is a simple, overarching theory about the relationship between revolution and international relations. The author's insistence on depicting the extraordinary variation in the many international dimensions of revolution prevents him from articulating one. Instead, Halliday identifies a set of problems that revolutionary regimes pose to the international system (and vice versa) and the patterns in which they are often (although not always) worked out.

For example, Halliday points out that some (but not all) new revolutionary regimes attempt to export their brand of revolution abroad—and usually fail. Some status quo powers (but not all) attempt to overthrow new revolutionary regimes; although they sometimes succeed, they usually fail. Some revolutionary regimes (but not all) attempt to separate from the international system and pursue an independent path to rapid economic development. This always fails, but whereas some revolutionary regimes have abandoned the effort relatively quickly, others pursue it doggedly for decades. In short,

there are regular patterns in the way revolutionary regimes interact with the international system, but there are also significant variations.

One shortcoming of the book is that it focuses almost exclusively on relations between revolutionary states, on the one hand, and status quo powers, on the other. A more complete account of revolution and world politics would seek to explain the highly complicated relationship patterns among revolutionary actors—such as between revolutionary regimes, on the one hand, and both revolutionary movements and other revolutionary regimes espousing the same ideology, on the other, and between revolutionary actors espousing different ideologies. These latter relations are important to study; sometimes (as with the Sino-Soviet rift) they become so hostile that they induce revolutionary regimes to seek varying degrees of alliance with the status quo powers they formerly despised. (But, as Halliday would point out, this does not always occur.)

Halliday is well known for being, especially in his early career, sympathetic toward revolutionary causes. At times this sympathy may lead some to question his scholarly detachment. In some cases, for example, Halliday does see revolution in one country leading it to war with others (as Walt argues), but he (unlike Walt) insists that the status quo powers are primarily responsible. For the most part, however, Halliday is merciless in identifying the shortcomings of revolutionary regimes, especially in chapter 10, in which he analyzes the disastrous consequences of attempting to delink from the rest of the world.

On the whole, Halliday provides a masterful overview of a very broad subject. Although he may not present a simple, elegant theory about revolution and international relations, anyone seeking to articulate one must first grapple with the challenges to such a theory posed by this book.

Our Own Backyard: The United States in Central America, 1977–1992. By William M. LeoGrande. Chapel Hill: University of North Carolina Press, 1998. 773p. \$45.00 cloth, \$24.95 paper.

William I. Robinson. New Mexico State University

Central America was in the spotlight of world attention from the 1970s into the 1990s and a major site of revolutionary challenge to international order. The appearance of guerrilla movements, the breakdown of the prevailing agro-export economic model, and the mounting civil strife in the 1970s ushered in a period of dramatic change. By the 1980s the region was engulfed in a general crisis: full-scale civil wars of revolutionary insurgency and U.S.-organized counterinsurgency, the collapse of the regional economy, and the demise of dictatorial forms of political authority. The 1979 Sandinista revolution in Nicaragua and near revolution in El Salvador thrust the isthmus onto the center stage of a rekindled Cold War. No less dramatic in the late 1980s into the 1990s were the peace and demilitarization processes under the mediation of international organizations, transitions to polyarchy, and economic restabilization under a new model of free-market capitalism.

The Central American crisis has generated an extraordinary amount of rich scholarship, and William LeoGrande makes an important contribution with his study of U.S. involvement in the region during this remarkable period. Intervention in Central America during the 1980s, as LeoGrande observes, was a centerpiece of U.S. foreign policy as it entered the critical transition period between the Vietnam War and the post—Cold War era. LeoGrande has

taken on the herculean task of chronicling, blow by blow, the unfolding of U.S. policy toward the region as it applied to Nicaragua and El Salvador. The result is a truly encyclopedic study, a meticulously documented and minutely detailed reconstruction of policy. To the best of my knowledge, the empirical chronicle is a faithful rendition of these historical events. It is also well written, almost literary in its style, and makes for engaging reading. I know of no other study from among welter of recent scholarship on the region that documents as much of the historical record on U.S. policy. The book is not a social scientific analysis; it is largely a descriptive chronicle. If that is what the reader is seeking, then this is a medullar volume and an essential—perhaps the—reference.

I do not wish these merits to get lost in a critique of substantial weaknesses, of which three stand out. First, as a chronicle, the book is bereft of a theoretical framework with which to organize the mass of description or to situate U.S. policy and inter-American relations. The narrative gets bogged down in so many trees that we lose sight of the forest. Page upon page, for example, tediously reconstruct the details of wrangling between members of Congress and the executive over the precise terms and amount of periodic budgetary appropriations for Central America. But these facts cannot speak for themselves. Any number of theoretical traditions from the social sciences could have served the purpose, such as sociology's world-system theory, with its notions of core powers and center-periphery relations; realist or constructionist theories of hegemony, interstate competition, and power dynamics from international relations; and approaches from international political economy that underscore the asymmetries driving international political and military conflict. Also, LeoGrande does not tap any theory to explain the internal dynamics of U.S. statecraft. Elite or managerial theory might have worked well, as LeoGrande is keen on drawing out institutional conflict and bureaucratic tussles within the policymaking apparatus.

In fact, the book does not really have a central argument. The closest we come to an analytical (let alone theoretical) framework is LeoGrande's claim that policy was the outcome of the schism between "hardliners" and "pragmatists," Republicans and Democrats, "reinforced by rivalries of ambition and personal animosity" (p. ix), a claim that guides the study. This may have some empirical validity and explanatory value, but events and outcomes cannot be reduced to the preferences of individual actors or decisions taken on the basis of role perception. Policymakers are not independent actors, and U.S. foreign policy cannot be explained by the specific views of individuals or by political positions taken at face value. LeoGrande's conclusion that "hardliners always prevailed . . . because they accurately reflected the emotional commitment of the president himself [Reagan]" (p. 581) is simply an untenable explanation for historical events of the magnitude of U.S. intervention in Central America in the 1980s. Beneath this surface level of analysis to which LeoGrande's study is limited lie more fundamental historical processes and structural forces that shape and situate the surface political behavior of actors and that ultimately informed U.S. policy.

The study's empiricism is related to its second major flaw. It is a politically compromised account. Political commitment in a scholarly work is not necessarily a weakness. The notion of the value-free intellectual is a myth; there is always a normative structure that underlies scholarship. But the implicit political commitment in LeoGrande's study is a liability because it prevents a more

complete understanding of what drove U.S. policy and a more accurate analysis of the divisions within the U.S. power structure that embroiled policy toward Central America. LeoGrande worked from 1982–86 on the staff of the Democratic leadership in Congress, dealing specifically with U.S. policy on Central America. If this experience made him well placed to chronicle policy it also may have prevented him from offering a perspective more distanced from the liberal Democratic interpretation of this area of U.S. foreign policy. Such a perspective might have been achieved had LeoGrande been guided by some theoretical framework.

The hardliners versus pragmatists and Republicans versus Democrats approach denies the essential unity within the foreign policy establishment over ultimate objectives in Central America. In fact, there was never really any breakdown of underlying consensus on the region. With perhaps a handful of exceptions, U.S. lawmakers from liberal Democrats to right-wing Republicans were united in the ultimate outcome they wished to see in Central America. Democrats opposed hardline Reagan policy not because they saw revolutionary change to be just or necessary, or because they viewed the United States as a historic force of oppression and domination over Central Americans. They opposed it because they saw such methods as sponsoring death squads, mining harbors, and trading arms for hostages as reckless and counterproductive to the ultimate goals of containing the revolutionary challenge, restabilizing a procapitalist, elite-based social order in the region, and reasserting U.S. control. These objectives were not in dispute. The debate centered on how to achieve them, an issue of tactics and strategy not of essence, no matter how heated that debate became. LeoGrande comes across at times as apologetic of—even embarrassed by-Democrats for their inability (unwillingness?) to oppose more fundamentally the hardline Republi-

The third flaw is that key pieces of the story are left untold. The chronicle is presented almost exclusively from the Washington perspective. LeoGrande admits as much: "The principal venue of this story is Washington, D.C." (p. x). But his explanation for this approach is not very satisfying. The view that, for good or ill, Central America's fate during this crisis depended fundamentally on decisions made in Washington denies the agency of Central Americans and reduces the dialectic of international dynamics and transnational social forces to a one-way street. Moreover, Guatemala is left out, as are Honduras and Costa Rica, the two political-military rearguards of the regional counterinsurgency/counterrevolution. One cannot expect everything from a single work, but a study of this magnitude (590 pages of text) could have substituted some of the more minute detail for a broader focus.

Perhaps the most significant part of the story entirely missing is the neoliberal restructuring of the Central American economy under Washington's tutelage. This was the economic counterpart to the U.S. political-military campaign. It facilitated the region's reintegration into global capitalism under a new model of accumulation. That reintegration weakened the old agro-export oligarchies, undermined revolutionary strategies, and strengthened modernizing technocratic groups, which contributed enormously to the political conditions for a negotiated outcome to the crisis that fit comfortably within the emergent global capitalist order.

State Interests and Public Spheres: The International Politics of Jordan's Identity. By Marc Lynch. New York: Columbia University Press, 1999. 327p. \$49.50 cloth, \$18.50 paper.

Rex Brynen, McGill University

Jordan is a relatively poor, weak country situated in a highly conflictual neighborhood. It has been very permeable to the shifting ideological winds of Arabism and has faced divisive questions of national identity and purpose (to the point of civil war in 1970–71). It also has, since the parliamentary elections of 1989, experienced both a substantial political opening and a subsequent restriction of public freedoms.

For all these reasons, Jordan is an excellent laboratory in which to assess the competing theoretical claims of realist/rationalist and constructivist approaches to the explanation of international relations. To what extent have Jordan's foreign policy interests and behavior derived from its struggle for survival in an anarchic region, shaped by patterns of regional threat, alliance, and resources? In what ways have changing regional norms and the shifting strands of Jordanian identity determined state interests? What role has been played in all this by the changing quality and nature of domestic political discourse?

In his excellent book, Marc Lynch takes on the task of evaluating competing explanations. He does so against the backdrop of four major shifts in modern Jordanian foreign policy: King Hussein's 1988 decision to disengage from the Israeli-occupied West Bank; Jordan's refusal to align with coalition forces against Iraq during the 1990-91 Gulf War; the decision, in 1994, to sign a peace treaty with Israel; and the reversal of Jordanian policy toward Iraq in 1995-96. These decisions, Lynch suggests, can only be partially, inadequately, and inconsistently explained by the dominant realist/rationalist approaches (threat balancing against external challenges; "omnibalancing" against a combination of external and domestic political factors; or rent-seeking in support of domestic neopatrimonialism). Constructivist approaches are seen by Lynch as having much to offer. These, however, ultimately fail to treat adequately the question of how and where identities, norms, and values ultimately emerge; of how precisely they undergo change; and in what ways they are linked to foreign policy behavior.

These gaps, Lynch suggests, are better dealt with from a public sphere approach, in which analytical attention is devoted not to the important role of norms and identity but also to the contexts and formats in which these values are contested, reshaped, and adopted. During the period covered by the book, both the Arab and Jordanian public spheres underwent dramatic changes. The Arabist core of the former was dealt a shattering blow by the Gulf War. The quality and importance of the domestic public sphere grew dramatically with Jordan's 1989 political liberalization, only to diminish somewhat with greater press restriction from 1994 onward. According to Lynch, "the shift in the location and efficacy of deliberation, between and within public spheres, powerfully affect[ed] Jordan's articulation of state interests and its strategic choices" (p. 70).

The bulk of the book buttresses these claims through detailed examination of the public debate (within the media, professional associations, parliament, and other fora) that preceded, accompanied, and followed each of the foreign policy decisions under examination. In each case, Lynch's examination is detailed, well written, and highly sensitive to the nuances of local politics. Indeed, although the book is

predominately aimed at debates within international relations theory, students of Jordan's domestic politics will find much of interest here.

Perhaps the weakest part of Lynch's analysis, not surprisingly, arises from an attempt to impute causal linkages between foreign policy behavior and public debate. In part, this is because the relationship often appears to be dialectical: Foreign policy decisions not only may reflect emerging consensus in the public sphere but also may be intended to shift public perceptions and attitudes (a point Lynch recognizes explicitly in the case of Jordan's 1988 disengagement from the West Bank). Moreover, in the absence of detailed process-tracing of particular decisions, it is difficult to know whether the private calculus of decision makers is accurately reflected by public discourse. Methodologically, greater use could have been made of interviews in an attempt to provide some of this. It may well be, however, that the semiclosed nature of Jordanian (especially royal) decision making and the relatively recent nature of events would have limited the effectiveness of such an approach.

Overall, this is a very valuable book. It is a major addition to the rich and growing literature on the complex interrelationships between domestic and foreign policy in the Middle East. Without overstating his contribution, Lynch also offers keen insight into the broader contributions of realist/rationalist and constructivist approaches to the study of international relations, the opportunities for theoretical synthesis, and the considerable value of an analytical division of labor between them. His articulation of the importance of the public sphere approach is clear and effective. In short, this work deserves to be widely read.

The Americas in Transition: The Contours of Regionalism. Edited by Gordon Mace and Louis Bélanger. Boulder, CO: Lynne Reinner, 1999. 195p. \$38.00.

Marjorie Woodford Bray, California State University, Los Angeles

Economic integration is the concept of regionalism emphasized by most of the authors in this book. They are largely Canadian (one is Brazilian), which flavors their perspective. In the first article, "Hemispheric Regionalism in Perspective," Mace and Bélanger define regionalism as "a multidimensional political process of integration occurring between two or more countries in a given geographical region." They do not expect it to lead usually to a new political unit but to greater cohesion and some "joint management" of common problems. They do not see it as automatic but the product of "human agency." Processes can be halted or interrupted when actors change. The interplay of actors' strategic behavior and the processes of regionalism are what the book purports to study (p. 1). The definition of actors is an inclusive one. There are chapters on the U.S. Congress, Brazil, middle states (Argentina, Mexico, and Canada), the Organization of American States (OAS), business, and social

The book begins with articles that define hemispheric regionalism and give the history and structure of inter-American relations. The definition of issues of regionalism that are important in addition to the economic dimension are security, the environment, and cultural values. The historical discussion concludes with the question of the fate of regionalism in the Americas since the Summit of the Americas held in Miami in 1994, which called for a timetable of integration into a hemispheric free trade area to be achieved by 2005. Mace notes that the "mood" of the actors has "fluctuated

wildly" since (p. 35). This point is particularly illustrated in the later chapter on the U.S. Congress by Bernard Lemelin.

The discussion of the structure of trade emphasizes, with statistical analysis, the dichotomy between the trade relations among those nations that trade largely with the United States and the cluster of the southern cone. The latter increased with the establishment of Mercosur (the common market set up by Brazil, Argentina, Uruguay, and Paraguay). Also stressed are the dominance of the United States economically in the region and the disparity in the levels of economic development and levels of income per capita. The article by Ivan Bernier and Martin Roy focuses on the difference in the structure and aims of Mercosur and the North American Free Trade Agreement (NAFTA) among the United States, Canada, and Mexico and the difficulty of combining them into Mercosur. The latter is a gradual process that "leaves room for backtracking" (p. 79), whereas the United States insisted that all elements be defined before NAFTA was

The authors of the article on the OAS (Guy Gosselin and Jean-Philippe Thérien) do not expect that organization to play a primary role in economic integration, although they believe its role in promoting democracy and human rights has increased. They cite the importance of the Canadian decision to participate in the organization and to create the Unit for the Promotion for Democracy (UPD), which by mid-1998 had observed 37 electoral processes. The authors note that the Inter-American Commission on Human Rights was allowed to observe the situation regarding human rights in Brazil in 1995 and Mexico in 1996. Also important was the OAS role with the United Nations in the peace process in Central America, especially Nicaragua, although in El Salvador and Haiti it was obliged "to yield the political leadership of peacekeeping operations to the United States" (p. 191). They conclude that because of limited resources the OAS cannot play a major role in trade and even human rights. The Miami summit was held outside the Organization of American States, which weakened its role in integration. They also point to the fact that some Latin Americans remain suspicious about linking democracy to trade liberalization, as promoted by the United States. "Simply put, the United States sees poverty as a domestic issue, whereas the OAS argues that it is a transnational problem" (p. 89).

The analysis of different business perspectives by Klaus Peter Fischer is especially interesting. He divides business interests into three groups: traditional exporters and merchants, new industrialists, and subsidiaries of multinational corporations (MNCs). He expands the second group to add a fourth category, the most successful of the new Latin American industrialists, which he calls Third World Multinationals (TWMs). He dissects these interests' differing attitudes toward regional integration. The traditional groups view integration with "benign tolerance" (p. 202). The new industrialists "traditionally adopted a stance of qualified opposition" toward integration (p. 203), but when it was inevitable, they wanted to be involved to protect their interests. The TWMs now promote integration. Fischer calls Mercosur the brainchild of Grupo Bunge and Bourne of Argentina (p. 205). He sees MNCs as allies of the traditional sector and supporters of integration. Also, he divides models of integration into three categories: secessionist-protectionist, integrationistprotectionist, and integrationist-competitive. The first two were competing with each other in the 1970s and 1980s; the third, which is represented by Mercosur, is now the "dominant" position. He concludes that although the United States has been the leader in promoting integration in Latin America since the late 1950s, the results have been alternative

models developed by Latin Americans in response to U.S. initiatives.

The chapter on social movements, "Regionalism from Below," is an extremely important and complex analysis. It cites cross-border efforts of resistance to integration, such as that which took place in relation to NAFTA. Also discussed is opposition to the Organization for Economic Co-operation and Development (OECD), the World Bank, and the International Monetary Fund (IMF) by nongovernmental organizations as an effort to create a "global civil society" (p. 224). The areas of cooperation lie in labor unions, women's groups, religious organizations, environmental interests, microbusinesses, teachers, health care workers, and others. These coalitions and networks play a double role in regional integration. They "transcend nationally bound politics," but they also attempt "to transform national and local sites of politics" (p. 235).

This book is a valuable contribution to the understanding of hemispheric integration. The only glaring lack is how the process affects smaller states, which are frequently the most poverty stricken. For example, the chapter on Mercosur makes no mention of the roles of Uruguay and Paraguay.

Deadly Transfers and the Global Playground: Transnational Security Threats in a Disorderly World. By Robert Mandel. Westport, CT: Praeger, 1999. 139p. \$55.00.

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Robert Mandel employs a descriptive and imaginative metaphor to explain the problem of covert transnational transfers. Like the typical neighborhood playground, the international system is an increasingly anarchic and dangerous environment. Due to an insufficient number of effective monitors, individuals and groups dissatisfied with the status quo can engage in illegal activities with little or no fear of punishment. The recent computer viruses that caused billions of dollars in damage and the global AIDS epidemic illustrate the devastating economic, political, and social consequences of the covert transnational transfers discussed in this book.

Mandel examines six covert transnational transfers (conventional arms, illegal immigrants, illicit drugs, hazardous materials, infectious diseases, and information disruptions). By selecting six "deadly transfers" that endanger national and international security, Mandel focuses on the strongest challenge posed to states by covert transnational flows. Each transfer threatens at least one of the three critical protection goals of every state: biological survival, political authority, and socioeconomic cohesion. In addition, the six transfers are highly interconnected. For example, individuals and groups involved in the sale of illicit drugs may also profit from the transportation of illegal immigrants. Policymakers must consider the interconnectedness of the deadly flows when attempting to develop an effective response.

Mandel identifies three different groups of actors on the global playground: unruly perpetrators, ineffective monitors, and not-always-innocent victims. He offers the reader a succinct explanation for the behavior of the perpetrators and ineffectiveness of the monitors. Frustrated with the status quo, the perpetuators resort to covert transactions to address perceived grievances. An anarchic and increasingly global system facilitates their self-interested behavior. Concerns over sovereignty limit the effectiveness of states, intergovernmental organizations, and nongovernmental organizations. The classification of multinational corporations as "largely involuntary" victims is the one questionable aspect of the classification system. They are portrayed as naive victims

stripped of "power, protection, or property" (p. 8) by the perpetuators, such as rogue states and terrorist groups. Multinational corporations involved in covert transactions, such as the illegal transfer of conventional arms and hazardous materials, are not involuntary victims.

Mandel identifies two different responses by the elite and masses to the problem of deadly transfers. One group denies that these represent a problem and adopts an optimistic perspective that is grounded in a faith in the stabilizing values found in capitalism and democracy. A second group, after acknowledging the severity of the problem, adopts a "fortress" mentality and responds to the threat with cynical renophobic self-protection behavior. Mandel ignores a third response: the attempt by states, intergovernmental organizations, and nongovernmental organizations to address the problem. For example, the Western democracies responded to the threat of nuclear proliferation by creating such regimes as the Missile Technology Control Regime and Wassenaar Arrangement on Export Controls to limit the transfer of necessary technology and materials.

Mandel offers several systemic and attitudinal explanations for the increasing frequency of covert transnational transfers. He argues that the structure of the post—Cold War international system "has been uniquely conducive to a mushrooming of these ominous transnational flows" (p. 25). More specifically, there were dramatic improvements in transportation and communication technology, a growing number of increasingly powerful nongovernmental organizations, and growing global interdependence during the first ten years after the Cold War. One can see the relationship between these trends and an increase in ominous deadly transfers. Yet, Mandel fails to explain adequately the link between the trends and the end of the Cold War. Why should the end of the Cold War have led to major scientific breakthroughs in our ability to communicate with one another?

According to Mandel, individual attitudes—such as the emphasis on freedom and individualism associated with democratization, an "escape-from-reality" syndrome, and greater emphasis on immediate material self-gratification—have also contributed to the growing frequency of transnational transfers. A society obsessed with escaping reality and pursuing immediate material self-gratification might be more susceptible to covert transfers. Yet, democratization should reduce the demand for such transfers by allowing individuals and groups dissatisfied with the status quo to address their grievances by working within the political process.

Mandel uses six short case studies to illustrate the severity of the problem. Each includes a discussion of the scope, recent history, and security implications of the transnational transfer. Given the covert nature of the transfers, Mandel does a commendable job of estimating the scope of the problem. Conventional weapon components and finished conventional weapon systems provide the best support for his argument. Weaker cases are illegal immigration and hazardous materials.

Comparing illegal migration with other covert transnational transfers, such as illicit drugs, seems inappropriate given the disparity in economic, political, and social costs between the two activities. Whereas the use of illicit drugs imposes heavy economic, political, and social costs and offers no known benefit to society, the effect of illegal immigration is much less certain. For example, countries may partially resolve a labor shortage by temporarily ignoring illegal immigration.

The inclusion of both toxic waste and military-grade uranium and plutonium in the hazardous materials case is a second problem. There are key differences between the two.

Toxic waste, which is an undesired by-product of the manufacturing process, typically provokes a "not-in-my-backyard" response, whereas potential proliferators seeking to develop nuclear weapons covet military-grade uranium and plutonium. Nuclear weapons represent a much greater threat to biological survival than toxic waste. Furthermore, the American government has not documented a single successful illegal transnational transfer of military-grade uranium or plutonium. The inclusion of a case study that considered the transfer of material and technology necessary for the production of weapons of mass destruction would have resolved this problem.

Whom should we blame for the deadly transfers? Mandel criticizes the industrialized democracies. They have implemented policies to halt the transactions that "exhibit considerable ambivalence, inconsistency, and downright hypocrisy" (p. 87). More specifically, the hypocritical response is the result of the gap between political rhetoric and public policy. The source of the gap is the tension between national security concerns and financial gain. The lack of a "uniform and universal rule-set that is consistently voiced and followed" (pp. 96–7) compounds the problem.

Mandel questions the efficacy of traditional approaches to limiting deadly transfers, such as coercive force, international law, legalization, international conferences, and reducing supply. He offers an unorthodox approach that hinges on reducing the perception gap between individual wants and societal costs, an attitudinal change that alters the incentives faced by individuals, groups, and states considering a covert transnational transaction. Potential perpetrators who understand that immediate self-gratification imposes costs on society and may even limit their individual benefits ought to be less likely to engage in illegal transactions.

In order to change attitudes, Mandel offers five policy suggestions: reduce the demand for the transfer; expose and humiliate the perpetuators; develop an integrated holistic response to all the transnational flows; facilitate public discussions about the consequences of the transfers; and encourage personal sacrifice to reduce transnational flows. These public policy options have two limitations. First, the author fails to provide real-world applications. For example, how might a government facilitate a discussion of the costs imposed on society by individuals interested in instant self-gratification? Second, Mandel overemphasizes the role played by most of society in the covert transfers. Most members of society simply do not participate in the illegal transactions described in the book.

Those interested in a thought-provoking analysis of covert transnational transfers should read this book. Mandel addresses a very complex and challenging problem in a manner that is certain to encourage discussion among scholars and policymakers.

Security, Identity, and Interests: A Sociology of International Relations. By Bill McSweeney. Cambridge: Cambridge University Press, 1999. 239p. \$54.95 cloth, \$19.95 paper.

Rodney Bruce Hall, University of Iowa

In this ambitious new book, Bill McSweeney applies a great deal of thought to questions at the heart of an ongoing project to retheorize international security studies and to metatheoretical issues at the heart of current debates in the international relations field. McSweeney powerfully critiques realist and objectivist approaches to international security studies, and he argues that security is "inextricably related to identity, and security policy to the reconstruction of collective

identity" (p. 12). But McSweeney conversely argues that, because the social reality within which security policy is formulated is socially constructed, an equal emphasis on the role of material interests is required to explain the rise and transformation of collective identity.

McSweeney maintains that two basic notions of "security" run through the history of security studies: the "common" or international security of all nations versus the "national" view that security is a property of the state. Both are convincingly critiqued. He groups the literature into four phases and analyzes each. The early "political theory" period was followed by a "political science" period, which featured the notion of "national" security. Then there was a neoliberal institutionalist "political economy" phase. The present "sociology" era is characterized by constructivist, postpositivist, and critical theoretic challenges to the objectivist, behavioralist, and the positivist theoretical commitments of earlier phases.

McSweeney is at his best when critiquing structural arguments that seek to explain behaviors "in terms of laws or causal generalizations... motivated by the need to rid the theorist of the messy complexity of human agency" (p. 38). He is also at his best in providing a very learned, thoughtful, and useful discussion of critical issues of agency and structure in social theory that articulates the reflexive sociologies of Anthony Giddens and Pierre Bourdieu and the ethnomethodology of Harold Garfinkle.

The book is not devoid of challengeable assertions, many of which stem from McSweeney's limited and tendentious reading of the constructivist literature and a similarly tendentious reading of the postpositivist literature in international relations theory. McSweeney takes on the work of Alexander Wendt on the constructivist side and David Campbell on the postpositivist side. They are exemplars, in his view, of how these bodies of scholarship fail to discover his key assertion that "[material] interests play a mutually constitutive role with identity" (p. 130). McSweeney cites few postpositivists, most notably James Der Derian, Martin Hollis, and Steve Smith.

On the constructivist side, the author cites few scholars aside from Wendt. There is a passing reference to Nicholas Onuf, and only one work of John Ruggie is mentioned briefly. McSweeney fails to cite a single, sole-authored work of the equally prolific and foundational constructivist scholarship of Friedrich Kratochwil. Throughout the book McSweeney writes as though in critiquing the work of Wendt he is thereby critiquing constructivism. He appears unaware of the diversity of theoretical perspectives in the first generation of constructivist thought, let alone the second generation. Many of the latter owe as many or more intellectual debts to Kratochwil, Onuf, and Ruggie, among others, as to Wendt, and their works explicitly or implicitly challenge the objectivism, positivism, and state-centrism with which McSweeney charges Wendt.

In short, treating the work of Wendt as representative of constructivism does not do justice to this diverse body of scholarship, which will likely limit the influence of Mc-Sweeney's argument. The author seems unaware that Wendt's Social Theory of International Politics (1999), in particular, is a subject of criticism among constructivist scholars on the ontological and/or epistemological "left" of Wendt (where McSweeney appears to reside). It is by no means yet clear that Wendt's variant will be embraced as the foundational text for the future development of constructivism. Yet, McSweeney employs his reading of Wendt to argue that "constructivism does not necessarily pose a challenge to the methodological principles and epistemological assump-

tions of the mainstream approach" (p. 203) to international relations. But Kratochwil and Ruggie, together and individually, as well as Onuf and others have challenged these principles and assumptions early and repeatedly. Significantly, Kratochwil recently criticized Wendt's book for treating social causality and material causality in analogous fashion. (Friedrich Kratochwil, "Constructing a New Orthodoxy? Wendt's 'Social Theory of International Politics' and the Constructivist Challenge," Millennium 29 [2000]: 73–101).

McSweeney's reading of Wendt is studded with challengeable assertions. He accuses Wendt's constructivism of "cultural determinism" for separating the concepts of agency and structure, placing them in opposition to one another, which fails to realize that while identities structure interests, interests conversely help to structure collective identities.

It is not clear that McSweeney has given Wendt's book a thorough read. In Wendt's chapter 5 (p. 231) it is specifically stated that actors may choose identities on the basis of interests, and the thrust of chapter 7 is consistent with this proposition. But Wendt's broader point is that any definition of interest, whether "material" or not, always logically presupposes the existence of some kind of identity. An actor cannot know what she wants until she knows who she is. Thus, when actors "choose identities on the basis of interests," they are choosing one identity on the basis of interests that are constituted by another identity that is more constitutive within a hierarchy of identities. I cannot see how a careful reading of Wendt's chapter 5 (pp. 224-33) can sustain McSweeney's assertion that Wendt treats identities and interests as rival propositions. Also, McSweeney does not offer a coherent account of "material interests" that does not presuppose some conception of identity, or make clear whether he believes we can detach material interests from identities altogether. He usually seems to assert that they may not be separated, but his treatment of the issue creates some confusion regarding his own position.

McSweeney attempts to illustrate the generation of new collective identities from material interests empirically with discussion of the identity consequences of European integration, the eastward expansion of NATO, and the 1998 Ulster Accord, but he does not explore alternative explanations in any systematic fashion. These empirical cases support McSweeney's assertions only if we accept his "seduction" model of integration, which could benefit from further theoretical as well as empirical development.

These caveats aside, I am in broad sympathy with Mc-Sweeney's call to develop a more explicitly reflexive sociological constructivism. I find much to praise in his efforts to broaden and retheorize the concept of security and in his assertion that the success of this project depends upon a reflexive understanding of the relationship between agencies and structures, as well as an appreciation of the moral choices involved in defining security. McSweeney provides an important contribution to these issues that deserves to be carefully read, with the caveat that his spare and tendentious reading of the constructivist literature leaves him with mistaken impressions of the status of the "predominantly American" (p. 203) scholarship he critiques.

Liberal Peace, Liberal War: American Politics and International Security. By John M. Owen IV. Ithaca, NY: Cornell University Press, 1997. 246p. \$45.00 cloth, \$17.95 paper.

Sean M. Lynn-Jones, Harvard University

Liberal Peace is a noteworthy and original addition to the small but growing number of books (as opposed to articles)

on the apparent absence of war between democratic states. Owen advances our theoretical understanding of how the liberal peace works and offers further empirical support for the democratic peace proposition. His central arguments are that the democratic peace is actually a liberal peace and that liberal ideas and institutions operate together to promote peace between liberal states. These arguments are supported by ten case studies of international crises involving the United States between 1794 and 1898. These crises include the 1796-98 quasi-war with France, the War of 1812, the Mexican-American War, Anglo-American crises during the U.S. Civil War, and the Spanish-American War. In each case, Owen examines whether the two states involved were liberal, whether they perceived one another as such, and how these perceptions influenced the course of the crisis. He finds that if states saw each other as liberal they avoided war and usually invoked shared liberalism as the basis for that choice.

The book makes three important contributions to the growing literature on whether and why liberal or democratic states do not war with one another. First, Owen offers a comprehensive and thoughtful explication of the logic that explains why such states are likely to remain at peace. He argues that liberal ideas matter, but they have influence on foreign policy only when strong liberal institutions exist. He thus rejects the dichotomy between normative and institutional explanations for the democratic peace and suggests that a synthesis is necessary.

Second, the book presents detailed case studies, which are essential complements to the quantitative work that has been prevalent in the literature. Owen rigorously and carefully employs qualitative research methods, engaging in processtracing to uncover the causal mechanisms that operated in each crisis. The result is not only an exemplary demonstration of the use of qualitative methods but also a convincing demonstration that liberal ideas and institutions actually cause liberal states to behave differently from other types. In many of the crises, liberals in the United States and other countries argued that one liberal state should not attack another. Some even wrote of "the peace between republican peoples" (p. 139). This pattern of behavior is also a useful reminder that the idea of a democratic peace exerted practical influence on diplomacy in the almost two centuries between its initial articulation by Kant and its rediscovery by political scientists.

Third, the book stands out among recent studies because Owen does "not support the notion that there has never been a war between liberal states" (p. 6). He also argues that perpetual peace would not emerge in a world of liberal states, because illiberal states serve as the "other" against which liberal states define themselves.

Owen's argument for the general tendency of liberal states to remain at peace with one another may be more defensible than claims that the democratic peace proposition is an "iron law," but neither proponents of the proposition nor their realist critics are likely to be satisfied with it. The former will argue that Owen has failed to identify any clear-cut cases in which liberal or democratic states fought one another. They would not regard the cases that he lists as candidates (the War of 1812, the U.S. Civil War, and the Spanish-American War) as international wars between liberal states. Realists will suggest that Owen exaggerates the strength of the liberal peace. They will point out that realist explanations account for the outcomes in some of Owen's cases. For example, Owen concludes that Chile's awareness of "superior U.S. power" (p. 158) motivated it to accept U.S. demands in the 1891-92 crisis over the treatment of the crew of the U.S.S. Baltimore in Santiago. Realists will also argue that power politics, not liberal values, can explain the results of other crises Owen analyzes (Christopher Layne, "Kant or Cant: The Myth of the Democratic Peace," *International Security* 19 [Fall 1994]: 5–49).

Despite its strengths, Liberal Peace has two important weaknesses. The first is that Owen risks undermining much of his argument by recognizing that liberal states sometimes fail to perceive one another as such. Realists will claim that this vitiates the peace proposition, because perceptions are malleable and are often influenced by levels of threat, a variable central to realism. Several of Owen's cases (e.g., Mexican perceptions of the United States in 1845-46, Chilean perceptions of the United States in 1891-92) indicate that liberals may perceive a liberal state as illiberal when it threatens the territorial integrity of their own state. To his credit, Owen recognizes that threats can determine whether a liberal state regards another as liberal. He finds that realist theories best explain some cases, including how Mexico in 1845-46 and Chile in 1891-92 perceived the United States. He argues, however, that in the majority of cases the most important influence on perceptions is whether liberals have a positive view of the domestic regime of another state: Liberals in state A will perceive state B as liberal when it adopts the domestic institutions favored by liberals in state A. Despite Owen's valiant and sophisticated attempts to address this criticism, realists are unlikely to be persuaded. They will point out that realist theories are likely to operate in the most threatening crises—precisely the type of case in which Owen admits that threats or other realist variables shape perceptions of whether states are liberal.

A second important weakness is that Owen selects only cases involving the United States, and all the crises examined occurred before 1900. It would be unreasonable to expect a single author to conduct detailed case studies of crises involving all liberal states, but the limited focus raises questions about whether the conclusions apply generally. Significantly, in the crises that Owen examines liberal ideas seem to have less influence on foreign policy in countries other than the United States (e.g., Mexico, Chile, and Spain). The book's argument would be strengthened if it were confirmed by additional systematic case studies of a variety of liberal and democratic states in the twentieth century. Owen acknowledges this criticism (pp. 62-3) but does not offer an entirely satisfactory response. He might have pointed out, for example, that many criticisms of the democratic peace proposition claim that it relies excessively on data after 1945 and that his focus on an earlier period helps answer these critiques.

Several trivial blemishes mar the book. The case studies sometimes offer only minimal discussions of the background and context. No book can be a complete history of U.S. diplomacy in the nineteenth century, but many readers will not be familiar with the details of U.S.-Mexican diplomacy before the Mexican-American War or the Virginius Affair. In the absence of sufficient explanations of the historical context, Owen's discussions of each crisis often become somewhat mechanistic enumerations of quotations.

The final chapter, "Implications and Conclusions," is another weak point. It is tempting to apply the book's analysis to contemporary world politics, but this chapter does not execute the task particularly well. The selection of the India-Pakistan and Japan-Korea dyads for analysis seems arbitrary. The discussion of how liberalism has influenced relations between these countries is much more superficial than the analysis of the cases involving the United States.

These shortcomings notwithstanding, Liberal Peace is an impressive achievement and a significant addition to the

literature. Proponents and critics of the democratic peace proposition alike will have to take into account its sophisticated and nuanced explanation. Scholars who use case study methods will find the book a useful model. Owen's work provides strong evidence that liberalism exerts significant influence on the crisis behavior of liberal states. It should stimulate further research on the influence of liberal ideas and institutions on foreign policy more generally.

Civil Wars and Foreign Powers: Outside Intervention in Intrastate Conflict. By Patrick M. Regan. Ann Arbor: University of Michigan Press, 2000. 172p. \$39.50.

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Patrick Regan focuses on two important questions: When do states intervene in civil wars, and under what conditions is this intervention likely to end the violence? The book is an admirable attempt to use serious social science analysis to respond to the concerns of policymakers, who are without much basic information in this new and challenging arena.

The analysis is based largely on logit analysis of 138 intrastate conflicts from 1944 to 1994. (The number is somewhat larger than in other studies because Regan drops the minimum number of casualties from 1,000 to 200.) In 89 of these cases there was at least one unilateral intervention; in all, there were 194 such interventions. (Multilateral interventions are evaluated separately.) He judges that only about 30% helped stop the fighting.

Regan found that states were more likely to intervene during the Cold War than afterward, when conflicts were less intense, and when there was a humanitarian crisis. Interestingly, intervention was less likely in states with many shared borders rather than few, I wonder if dropping out less powerful neighbors would have made a difference. Success, defined as stopping the fighting, was more likely when interventions were undertaken by major powers, involved a mixture of economic and military techniques, supported the ruling group, and were directed at wars with relatively few casualties. I was surprised by the conclusion that intervention in intense conflicts, when it occurs, is much more likely to be successful than in less intense ones. Perhaps governments only become involved in such conflicts when they feel conflictent of success.

In general, this analysis is very well done. Regan does a good job of explaining what he is doing, although he involuntarily illustrates the tension between social science and policymaking by making the book harder to read. He is particularly good about noting the variables omitted from analysis (pp. 24-6) and on the whole issue of selection bias (p. 144 ff.). Separate analysis of high-casualty cases is a good idea and yields interesting conclusions: "Cultural" conflicts differ from ideological ones only in these cases, and for these cases success is more likely in religious wars than ideological ones, with ethnic conflicts in the middle but leaning toward the ideological end (p. 91).

Regan has a chapter on multilateral interventions, which have become much more common in the past decade. The analysis is based on comparative case studies rather than quantitative data and is somewhat less persuasive than the primary analysis. He argues that multilateral interventions have different functions from unilateral interventions; they focus less on ending violence and more on reducing the likelihood of renewed violence after the parties have reached a settlement (what is often called peacekeeping and peace-

building). Therefore, he concludes that success is more likely when the intervention is neutral, has the consent of all parties, and involves a clear and appropriate strategy.

Of course, intervention is something of a moving target, as Regan notes. At least for the United States, unilateral intervention seems a less likely policy option today than during the Cold War. At the same time, we face the problem of what to do when one or both sides will not or cannot reach agreement while creating a situation that the international community (whatever that means) is unwilling to tolerate. Thus, we have a second generation of multilateral intervention, sometimes called peacemaking, in which military force is used to end the violence, even though this means taking sides in the conflict, before peacebuilding is applied; Bosnia, Kosovo, and Sierra Leone are current examples. Moreover, as William Zartman has pointed out, governments without interests in particular conflicts are unlikely to commit significant resources to their resolution, but such interests mean that they will not be neutral. This insight remains important even in multilateral interventions, where individual governments still have to decide whether and how to contribute. Thus, Regan's distinctions may not apply in some of the more interesting and urgent cases.

On balance, this is an important study of a major issue for both policy and theoretical reasons. Its utility is somewhat limited because the data end in 1994 and because the focus is on unilateral rather than multilateral interventions. Moreover, despite Regan's explicit desire to speak to policymakers, the book is rather heavy going for them or for undergraduates. Nonetheless, future researchers will have to take this book and its conclusions into consideration.

The Netherlands: Negotiating Sovereignty in an Interdependent World. By Thomas R. Rochon. Boulder, CO: Westview, 1999. 318p. \$65.00.

Steven B. Wolinetz, Memorial University of Newfoundland

This is a broadly gauged treatment aimed at general and specialized readers. Rochon begins with struggles against the sea and guides the reader through history, social structure, the welfare state, and foreign policy. En route, he highlights accommodation and Dutch efforts to maintain sovereignty. Rochon links the two: Forging consensus enables the Dutch to preserve autonomy.

Rochon starts with water management and the Dutch Republic. Chapters 3 and 4 examine segmentation along religious or ideological lines, its demise, and changes in voting behavior and parties. Chapters 5 and 6 treat institutions and policy processes, particularly deep-seated habits of accommodation. Early pluralism promoted tolerance. Pillarization in the late nineteenth and early twentieth century resulted in a politics of elite accommodation. Postmaterialism undermined this. The Dutch now use different rules of the game to accommodate old and new groups under "pluralist corporatism." Rochon then examines the development, enhancement, and retrenchment of the welfare state. Chapter 8 considers trade and the bases of neutrality before World War II and multilateral engagement since then. The conclusion relates consensus to Dutch success in international politics: Consensus is achieved by following rules of the game modified to cope with changing realities. Successful practices include accepting inaction when there is no solution, consulting widely, and recruiting leaders who can find solutions. Consensus allows the Dutch to adapt to their environment and resist demands from larger countries while exercising a modicum of influence in international affairs.

This is a book I was prepared to recommend and in some respects still can. It is well written. Anecdotes and literary references make it accessible. There is a wealth of information, although not all of it is accurate. On occasion, Rochon jumps to conclusions that are not always warranted or fully substantiated.

Errata include the terms of a 1954 Roman Catholic edict, the cabinet in which a key minister served, how Socialist leader Joop den Uyl rose to prominence, the proper name of an institution, and the sequencing of events. Specifically, the *Mandement* did not forbid voting for the Labour Party (p. 178) but suggested as much by banning membership in socialist unions. Lubbers was Minister of Economic Affairs under den Uyl, not van Agt (p. 181). Den Uyl did not become leader because he could placate the New Left (p. 179)—this happened later—but because of a long career in party and public positions. The formal name of the SER is the Social and Economic Council (Sociaal-Economische Raad) rather than Socio-Economische Raad; its size, until recently, was not 30 but 45 (p. 170).

These errors do not change the argument, but others do. Rochon claims that "maintenance of peaceful relations between the pillars" reflected "shifting coalitions between them" and goes on to argue: "Catholic leaders supported the Liberals for several decades in the 19th century...but as emancipation was achieved ... [they] ... forged an alliance with Protestant leaders on ... public support for denominational schools" (p. 39). This is correct only if it refers to formal legal emancipation. Social emancipation had barely begun when the first confessional cabinet assumed office in 1888. Thereafter, confessional cabinets alternated with liberal cabinets until proportional representation (PR) was introduced in 1918. The confessionals governed through the interwar period, reinforced with smaller liberal groups after 1933. There is no evidence of shifting coalitions in periods when peaceful relations among the pillars should have been most at risk.

Rochon's views of social democracy and post-World War II rapprochement are difficult to accept. The author claims that the construction of the socialist pillar was a response to confessional mobilization (p. 32). Yet, socialist efforts to delineate a pillar paralleled Catholic mobilization, and socialist organization resembled not only that of the Calvinist and Catholic pillars (p. 32) but also that of kindred parties in neighboring countries. Rapprochement between Social Democrats and the confessional parties during World War II was at least as important as between Catholics and Protestants (pp. 162-3, 194-7 ff). Social Democrats had been excluded before 1939. Cabinets dominated by Social Democrats and Catholics built the postwar welfare state. Nearly equal in strength, the two parties wrestled about its shape. Impetus toward corporatism came not only from Catholics but also from Social Democrats, who saw corporatism as a way to combine planning and democracy. Protestants became involved later in the debate. Also, postwar labor peace cannot be attributed to confessional unionism (p. 163). The largest federation, the socialist NVV (Netherlands Federation of Trade Unions), was anxious to exchange industrial action for political influence.

Misinterpretations sometimes result from the juxtaposition of facts or events. The 1937 law that allowed extension of collective agreements to entire sectors was not a device to combat unemployment (p. 195) but to prevent employers from undercutting one another. Trade unions were not weakened by declining membership after the 1950s (p. 169); the decline did not occur until the 1980s. The Foundation Labour has not lost influence (pp. 169–70);

renewed social partnership in the 1980s and especially the 1990s restored its prominence. The Social and Economic Council also remains important. Just because governments are no longer required to consult it (pp. 171–2) does not mean that they do not.

Rochon argues that the Netherlands is a consensus democracy, characterized by extensive accommodation, but he says little about what is being accommodated. Following Lijphart, Rochon assumes that pillarization could have brought the Netherlands to the brink of war (pp. 3-4, 37). Yet, he presents no evidence that mobilization by leaders of competing religions, demanding separate schools, moral rectitude, and, in the case of Calvinists, Sunday rest, was likely to do so. Also, there is no reference to the subsequent debate on pillarization. Similar problems exist with assertions about changing rules of the game. These are mentioned, but little is said about how they operate.

Characterizing the Netherlands as an instance of "pluralistic corporatism" is questionable. The last thing corporatism needs is another modifier, let alone one implying its opposite. Rochon uses corporatism to mean giving groups a right to be heard. He argues that more and more groups enjoy rights to be consulted via advisory boards. This is true despite occasional efforts to eliminate them. The system is more fragmented than before, but its diversity is better captured by the notion of separate policy networks, common in the comparative literature.

One of Rochon's aims was to demonstrate Dutch pragmatism and flexibility. This he does, but perhaps not as successfully as he intended.

Cuba: Confronting the U.S. Embargo. By Peter Schwab. New York: St. Martin's Griffin, 1999. 226p. \$29.95 paper.

William M. LeoGrande, American University

Peter Schwab is outraged by the harm done to the living standards of ordinary Cubans by the U.S. economic embargo, which he calls "a brutal and vicious campaign" intended to starve them into submission (pp. 55, 101). He often lets his justifiable anger get in the way of his analysis, which leads to exaggeration and, too frequently, factual missteps. This book examines the terrible economic crisis that Cuba suffered during the 1990s after the collapse of the Soviet Union, especially the severe shortages of food and medical supplies, and the political consequences of this economic crisis.

Schwab's approach exemplifies the danger of focusing too intently on one aspect of social reality. He attributes all of Cuba's problems to the U.S. embargo. The general macroeconomic crisis of the 1990s, the decay of the health care system, the shortages of food, gasoline, and soap—literally every economic difficulty Cuba has faced in the past decade is ascribed to U.S. policy. Washington has kept Cuba "in a state of economic contraction and depression since at least 1991" (p. 168). Schwab even blames Castro's reconciliation with the Catholic Church—his "capitulation to Catholicism" (p. 119)—on the embargo, implausible as that sounds.

There is no doubt that U.S. economic sanctions have hobbled the Cuban economy, especially over the past decade, as Washington has slowed Cuba's reintegration into world trade and capital markets. In the area of health care, the effect has been most dramatic because so many drugs carry U.S. patents, and their export to Cuba is restricted. But no analyst of the Cuban economy, whether based in the United States, Europe, or Cuba itself, thinks that the main cause of the economic crisis has been the U.S. embargo.

The proximate cause was the collapse of the Soviet Union,

which deprived Cuba of about \$4 billion in annual economic assistance. Without that aid, the capacity to import shrank by about 75%. The economy went into a tailspin, losing 35–50% of GDP between 1989 and 1993 (but not 60% in a single year, as Schwab asserts on p. 71). To keep domestic production going as best it could, Cuba used scarce foreign exchange to import fuel and raw materials. There simply was no money for consumer goods, even such basics as food and medicine. The Cubans were not prevented from buying necessities by the U.S. embargo; Europe would have sold them most of these goods if they could have paid for them (albeit at higher cost).

The structural cause of the crusis lies in Cuba's failure to diversify the economy away from sugar because of its role in the "socialist division of labor." When the socialist bloc disappeared, Cuba was left with an inefficient industry producing for a world sugar market characterized by a long-term decline in prices. The solution has been a major shift into tourism, which now surpasses sugar as producer of hard currency.

The U.S. embargo has certainly impeded Cuba's ability to adjust. The U.S. travel ban limits the potential of the Cuban tourist industry; U.S. trade prohibitions raise the shipping costs to Cuba of buying and selling bulk commodities; and U.S. threats of litigation have probably limited the flow of foreign direct investment to the island. But the embargo is a hindrance, not the cause of Cuba's problems. By blaming the embargo for everything, Schwab misses the most interesting issue facing contemporary Cuba. Can a small country that is dependent on the international market (and therefore must organize its economy "efficiently" by market standards) maintain the sort of social and economic egalitarianism that has been a hallmark of the Cuban revolution?

Schwab clearly admires the Cuban government's efforts to provide for basic social and economic needs, and he agrees with the notion that these are fundamental human rights at least on a par with political rights and liberties. He takes a less defensible step, however, when he counterposes socioeconomic rights to political ones, arguing that there is a necessary tradeoff between them.

Elsewhere, Schwab has written critically of "Western" notions of human rights that give priority to political liberty at the expense of socioeconomic conditions. Here, he argues that the Universal Declaration of Human Rights is essentially an expression of Western cultural chauvinism "imposed" on the Third World (p. 6). He quotes approvingly the Cuban government's argument that the exercise of individual rights must be subordinated to the common good and can be legitimately restricted when "their use would lead to the erosion of the goals of the revolution" (p. 8). Left unanswered is the question of who defines the common good and by what authority. The goals of the revolution have gone through many twists and turns in the forty years since Castro's barbudos (bearded ones) rode triumphantly into Havana. How long can a government continue to claim a mandate to rule based upon a political uprising that occurred before most Cubans alive today were born?

Even more troubling is Schwab's insistence that socioeconomic rights and political rights are inherently in conflict. "If economic and social rights are primary within the context of what is good for the group as a whole... then individual political and civil rights, which conflict with the paramountey of the larger group, must be denied" (p. 113). He makes no analytic case for this, other than to insist that since Western notions of political liberty emerged from the development of capitalist society, they had to be abandoned when Cuba rejected capitalism (pp. 59-60). By this facile logic, socialist

democracy is an oxymoron; socialism requires authoritarianism. If we have learned anything from the failed socialist experiments of the twentieth century, we ought to have learned that without real democratic accountability, governments—even those that begin with the best intentions—end up serving their own interests rather than the public's.

Descending from the theoretical to the practical, Schwab argues that the Cuban government's intolerance of dissent is due largely to a siege mentality instilled by forty years of conflict with the United States. Castro sees in every dissident a plot by Washington to overthrow the revolution. Governments seeking an excuse to repress their opponents always look for a foreign enemy to blame, but never has a government had such a perfect foil as Washington provides for Fidel Castro. Over the years, the United States has invaded Cuba, conducted paramilitary sabotage, tried to assassinate Castro, and, of course, maintained an economic embargo with the explicit purpose of bringing down the government. Cuban dissidents themselves report that when U.S.-Cuban relations relax a bit, they enjoy more political space, and when relations sour, they suffer.

Schwab acknowledges that not every Cuban dissident is an agent of the CIA, and he argues that the Cuban government ought to allow greater political freedom, a recommendation that reveals he is not entirely convinced by his own argument that socialism and democracy are incompatible. Still, the chapter entitled "Political Dissent" focuses more on the machinations of Washington and the Cuban exile community than it does on dissidents in Cuba. It demonstrates why Castro feels threatened, but it does not explore the current intellectual ferment in Cuba as its well-educated population grapples with how to modify the socialist model in order to survive in a capitalist world.

Schwab concludes with speculations about Cuban politics after Fidel, which are entertaining but, like all such ruminations, should not be taken too seriously. What happens after Fidel depends on how and when he departs, neither of which is predictable. Schwab expects a smooth transition to a government that will remain socialist. "Fidel Castro's political visage will always be retained and will morally disallow the United States from attaining the dominant and oppressive role it held... prior to the revolution" (p. 186). Whether this wishful thinking will hold up despite the power of international markets remains to be seen.

Ironically, as Cuba rejoins the global system, the embargo is the one thing that prevents the mammoth U.S. economy from engulfing Cuba, as it did before 1959. Without the embargo, trade with the United States would quickly dwarf trade with every other partner, investment from U.S. firms (including Cuban-American firms) would dwarf investments from elsewhere, and tourists from the United States would dwarf the numbers from Canada and Western Europe. Today, the principal threat to national sovereignty and social welfare is not the U.S. embargo but Cuba's subordination to the rigors of international markets; not the political whims of Washington, but the impersonal demands of global capitalism.

Violent Cartographies: Mapping Cultures of War. By Michael J. Shapiro. Minneapolis: University of Minnesota Press, 1997. 241p. \$49.95 cloth, \$19.95 paper.

Ronen Palan, University of Sussex

The title of this book does not prepare the reader for the scope and ambition of the project. Far from being a rehashed version of the familiar if fashionable concern with conceptual, analytical, and discursive boundaries, it lays the groundwork for an alternative, ethnographic approach to security studies.

Shapiro's premise is that there can be two approaches to security studies. The conventional one takes its cue from Clausewitz and views the use of violence as a purposeful instrument of state policy. This approach has largely lodged within what is called a realist theory of power and state action. Shapiro advances an alternative ethnographic or "ontological" approach, however, which seeks "to attenuate identity commitments by reflecting on the boundary practices and history-making narratives by which they are shaped" (p. 138).

Drawing on Hegelian-Freudian-Lacanian theories of subjectivity, Shapiro argues that violence and the use of force are instruments of identity construction. According to these theories, self and identity, whether in the construction of the ego or the construction of the superego of the social collectivity, are an interactive social process. Interestingly, and correctly in my view, Shapiro does not identify with the fashionable symbolic interactionist theories of the self; rather, he delves deeper into the complex but rewarding world of Lacanian theory and is able to advance a dynamic theory of encounter.

The process is violent, or can be, to the extent that the construction of the other as a threat is always a construction of a binary opposition between two equally constructed "selfs." The violence that accompanies a particular representation of the other, therefore, has meanings that go much deeper than security studies acknowledges. Violence is lodged within an expression of an economy of representation. The cartography of modern violence tells us something very important about the broader relationships between the use of violence and collective and individual identities. It renders political economy as a cartographic pursuit: defining social bodies and boundaries and assigning meaning to these bodies as enemies, foes, and friends.

In that sense, security studies is not a separate field of investigation. On the contrary, Shapiro sees an important link between the naturalizing perception of territory that is presented as given in conventional discourse, "the commercial instinct," and the specific modes of modern violence. In the final analysis, a holistic political economy emerges that brings together theories of capitalism, modes of war, and collective identities. This theoretical framework allows Shapiro to narrate a history of modern violence in a novel way. The manner and technologies of constructing self and other (and the other way around) undergo change. The digitalization or derealization of the enemy, mediated through modern technologies, has wider and deeper effects on the construction of national or collective identities. There is no implicit or explicit causality. Shapiro does not argue that the digitalization of war causes the dispersion of modern identities, but within that it is part and parcel of it.

All this is good, interesting, and certainly worth dwelling upon, but it is not entirely novel. Shapiro's true contribution perhaps lies elsewhere. Rather than insist on the gap dividing the two traditions of security studies, Shapiro shows convincingly that the ontological tradition has always existed, underpinned and informed although unrecognized by the first. This is a shrewd move. To begin with, it allows Shapiro to advance a "proper reading" of Clausewitz as an important contributor to the ethnographic tradition. For Clausewitz, war is an instrument of state policy, no doubt, but it is viewed as an instrument of the state, itself attributed the trappings of a personality and individuality. So war and violence are forms of engagement in which the collective self defines its histor-

ical purpose. Hegel was very explicit about this aspect of war making, but by insisting on the pervasive nature of the ethnographic theme, Shapiro is able to subvert not only the founding fathers of strategic security studies but also, and more important, their contemporary sons and daughters. Under close scrutiny, the role played by, say, General Schwartzkopf during and after the Gulf War and analysis of some "conventional" if enthusiastic supporters of U.S. policy reveal clearly ethnographic concerns with symbolic action and collective identity. In Shapiro's skillful hands, the very thing that conventional security studies tries to forget comes back to haunt it.

Yet, the book fails in its final and most ambitious if unstated aim. All maps are constructed, attaching meaning to places and boundaries in a particular manner. Conventional geopolitical maps are replaced by a different cartography that maps out a constructed violent space. But why these particular violent cartographies of the late twentieth century? An outline is sketched out here, a few notes are added, but ultimately the picture remains fuzzy and uncertain.

Some readers may take exception to the style and structure of this book. In the current climate of "scientific" approaches to political science, Shapiro's collage of readings, his discursive if accessible style, and his eclectic yet rigorous use of diverse literature do not intimate to the reader either the true purpose of his project or the limitation of his undertaking. But then the discursive style gets the better of him, and the book is rather repetitive toward the end. Bearing in mind these shortcomings, Violent Cartographies is a fascinating, beautifully written, and unquestionably important contribution.

Mortal Friends, Best Enemies: German-Russian Cooperation after the Cold War. By Celeste A. Wallander. Ithaca, NY: Cornell University Press, 1999. 229p. \$49.95 cloth, \$19.95 paper.

Jeffrey T. Checkel, ARENA/Universitetet i Oslo

Celeste Wallander has written an excellent book, one that will appeal to Europeanists, post-Soviet scholars, and especially theorists of international relations (IR). The empirical core of the volume is a careful reconstruction of German-Russian security relations in the immediate post—Cold War period (1991–96). Wallander's data include Russian and German primary source material, patterns of actual behavior, and, perhaps most important, more than 140 interviews with policymakers, analysts, and academics in the two countries. Theoretically, she develops an institutional argument that goes beyond saying "institutions mattered." Instead, Wallander theorizes (and then empirically documents) the specific ways and conditions under which international institutions did—and did not—affect Russian-German relations.

The theoretical argument is developed in the first two chapters, through an approach clearly within the neoliberal institutionalist tradition of Keohane and others. These scholars argue that international institutions provide information and reduce uncertainty; at the state level, they constrain the strategies of self-interested actors. Wallander's important contribution is to add nuance and specificity to this spare theoretical frame. In particular, her "stronger institutional theory of security relations" (p. 28) explores how variation in institutional form and function and the "layering" of international institutions affect the likelihood of states choosing cooperative security strategies (pp. 27–40). The book's rigorous design then tests these institutionalist, as well as

alternative realist, hypotheses over a number of different cases in Russian-German relations of the 1990s.

After documenting that German and Russian policymakers shared some common interests and recognized that the use of force would be costly (the preconditions for the author's theory to be operative), Wallander explores the role of international institutions in shaping relations between these two great European powers in several areas. These include the withdrawal of Russian troops from unified Germany, multilateral conventional force reductions (the Conventional Forces in Europe, or CFE, Treaty), "out-of-area" missions for German forces, regional conflicts (Yugoslavia and within the former Soviet Union), weapons proliferation, reactor safety, and Russian economic reform (chaps. 4-7). The author's extensive interview material gives these cases a sense of "history in the making" that is so often missing from institutionalist accounts, which focus on the reduction of transaction costs and the like.

More important, the analysis is not of the either/or type, which gives all the causal weight either to power (realism) or institutions (institutionalism). Instead, Wallander offers a careful both/and argument, in which power and interests are refracted through and, in some cases, shaped by institutions. In this regard, the book is a model of social science research, exploring both instances in which institutions are causally important (Russian troop withdrawals from German territory, implementation of the CFE treaty) and cases in which their influence was marginal (conflict resolution and prevention in the former Yugoslavia) or nonexistent (nuclear reactor safety). Wallander thus uses the analysis to elaborate scope conditions for when and how international institutions matter (see, especially, chap. 8). This style of nuanced argumentation and empirically informed competitive hypothesis testing marks a welcome relief from the often vacuous and abstract tone of much of the neoliberal, neorealist debate.

Somewhat paradoxically, the book's rigid adherence to a positivist framework is perhaps also its greatest limitation. When Wallander uncovers evidence that does not fit within the neat causal arrows of the institutions state strategy relation, it is either set aside or left underused. Consider two examples. First, the author provides ample documentation that international institutions have not just influenced German strategies but, at a much deeper level, helped construct its very interests and preferences. Over and over, Wallander's interviews with high-level German policymakers and analysts show them to be almost reflexively institutionalist. They find it exceedingly hard to conceive or define German interests outside the dense institutional network within which their country is embedded. Unfortunately, except for a brief mention in the concluding chapter (pp. 204-6), the author fails to exploit fully such findings. This is a pity, for it could contribute to debates among Europeanists on the so-called Europeanization and internationalization of German national identity over the past 50 years.

Second, Wallander portrays European and other international institutions as passive actors, as a resource to be exploited by self-interested states. They have no sense of agency in their own right. Throughout the book, however, institutions often play a very active and social role, serving as forums for political dialogue (p. 106), as settings in which learning (pp. 129–30) and education (p. 190) occur, or as arenas in which states are socialized into the ways of the international community (pp. 161–3). Put differently, the author has uncovered evidence of institutions' constitutive power, a subject of heated debated among constructivists and other IR theorists. To probe these constitutive dynamics,

however, she would need to relax her epistemological stance, whose strict causal underpinnings force her to hold something constant—state interests here. This blinds Wallander to the possibility that institutions may also transform those very interests.

Yet, to make such criticisms is further testimony to the quality of this book. At the end of the day, Wallander's approach is problem and not method driven: She honestly reports results even if they do not fit neatly within her epistemological and theoretical frameworks. Indeed, both my criticisms and the author's own comments in the concluding chapter (pp. 204–11) suggest that the way ahead for institutional research on international relations is to escape the narrow confines of the "neo-neo" debate. The issue is no longer whether institutions matter (we know they do) but how they have effects. Do they constrain state strategies or constitute core state properties? Wallander's book should be a model for both rationalists and constructivists as they debate these issues in the years ahead.

Social Theory of International Politics. By Alexander Wendt. Cambridge: Cambridge University Press, 1999. 429p. \$59.95 cloth, \$24.95 paper.

David Dessler, College of William and Mary

Alexander Wendt offers a sustained philosophical reflection on the study of world politics. By "social theory" he means not substantive theory but philosophy of social science (p. 5). His subject is "the ontology of international life" (p. 370), and his main purpose is to open a space for constructivism in the study of international relations (IR). Constructivism's foils are materialism and individualism. Whereas materialists believe that "material forces as such—what might be called brute' material forces—drive social forms," constructivists claim that "material forces are secondary, significant insofar as they are constituted with particular meanings for actors" (p. 24). Individualists (most prominently, rational choice theorists) believe that social actors preexist social structures, but constructivists argue that structures constitute agents (p. 26).

Wendt assigns to constructivism the task of building "constitutive theory" and explaining "constitutive effects" in IR. Constitutive theory differs from causal theory, he argues. The latter asks "why" and shows how "a temporally prior X produced an independently existing Y." Constitutive theory asks "what" and reveals "the structures that constitute X or Y in the first place" (p. 83). "The distinctively constructivist hypotheses about the role of ideas and social structure in world politics are primarily about [their] constitutive effects" (p. 78).

Wendt brings to Social Theory of International Politics an immense learning, an instinct for asking big questions, and a careful eye for the nuance of important debates. He advances arguments across several levels of analysis, ranging from the philosophy of science as a whole to the philosophy of social science to the philosophy of international relations theory, and he draws on many disciplines, including political science, social psychology, sociology, and philosophy. Wendt's writing is sometimes difficult but invariably elegant. This is a very impressive work.

Three problems in the book concern the analysis of theory and explanation. First, Wendt classifies some IR theories, including neorealism, as materialist. (This classification is important because he relies on it to argue that constructivist IR is distinctive in not being materialist.) "What makes a theory materialist" is that it explains phenomena "by refer-

ence to 'brute' material forces—things which exist and have certain powers independent of ideas" (p. 94). Neorealism counts as a materialist theory because it identifies regularities linking the international distribution of power to state behavior (pp. 97, 102–3). Wendt believes that neorealists thus attribute to matter powers that no mere physical objects can have. "Neorealism 'fetishizes' material capabilities in the sense that it imbues them with meanings and powers that 'can only be correctly attributed to human beings'" (p. 109).

This argument falls to distinguish laws from theory. Neorealism's laws (statements of regularity, e.g., "bipolar systems are stable") might be termed "materialist" in the sense that they suggest we can predict aspects of state behavior by counting up material things. But neorealist theory (the framework that explains these laws) holds that the distribution of power makes state behavior predictable only because decision makers grasp it as meaningful in predictable ways. "The theory is built up from the assumed motivations of states," writes Kenneth Waltz (Theory of International Politics, 1979, p. 118). It might be true that neorealism does not give an adequate account of these motivations, which is a recurring theme in constructivist scholarship, but it does acknowledge their explanatory force. Neorealists do not "fetishize," or attribute human capacities to, material capabilities. "Structures do not act as agents and agencies do," explains Waltz (Theory, p. 74). Neorealists agree that "it is only because of their interaction with ideas that material forces have the effects that they do" (p. 112), a belief that Wendt identifies as the essential commitment of nonmaterialist IR theory. Perhaps some will want to argue, despite these points, that constructivism is still somehow less materialist than neorealism. If so, they will need a more discriminating analysis of the explanatory structure of these theories than Wendt provides.

Second, Wendt encourages IR scholars to pursue a new type of theory and explanation, which he labels "constitutive" (pp. 77-88, 165-77). To clarify it he develops the example "water is H_2O " (pp. 25, 83). This statement identifies H_2O as the molecular structure of water. It is used to explain why water behaves as it does (freezing below 32° F, condensing on a cold surface, and so on). Wendt calls it a "constitutive claim," which suggests that we interpret it to mean water is constituted by H2O. This manner of speaking lands us in a conceptual muddle, however, when it leads to talk of "constitutive effects" and "constitutive explanation." Wendt suggests that water's behavior is explained causally, whereas water is explained constitutively. But the claim "water is H₂O" merely tells us that the behavior of water is the behavior of H₂O; it does not explain water in any additional sense. To explain why water behaves as it does under various conditions, we point to the structure of the H₂O molecule (its two covalent bonds, the fact that it is bent at an angle of 105°, and so on). This is what theory is all about. It would be confusing, however, to go on to say that H₂O "constitutively explains" water or that water is a "constitutive effect" of H2O. Water just is H₂O.

Similarly, to believe that the Cold War consisted of the ideas Soviet and American policymakers had about superpower relations—"in an important sense those ideas were the Cold War" (p. 375)—is merely to hold that the trajectory of the Cold War, with all its twists and turns and ebbs and flows, just was the course of thinking about it. It would not make sense to conclude that the Cold War was a constitutive "effect" of those ideas, or that those ideas constitutively "explain" the Cold War. Specifying what something is does not explain it.

This has important implications for constructivism in IR. Wendt argues at length that the explanatory forms of the natural sciences can be transferred to the study of society (pp. 47-91). The problem is that he misconstrues the natural sciences. There is no such thing as constitutive explanation in science. Constructivists might very well be right that ideas are more important in the world than traditional IR theories have suggested, but to show this, they will have to rely on conventional categories of scientific theory and explanation.

Third, Wendt suggests that IR scholars can productively ignore their epistemological differences. He treats epistemology as an unfortunate distraction and argues that researchers should just get on with their empirical work (pp. 47–8, 90–1, 373). "Epistemological issues are relatively uninteresting" (p. 90). Wendt envisions a pluralist research community in which all can go about their business: "empiricists looking for behavioral laws, rationalists building deductive theories, process tracers doing case studies, critical theorists thinking about deep structures, postmoderns doing constitutive theory" (p. 91). The only epistemological arguments he rejects in the field are those that reject other epistemologies (pp. 47–8, 90–1). "The point is that everyone gets to do what they do" (p. 91).

It is difficult to see how this can work. If some scholars believe social scientists should be developing causal theory, and others argue that causality is an inappropriate category for grasping social relations, if some hold that the principles of statistical inference must be followed to justify causal inference, and others argue that the rules of inference are not reducible to statistics, and if some believe that explanation requires covering laws, and others maintain it does not, and so on, we cannot just endorse these differences. Epistemological pluralism is self-refuting. None of these arguments about theory and method may be wrong, but none is right either.

Wendt does not analyze epistemology in detail, and it would be unfair to criticize him for not including even more discussion in an already long and complex work. Yet, epistemology is something his research program must eventually confront. Calls for IR scholars to resist epistemological debate—"the point is to explain the world, not to argue over how we can know it" (p. 373)—only serve to remind us that, in the end, it cannot be avoided.

APSR EXTERNAL REVIEWERS, 1999-2000

The peer review process on which scientific publication relies depends on the intellectual contributions and generosity of those scholars who give of their time to read and evaluate the work of their peers. The Editor and the Editorial Board thank the following scholars, who served as manuscript reviewers from August 15, 1999, through August 14, 2000.

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